SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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FRIDAY, JANUARY 4, 1907

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FACT AND THEORY IN SPECTROSCOPY[1]

BEFORE passing to some present problems in physics, let us pause a moment to consider the losses which our science has sustained since the last annual meeting.

The life and work of Professor Langley, who died on the twenty-seventh of February last, will long continue to form an important chapter in the history of astrophysics. To the study of this science he brought rare skill, perseverance and clearness of purpose. Among his most important contributions is to be mentioned his epoch-making determination of the distribution of energy through the spectra of various sources, including especially the sun, moon and firefly. His measurement of the lifting power of an aeroplane driven at a definite angle with a definite speed, his exquisite discussion of the 'Internal Work of the Wind' with its accompanying explanation of soaring and his still later achievement of actual flight are matters which have perhaps only recently received fair appreciation. The recent performance of the Wright brothers in Ohio and the flight of Santos Dumont in a 'manned' machine are but two events in the logical series which Professor Langley did much to initiate.

On the nineteenth of April, 1906, occurred a great tragedy. Nothing in the behavior of that remarkable element which

[1] Vice-presidential address before Section B of the American Association for the Advancement of Science, New York, December 28, 1906.

he helped to discover can be considered more unexpected or more inexplicable than the death of Professor Pierre Curie.

The manner in which the clew given by uranium rays was taken up and followed out with persistent endeavor, clearness of vision, simplicity of life and modesty of character, has been rarely paralleled in the history of physics.

Our admiration for his scholarship and for his generous chivalric nature is united with a keen sense of loss to science and a warm sympathy for his brilliant and loyal comrade.

On the fifth of July, last, Germany and the rest of the world suffered a deplorable and inexplicable loss in the death of Professor Paul Drude. Brilliant and numerous as his achievements were, it is difficult to believe that his work was more than half done. Perhaps no better illustration of his genius can be found than in the beautiful manner in which he has quantitatively connected the subjects of thermal and electric conductions, on the basis of the electron theory; while his two splendid volumes have rendered all students of physics his debtors.

The death of Boltzmann, two months later, was an equally great mystery. The most valuable work of this remarkable and somewhat bizarre character undoubtedly lies in the field of the kinetic theory of gases.

His treatise on this subject constitutes for me—I confess it freely, but sadly—a sealed volume. Were I to attempt to convey to you any idea of its importance I should feel, only in a much truer sense, what Boltzmann himself expresses in the preface of his wonderfully lucid exposition of Maxwell's electromagnetic theory, when he quotes from 'Faust.'

> "So soll ich denn mit saurem Schweiss,
> Euch lehren was ich selbst nicht weiss."

Boltzmann's two visits to America, one in 1898 and one in 1904, were full of interest for many of the members of this section. Suffice it to say that some of his colleagues have expressed the opinion that since the death of Helmholtz, Boltzmann has been the leading physicist of Germany.

I beg now to call your attention to a matter which seems to me somewhat intimately connected with spectroscopic progress. And one can perhaps do this most simply by first offering a definition of spectroscopy; secondly, stating what may be considered the fundamental facts of the science; and, thirdly, considering to what extent these facts—for one hesitates even yet to call them 'principles'—are explained by any general theory of the subject.

Any explanation of light which is general and satisfactory may be said to include at least two chapters, namely, one which shall explain the transmission of radiation and another which shall treat of the origin or production of radiation. The first chapter treats of the electromagnetic ether; the second treats of matter which is at once 'the source and recipient of radiation.'

It was in the autumn of 1888 that the experiments of Hertz in a certain sense closed the chapter on the transmission of light, a large part of which had been written by Maxwell in 1864. Since then, the second portion of the theory—that dealing with the radiant atom—has assumed larger importance. Any treatment of the production of radiation falls more or less naturally into three parts, namely, (1) the radiation of solid and liquid bodies which is almost, but not quite, independent of atomic structure; (2) the radiation which takes its rise in radioactive substances and which is apparently dependent upon atomic collapse; and (3) the radiation of gaseous substances, dependent almost entirely upon normal atomic structure, and possibly also upon the mode of excitation.

The subject to which your consideration is invited has to deal only with radiation of this third class. Radiation which in terms of the electron theory is said to be due not to abrupt or discontinuous acceleration but to periodic acceleration.

DEFINITION OF SPECTROSCOPY

The science of spectrum analysis which was born, one might almost say, in Bunsen's laboratory in 1859, developed within a period of four or five years into a science of a totally different character, a science which enabled Kirchhoff at once to study the physical structure as well as the chemical constitution of the sun, a science which permitted Huggins as early as 1864 to distinguish between star clusters and nebulæ, a science much wider than spectrum analysis, a branch of learning which we now call spectroscopy. Briefly defined, it is that science which has for its object the general description of radiation, including the production of radiation, the analysis of radiation, the registration of radiation and the measurement of radiation.

The theory of separating, recording and comparing radiation is by no means simple or complete. That these last three operations demand in practise the highest degree of skill is exemplified by the work of Rayleigh, Rowland, Michelson, Perot and Fabry, and Hale.

There is, however, a certain very true sense in which these last three processes are merely preparatory to a more profound study of the first, namely, the production of radiation. From this point of view, spectroscopy hinges upon the radiant atom —if there be an atom—and may be defined imperfectly and narrowly perhaps as *the science of the radiant atom.*

More than one brilliant and partially successful attempt has been made within the last quarter century to establish an adequate foundation for this science by devising what may be called a satisfactory atom. But before considering any of these attempts it may be well to state briefly what seem to be the criteria by which any such foundation is to be judged.

Perhaps it may be fair to consider that atom as most competent which will explain satisfactorily the largest number of the following nine facts.

CRITERIA

1. The fact that spectral lines are in general approximately sharp.

2. The fact that spectral lines are never perfectly sharp; but always have a finite physical width.

3. The fact that certain spectral lines are arranged in series and bands after the manner described so perfectly by Balmer's equation and its generalized forms.

4. The fact that increase of pressure causes a shift of spectral lines toward the red as discovered by Humphreys and Mohler.

5. The fact that a magnetic field will transform single lines into multiple polarized lines as discovered by Zeeman.

6. We come now to a group of phenomena which are not easily described under a single caption. I refer to phenomena such as those observed by Plücker and Hittorf, when they found one and the same gas in one and the same tube yielding very different spectra according to the mode in which the electric discharge was applied to make the gas luminous. In the same category doubtless belongs the extinction of air lines by the insertion of self-induction into the discharge circuit. Here may belong also the fact studied by Lenard and others that the region near the electrode of an arc gives a spectrum different from the region near the center of the arc; the fact also that the so-called 'spark lines' are introduced into an arc by reducing the current to small values, a fact first studied by Hartmann.

And certainly in this same category belongs the fact that the spectrum of an arc is modified when the arc is surrounded by an atmosphere different from ordinary air.

Here also lie the profound differences between arc and spark spectra of the same element.

Notwithstanding the fact that 'multiple spectra' is a term which has hitherto been employed to describe the Plücker tube variations, I propose that we generalize it and use it to describe this entire group of facts. Since the name is so appropriate, let us call the sixth fundamental phenomenon that of 'multiple spectra.'

7. Any competent atom must allow us to infer the relations which have been proved to exist between spectral phenomena and atomic weights.

8. The phenomena of line reversals and absorption bands.

9. The fact that heat alone, at least within the range of our highest artificial temperatures, produces characteristic spectra in only a few rare instances.

These, briefly, are the parts of the spectroscopic superstructure for which a foundation is sought. These are the various parts which it is hoped will, some day, be cemented together, by a simple and general theory, into a harmonious structure.

But there is a final criterion, even more fundamental than any of those which have been mentioned, that such a theory must satisfy, namely, this hypothetical radiant atom must not in its behavior, except as a very last resort, contradict any of the established principles of physical science, be they mechanical, electrical or chemical.

The principle of the conservation of energy must be satisfied even if it is necessary to assign an undreamed of amount of energy to each atom; in like manner Newton's third law is to be satisfied, even if the electromagnetic ether is called upon to furnish the reaction.

But even with this added criterion, the preceding list of nine phenomena is confessedly incomplete; the only object of such a catalogue is to include those typical fundamental facts which ought, apparently, to follow as immediate consequences from the structure of the radiating body, so soon as that structure is correctly guessed. Thus Doppler's principle is omitted on the ground of its being rather a kinematic law, governing periodic disturbances in any medium, than a dynamical fact to be explained in terms of atomic structure and forces.

THE SATURNIAN ATOM

Having established a set of criteria by which we may estimate the fitness of a radiant atom, it would be interesting, if I were competent, and if time permitted, to pass in review some of the various atoms which have been proposed in recent times; such as that of Kelvin, 1884, or those suggested by the Hertzian oscillator.

But neither of these two conditions is fulfilled, and I propose, therefore, to consider only one atom, namely, the one which by common consent, I think I may safely say, comes more nearly satisfying the demands of experimental fact than any other ever devised. I refer to the atom first proposed in a general way by Lord Kelvin in his paper entitled 'Epinus Atomized,'[2] and afterwards profoundly modified by Lorentz, Thomson and Larmor.

So much work along this line has been done in the Cavendish Laboratory that one feels impelled to call this 'the Cambridge atom'; in view, however, of its structure, perhaps 'the Saturnian atom' is a more appropriate designation.

Now as to the proper conception of the normal Saturnian atom—I am not certain that I know what this is, but my mental

[2] Baltimore Lectures, p. 541 (Cambridge, 1904).

picture of it is somewhat like the following:

1. Conceive a single negatively charged electron—whatever that may be—placed inside a mass of positive electrification—whatever that may be. On the basis of the Zeeman effect, we may imagine this electron to be revolving about the center of the positive charge; and we may assume its rate of revolution such that it is in equilibrium under the first-power-of-the-distance law.

But even if we did not have the Zeeman effect to suggest rotation we should be compelled, as Jeans[3] has shown, to introduce rotation, on the basis of Earnshaw's theorem, to secure stability. Any acceleration of this electron which is periodic will produce a periodic radiation of energy. Precisely such an acceleration is here present in the familiar rw^2 centrifugal acceleration which is periodic when we consider radiation along any one fixed direction. The frequency of this acceleration determines that of the radiation just mentioned. The electromagnetic effect is roughly that of an alternating displacement current.

2. Let us next suppose that instead of a single corpuscle we have a large number distributed throughout the same orbit. Their radiation is now almost nil, the vector sum of the accelerations being zero and the electromagnetic effect being roughly that of a *steady* current.

If the ring does not contain so many of these self-repellent corpuscles as to become unstable we have a simple type of a non-luminous and, during stability, non-radioactive element.

Imagine now that these electrons are the same for all elements; then one element differs from another mainly in the number, disposition and character of the rings which surround the central attracting charge, the number of corpuscles being 'of the same order as the atomic weight.'[4]

Such is a rough sketch of the normal Saturnian atom. The beautiful manner in which this structure permitted J. J. Thomson to infer the same periodicity in electrochemical properties as that contained in Mendelejeff's table is already familiar to you. But for the present inquiry this exquisite achievement of Thomson's is merely an 'aside.' So also is Drude's elegant connection of electrical and thermal conductivities in metals. Likewise his explanation of the Hall effect.

We come now to the question which is fundamental to all spectroscopic theory, namely, *under what conditions* does a gas atom become radiant.

This question may be asked and answered in two different senses:

First, one may inquire as to the laboratory conditions necessary to produce luminosity in a gas; the corresponding answer is threefold: either a high temperature, thus obtaining, in some rare cases, a heat spectrum, or secondly, a rapid chemical change as in flames, or thirdly, an electric field as in the arc, spark and vacuum discharge.

Again, one may ask what is the difference between the internal conditions of a radiant and non-radiant atom. So far as I am aware, this latter query has never received an answer which is definite or based upon indisputable experimental evidence. However, the Zeeman effect points to rotation in the luminous source and suggests the revolving electron as the light-giving body; but it is difficult to see how one electron could give rise to more than one line in the spectrum. Not only so, but, since the electrons are the same for all elements, it is clear that the electrons alone can not emit characteristic spectra, the

[3] Jeans, *Phil. Mag.*, **2**, 425, 1901.

[4] J. J. Thomson, *Phil. Mag.*, **11**, 774, 1906.

'sign-manual of the elements.' Riecke and Stark[5] have furnished us excellent experimental reasons, by observing the motion of luminous lithium vapor towards the cathode, for thinking that the radiant source is the positive ion; this, it will be noted, is not at all inconsistent with the evidence of the Zeeman effect, since the positive ion carries with it probably a large number of negative electrons, being itself merely a neutral atom minus one or more negatively charged corpuscles.

Thomson has recently presented three different lines of argument for thinking that the number of corpuscles in the atom is of the same order as its *usual* atomic weight, *i. e.,* in terms of hydrogen as unity. But most elements emit a number of spectral lines which is enormously greater than their respective atomic weights.

Thomson is, therefore, driven to conclude that "when an atom of an element is giving out its spectrum either in a flame or in an electric discharge, it is surrounded by a swarm of corpuscles; and combinations not permanent, indeed, but lasting sufficiently long for the emission of a large number of vibrations, might be expected to be formed. These systems would give out characteristic spectrum lines; but these lines would be due not to the vibrations of the corpuscles inside the atom, but of corpuscles vibrating in the field of force outside the atom."[6] The immediate cause of luminosity would be the bombardment of this system by free corpuscles, or in the case of flames the disturbing cause would presumably be molecular collapse.

At another time and place Thomson fortifies his view as to the complexity of the radiating atom, by citing the discovery of Lenard, that the speed with which the corpuscles are expelled from the atom by ultra-violet light does *not* depend upon the intensity of the incident ultra-violet light; thus indicating the necessity of at least two independent vibrating systems within the radiant atom.

I am not clear as to the proper interpretation of the measurements which have been made by Stark[7] upon the Doppler effect in hydrogen canal rays; but in any event they would seem to prove that the luminous source in the vacuum tube is not the simple corpuscle of the cathode rays, but the more complex, positive ion (not necessarily the positive charge) of the canal rays. While the experiments of Hull[8] during the past year combined with that of Schuster and Hemsalech point to a luminous source of such size and complexity that a condition of luminosity may be 'propagated along' it.

The upshot of the whole matter, then, is that we are landed with a picture of the radiant atom which is complex quite beyond description. One is reminded, indeed, of the words which Marie Corelli puts in the mouth of Lionel, the youthful hero of her 'Mighty Atom.' "Oh, dear Atom!" says he, "you must be very much more than I have been taught to believe you are."

Let us, however, accept the structure as we have received it and, with apologies for any feature of it which has not been correctly represented to you, let us now ask ourselves, how well it can serve as a foundation for our spectroscopic edifice.

APPLICATION OF CRITERIA

We may make this test most easily perhaps by asking how this atom will account for the nine fundamental phenomena mentioned above, and—

1. Does this Saturnian system give sharp lines? Can one predict from its structure that its radiation will be concentrated in a

[5] Riecke & Stark, *Physik. Zeitsch.*, **5**, 537, 1904.

[6] Thomson, *Phil. Mag.*, **11**, 774, 1906.

[7] Stark, *Ann. d. Physik.*, **21**, 401, 1906.

[8] Hull, *Proc. Roy. Soc.*, A, 521, p. 80, 1906.

few definite positions in the scale of wave lengths? I think it must be admitted frankly that, in the picture of the atom as given us, there is nothing which determines the rate of rotation of the corpuscle in its orbit, and nothing, therefore, which gives the same periodicity of radiation for all atoms of any one element.

This difficulty has been stated in a very forcible manner by Professor Jeans, and again by Lord Rayleigh. Each of them has proposed a method of getting around the difficulty, one[9] by introducing a law of electrical action different from the simple law of inverse squares, the other[10] by introducing mobile negative particles, which do not revolve, but which vibrate about their positions of equilibrium located in a rigid positive charge.

But the adoption of either of these suggestions would completely change the entire character of the atom. It may be that we shall later be driven into the corner and be compelled to accept some such mode of escape. But at present these devices impress one as highly artificial, and too inconsistent with other facts to warrant adoption.

The crux of the situation seems to be just here, if one assumes that the frequency of the radiation is identical with the rate of revolution of the corpuscles, he can not expect sharp lines in the spectrum. If, on the other hand, one assumes that light is due to the internal vibrations of the corpuscles, then not only does he fail to predict the Zeeman effect, but he is forced to conclude that since the corpuscles appear to be the same for all elements, the spectra of all elements should be identical. To derive the Zeeman effect from a rectilinear vibration by substituting for it two circular vibrations would seem to employ a purely kinematic device instead of offering a physical explanation. Nor is this all: The value of e/m obtained from the Zeeman effect is, if not correct, at least in beautiful accord with values determined in a variety of other ways.

Summarizing, one might say that the adoption of the Saturnian atom would compel us either to give up the Zeeman effect, or to give up sharp lines in the spectrum. On the other hand, I am not aware that it has ever been shown that even in a gas-spectrum the region between any two lines *is* entirely free from radiation. Is there any spectrograph so free from diffused light as to make an experimental answer to this question anything other than a more or less rough approximation? But even if every spectrum is, to some slight extent, continuous, the fact remains that spectral lines are essentially sharp.

2. Passing now to the second fundamental fact, which is that spectral lines are *not* perfectly sharp, but (within limits not yet resolved by any grating) possess a complicated structure, Professor Michelson[11] and Lord Rayleigh[12] have shown that in the case of a gas at low pressure the chief, if not the only, cause of widened lines is motion in the line of sight, an effect which depends as much upon pressure and temperature as upon atomic structure. But when it comes to the asymmetric distribution of intensity within these narrow lines, *i. e.*, a linear structure such as has been revealed to us especially by the interferometer in the hands of Michelson, an effect which would appear to be a function solely of atomic structure, then the chances of explanation in terms of the Saturnian atom appear even more remote than in the case of perfectly sharp lines.

3. The next query to be presented to the Saturnian atom is what explanation can be offered for the fact that very many lines in

[9] Jeans, *Phil. Mag.*, **11**, 607, 1906.

[10] Rayleigh, *Phil. Mag.*, **11**, 118, 1906.

[11] *Astroph. Jour.*, **2**, 251, 1895.

[12] *Phil. Mag.*, April, 1889.

the spectra of the elements are arranged in series such that the wave lengths of any one series are functions of only two constants and the successive whole numbers?

So far as I am aware, no answer which is even approximately satisfactory has ever been offered in reply to this question. The fundamental difficulty here has been shown by Lord Rayleigh to lie in our measure of force, so to speak; in the fact that force is a second derivative of displacement with respect to time. Describe any dynamical system you please in terms of a differential equation; integrate it under conditions which yield a periodic solution; solve for the frequency, and you will find its value always entering to the second power.

But the difficulty under which the Cambridge atom here suffers is not peculiar to it alone.

Ritz,[13] in a doctor's dissertation of extraordinary merit, offered at Göttingen, has succeeded in devising a formula which contains fewer constants than that of Kayser and Runge, yet represents the observed wave lengths with a distinctly higher accuracy. And it might, at first glance, appear that we have here a truly dynamical explanation of the series phenomenon. But on closer inspection one finds that the fundamental picture—the mechanism, if you please—from which Ritz derives his differential equation is one having properties which are purely hypothetical and, in nature as we know it on a larger scale, quite impossible.

His vibrating body is a square (sometimes a plate, sometimes a membrane), whose behavior he studies under different boundary conditions. But it has this remarkable property that the effect of any one element of the membrane upon any other element is not merely a function of the distance separating the elements, but varies directly as this distance. A device so artificial could at most be called quasi-kinematical or purely mathematical. However, in each of the particular cases which he has integrated the frequency expression turns out to be practically identical with Rydberg's formula. The only dynamical justification for the entire proceeding appears to lie in the fact that he has chosen a two-dimensional body to yield a double infinite number of spectral lines.

Garbasso[14] has made an interesting attempt to obtain the Kayser and Runge series from certain combinations of Hertzian oscillators. And his solutions have this special merit, namely, they all refer to physically realizable models. But the number of frequencies which he has computed is too small to furnish even an approximate test as to whether they satisfy the law of Kayser and Runge, much less do they point out a general dynamical system from which the law of the series may be derived.

In spite of the fact that no satisfactory explanation has been obtained, one can hardly avoid the conclusion that Rydberg's formula is something more than a convenient expression for interpolation. The fact that his second constant, N_0, is the same for all elements, while another has a characteristic value for each particular element, and that a third constant locates the particular series in any one element, would seem to indicate that these three quantities are in some sense parameters of matter. Yet I am aware that this view is a mere suspicion and is not at present capable of proof.

When other types of spectra, such as that of iron, are brought under the 'reign of law' we may find a simpler view; or what is more likely, we may feel, with

[13] Ritz, *Ann. der Physik.*, **12**, 264–310, 1903.

[14] Garbasso, 'Theoretische Spectroskopie,' pp. 130 and 180, 1906.

Professor Runge, that 'nature is getting more and more disorderly every day.'

4. Passing now to the pressure shift, we owe to Dr. Humphreys, one of the discoverers of the phenomenon, a clear explanation in terms of the Saturnian atom.

He points out first of all that an atom built on the model indicated, with quantitative specifications such as these given, by experiment, will exert an enormously powerful field at points near its center. And since the convection currents here existing are practically equivalent to Amperian currents in circuits devoid of resistance, it is clear that any currents induced in these atoms by attempting to thrust through them more or fewer lines of force than they now contain will be permanent and, therefore, unlike the induced currents in the wire circuits of our laboratories.

But an increase of current in such an atom means an increase of speed in the corpuscles, and this in turn means presumably an increase of frequency. From this it follows that, if by any means two atoms are brought closer together there will in general be a change of frequency, and one may expect this change to be sometimes an increase and sometimes a decrease; that is to say, two atoms which are made to approach by pressure, and which we may imagine as strung on a common axis, will sometimes be rotating in the same sense and sometimes in the opposite sense. The increase and decrease of frequency thus secured by pressure will have the effect of widening the line. The plausibility of this argument is much enhanced by the enormous strength of the magnetic field in the neighborhood of one of the magnetic atoms; in the case of the iron atom amounting to as much as 150,000 C.G.S. units at a distance of ten radii from the center and, of course, a thousand times greater than this at the center. So much for the widening due to pressure.

But Dr. Humphreys has also very cleverly suggested that this widening will not be symmetrical about the original position of the line, but about a new position on the red side of the old. For when two atoms happen to be rotating in the *same* direction they will attract each other and then get 'into the stronger portion of each other's magnetic field.'[15] In other words, those effects which result in lengthening the waves will be much more marked than those which shorten the waves. Hence increase of pressure will be accompanied by shift towards the red. It would be exceedingly interesting to know what difference of structure exists between the radiant sources of lines and of bands which causes this explanation to break down when applied to the latter.

5. Passing now to the effect of a magnetic field, an experimental fact which largely established, although it did not suggest, Lorentz's conception of the electron vibrating about an attracting center, it is at once evident that the Zeeman phenomenon must follow as a deduction from the Saturnian atom.

But Preston and Runge and Paschen have shown that the normal triplet is by no means an ordinary occurrence; on the other hand the breaking up is very much more complicated, a single line yielding anywhere from 3 to 14 components.[16]

Beautiful as the general agreement beween fact and theory here is, one finds it peculiarly difficult to understand how the central line of the normal triplet—the one due to the component of motion along the lines of force—can be split up at all by the magnetic field. But since, as a matter of fact, this component *is* split up, it is

[15] Humphreys, *Astrop. Jour.*, **23**, 243, 1906.

[16] Runge & Paschen, *Abh. Ber. Akad.*, February 6, 1902.

only fair warning that the forces here involved are possibly not to be limited to those of purely electrical origin; but it is not obvious what experiment can be devised to answer these questions.

6. Passing next to that somewhat motley group of phenomena which we have classed under the head of Multiple Spectra, I am not certain that there is a single fact in the entire group that can be predicted from the structure of the atom which has been assumed.

In his Royal Institution lecture[17] of this year, J. J. Thomson has suggested an atom with certain capacities for receiving and spending energy, and has described this atom by simple differential equations, involving certain atomic constants; but the connection between these constants and the Saturnian atom are by no means clear.

So that while he accomplishes the explanation of the phenomena for which this new atom is introduced, the explanation can hardly be said to hinge upon the atomic structure, which has so highly recommended itself in other directions.

At present, we seem to be justified in going little farther than to say that *rapidity of change of electric field* seems to be *a* (not *the*) determining factor in nearly every case. The main difference between the arc and the spark appears to be confined to the earlier stages of the spark. The oscillograph shows that spark lines are introduced into the metallic arc when the break is quickened; the interruption of an intermittent arc is very much hastened by a hydrogen atmosphere; and one might think that, therefore, the action of hydrogen in introducing the spark lines is completely accounted for, and perhaps justly so, if it had not been discovered by Hale, Adams and Gale[18] that an arc fed by a small steady current and surrounded by hydrogen also yields spark lines. But this calls for an examination of the steady (?) current by means of the oscillograph.

The effect of a very minute current in introducing spark lines probably also rests upon the greater rapidity with which the small current is interrupted. The effect of self-induction is to retard the break, and hence obviously to obliterate spark lines. The effect of a parallel capacity is, of course, to increase the speed of break. The spark lines obtained in arc under water are apparently special cases of the effect of a hydrogen atmosphere and a consequent rapidity of break.

A most valuable research at the present time would be one which would determine whether for a 'quickness of break' as a unifying principle we should substitute 'rate at which energy is delivered' or, as Thomson suggests, 'rate at which energy is delivered *combined with rate at which energy is spent.*'

Until more definite information upon this point is obtained, it remains almost impossible to say how the Saturnian atom must be modified in order to explain multiple spectra.

7. As to the relation between spectral phenomena and atomic masses, we must, I think, all feel the most hearty admiration for the manner in which Professor J. J. Thomson has succeeded in picturing the periodic law as an almost immediate consequence of the atomic structure which he has proposed. The way in which the normal atom, by the addition of a few subatoms, becomes electro-positive or electronegative is especially attractive. The achievements of Runge and Precht[19] in closely approximating if not accurately determining the atomic mass of radium is

[17] Thomson, *Chemical News*, **94**, 197, 1906.

[18] Hale, Adams and Gale, *Astroph. Jour.*, **24**, 213, 1906.

[19] Runge & Precht, *Physikal. Zeitschrift*, **4**, 285–287, 1903.

equally striking. Rydberg[20] has pointed out that certain spectroscopic properties of the elements recur with exactly the same periodicity as that discovered by Mendelejeff.

But it is important to observe that in none of these laws is the question of atomic structure involved, but only that of atomic mass. Not a single one of these phenomena has been predicted from previous notions concerning the atom, and indeed all our views concerning the Saturnian atom, with the single exception of the Zeeman phenomena, appear to be singularly devoid of that spirit of prophecy which characterizes all sound theory.

8. When confronted with the fact that many spectral lines show self-reversal, the atom which is now on the witness stand replies that its explanation of this phenomenon is identical with that of any other atom which consists of a vibrating mechanism.

The essential feature of a line-reversal appears to consist of a source *within* the arc or spark which emits, from whatsoever cause, a relatively wide line—thus approximating in small degree the incandescent solid first used by Kirchhoff. The *outside* region of the arc or spark emits, from whatsoever cause, a relatively narrow line, and its radiant atoms are, therefore, capable of absorbing only certain periods from those which appear in the broad or inner source.

Accordingly, the phenomenon of reversal is, in a certain very true sense, *not* an atomic problem. The fundamental question here involved is, however, the following, namely, how does it happen that, in the *inner* source, some atoms have their frequencies slightly increased while others have theirs slightly diminished? And this *is* an atomic problem.

[20] Rydberg, 'Rapports Congrès de Physique' (Paris), II., 217.

We may say that it is due to 'increased density' of the luminous vapor, but we are little wiser for that. In this emergency, the Cambridge atom with its entourage of stray corpuscles shows itself very capable; for it is exactly in such a region, as the interior of an arc, where the electric field is strong, and where collisions are frequent, that one might expect this 'swarm of corpuscles' to be varying largely—thus altering slightly the period of the radiant source, whatever that may be, both by change of inertia, and by change of electric field—acting, so to speak, both upon the numerator and denominator of the expression for the period. The case of double reversals has been reduced to that of single reversals by the clever experiments of Dr. Humphreys.[21]

The case of Wolf-Rayet stars[22] where H^{α} and H^{β} are bright while the remaining hydrogen lines are dark is, so far as I am aware, an unsolved puzzle.

9. Regarding the last of the nine criteria which have been cited, namely, the well-nigh impossible feat of securing a line-spectrum from a gas by means of heat alone, the situation seems to be as follows.

If one assumes that ordinary temperatures are due to the translational energy of the atom, while light consists in dissipation of vibrational energy *in* the atom, then Jeans[23] has proved, on dynamical grounds, that it is possible only in a minute degree to transfer energy 'from the principal degrees of freedom to the vibratory degrees of freedom'—at least with any temperatures which are encountered on this planet. In the case of ordinary collisions, the transfer is infinitesimal on account of the high frequency of the vibrations as compared with the duration of the collision; in the case of those rare collisions in which the

[21] Humphreys, *Astroph. Jour.*, **18**, 204, 1903.

[22] Campbell, *ibid.*, **2**, 177, 1895.

[23] Jeans, 'Dynamical Theory of Gases,' Ch. IX.

duration of collision is comparable with the period of vibration, the transfer of energy is 'infinitesimal on account of the extreme rarity of these collisions.'

This point of view has, I think, been supported by the experience of every one who has attempted to obtain characteristic spectra from gases under conditions in which electrical and chemical processes were excluded.

The assumptions back of Jeans's discussion are to be justified, if at all, by experiment. Hence the importance of such work as that which King,[24] Hale[25] and others have recently been doing by means of the electric oven, and of the fundamental experiments of Wood upon optical resonance.

As to the bearing of the Saturnian atom upon this fact, one finds it in about the same position as any other elastic atom, except that for ordinary mechanical rigidity one has to substitute the quasi-rigidity which comes from rotation of the electrons about the positively charged center.

The effort to render a gas radiant by means of high temperature alone has been aptly characterized by J. J. Thomson as an effort to boil a tea-kettle by burning down the kitchen; the spectroscopic analogue of Lamb's roast pig.

In view of all the evidence, the conclusion would appear to be that spectroscopists have greatly exaggerated the rôle of temperature in terrestrial sources. That the behavior of an arc or spark is determined largely by the temperature of its *electrodes* there can be no doubt; but it seems almost equally certain that the effect of changing temperature upon the character of the spectra is produced through the intermediation of changed electrical conditions in the source.

CONCLUSION

In turning the pages of Kayser's great compendium, which so adequately represents the present phase of spectroscopy, there is but one tinge of disappointment; and this is that, in the presence of such a wealth of facts, there is so little in the way of fundamental well-established unifying principles. And yet the only remedy appears to be one of the homeopathic sort, namely, more facts.

There is, however, this comfort: things are not as bad as they used to be. Our condition is somewhat that of the old judge who never liked to admit that he was not in perfect health. On one occasion, when he was just recovering from an illness, a friend met him on the street and asked him how he was feeling. "Well!" said the judge in reply, "I am not quite myself, but I am a great deal better than I was at the time when I was not so well as I am now." This, too, is to be remembered, that we can never hope for any solution which can in any sense be called final—*all* solutions are merely passing phases—the problem is not one either of mathematics or of history.

Let us then continue our search for the facts of the case confident in the belief that when this work has been properly accomplished, the unifying principle will be at hand. It should be to us a matter of no small pride that it is to our fellow members in this section that spectroscopy owes the bolometer, the curved-grating, the echelon, the spectroheliograph, and, therefore, in large measure the beautiful results obtained with these instruments.

In the meantime we must, I believe, all gladly admit two things: first, that the atom which we associate so closely with the Cavendish Laboratory more nearly supplies the desired principle than anything else which has been offered, and secondly, that the emission of a line spectrum is a very imperfectly comprehended phenomenon.

HENRY CREW

[24] King, *Astroph. Jour.*, **21**, 236, 1905.

[25] Hale, Adams and Gale, *ibid.*, **24**, 213, 1906.

SOME PROBLEMS CONNECTED WITH DEEP MINING IN THE LAKE SUPERIOR COPPER DISTRICT

THE copper mines of the Lake Superior district are essentially low grade. Their profitable operation is made possible by the great extent of the lodes, their comparative uniformity of character and the investment of great sums of money to maintain operations on a vast scale over a long period of years.

With but one important exception, the lodes are the vesicular tops of ancient lava flows which subsequent to solidification have had the cavities wholly or partly filled by the deposition of various minerals, among which is native copper. They dip at angles varying from 38° to 70°.

The modern shafts through which the rock is hoisted are either inclined, following the plane of the lode, or are vertical. The inclined shafts are of dimensions such as to provide for two railroads of approximately standard gauge on which run the 'skips' which are operated in balance. In addition there is room for the ladder way and air pipes, usually placed at one side. The vertical shafts have compartments providing usually for 'cages' and pipe and ladder way. Several of the inclined shafts are over 5,000 feet long. One has a length of 8,100 feet. Of the vertical shafts the three deepest are, respectively, about 5,200, 5,000 and 4,900 feet deep.

Long before such depths were actually reached there arose the question as to a possible limit set by the ultimate crushing strength of the rock which is penetrated. Manifestly, mining can not go to a depth such that the weight on walls of drifts and stopes will exceed the ultimate strength of the material of which they are composed. There is a widespread impression that the lake mines are approaching such a limit. There are current statements to the effect that pieces of rock occasionally snap off the rock faces because of the great strain, and are violently projected as if propelled by an explosive.

In this connection a few figures will be of interest. The average density of the rock of the copper-bearing series is not far from 2.87, that is, a cubic foot weighs about 179.3 pounds. Therefore, a horizontal square foot of area at 5,000 feet from the surface has above it a column of rock weighing 448 tons. The ultimate crushing strength of the average rock is not well known, but since it is mostly trap this may be safely assumed as at least 1,200 tons per square foot. If, therefore, the square foot above defined carries the entire column above it, it is loaded to much less than half the crushing strength and only at nearly three times the assumed depth will the load reach its crushing limit. At a dip of 38°, the pressure normal to the plane of the lode at 5,000 feet from surface is only 354 tons per square foot. It is in this direction that the crushing forces are mostly called into play. As the dip increases this normal pressure of course diminishes. At 52° it is 278 tons, and at 70° it is 152 tons per square foot.

However, the matter does not end here. The removal of large portions of the copper zone leaves considerable areas of the roof or hanging wall to be supported by the pillars which are left for the purpose, or by the walls of the opening, or by both. The weight on pillars and walls is thus increased and may easily approach the crushing limit. Take, for example, a long pillar 50 feet wide having on either side an open space of 150 feet. Suppose it in a lode dipping 38°. Allowing for neither rigidity nor arching, and supposing the weight on the pillar evenly distributed, at 5,000 feet deep it would be subjected to a pressure of 1,239 tons per square foot, a pressure under which it would fail.

As a matter of fact, in such a case the rigidity of the rock mass distributes a large part of the load out over the rock beyond the walls of the opening. That this rigidity may be considerable is illustrated in several cases where areas of hanging as wide as 200 feet or more have no support between walls, and yet have stood up for several years. They are not, however, at maximum depth.

In such an area a pillar when first cut out may have to carry but little more than its previous load. As the hanging wall slowly bends the pillar must take up more and more of the extra weight. This is not applied uniformly. As the rock between pillars and walls bends downward the tendency is to concentrate the load at the edge or face of the pillar, or wall, much as a beam does when supported in like manner. The outer parts of the pillar may thus become overloaded and here it will fail.

It does so by the splitting off of pieces of rock much as may sometimes be observed with a specimen in the testing machine, though on a much greater scale. These pieces break from the base as well as the top, and, as a rule, like any hard rock under a crushing load, the pillar fails suddenly. Small pieces of rock may fly to a considerable distance, and such occurrences have undoubtedly given rise to the above-mentioned exaggerated impression of the compressive stress to which the rock is subjected in the lowest levels.

The hanging rock mass moves, of course, when the pillar crushes, and the vibration due to the sudden though slight displacement is often conveyed to the surface. The result is a miniature but perfectly genuine earthquake which may be felt over a distance several times that of the pillar from the surface. With the crushing of the pillar and the movement of the hanging a readjustment of the weight takes place, and the process begins over again. Instead of the process being repeated exactly it is possible for the hanging to break in such a manner that the arching effect may protect this pillar, and place the load on others. Eventually, at great depths the hanging and foot must come together, and in one mine the final steps in the process came so rapidly as to completely wreck it.

The pressure normal to the plane of the lode is not the only action which may appear. The pillars are not, as a rule, separate from either foot or hanging. They are parts of the same rock mass, and it is not possible for the hanging to slide over the pillar. In consequence the readjustments which take place when a pillar fails as above described sometimes put an enormous longitudinal thrust on the foot, and in places its surface portion has buckled up under such stress. Also, at points where shaft pillars have been weak, shafts have been pinched and twisted under the same conditions so as to interfere with their operation.

Experience seems to have shown that at the great depths recently reached it is useless to expect to hold up the hanging rock mass for a long time by any scheme of pillars unless far too much of the lode is left in place, and that the only feasible method is to cut away the entire lode and permit the hanging to cave as rapidly as it will to the point where the broken rock fills again the whole space, and redistributes the weight over the footwall. Following this plan, cutting out the lode, or 'stoping,' begins at the point furthest from the shaft, and progresses toward it. With a wide shaft pillar, or with the shaft in the footwall, and with some such general method which avoids concentration of pressure where it can do harm, there seems no reason to anticipate serious difficulties due to crushing for a further depth at least as great as that already attained.

The difficulties of surveying the mine are not markedly increased by depth, except in the case of vertical shafts. When these are deep, and it becomes necessary to carry down an azimuth from the surface by means of two plumb lines hung in the shaft, there is presented a problem of considerable difficulty. It is almost impossible to free the lines entirely from disturbing influences which displace them from their normal positions. If either lines or plumb bobs are of magnetic material the presence of iron pipes in the shaft may result in seriously disturbing them. Falling water may be in such quantity and so directed as also to affect the position of the lines.

However, the air currents, which can not be wholly eliminated, whatever the precautions taken, are the most serious cause of disturbance. The temperature at the bottom of the shaft is higher than that at the top, and in consequence convection currents are formed. The heat supplied from the surrounding rock keeps them up. Indeed, with moderately steady temperature at the surface, and with the shaft idle, a remarkably stable condition of these air currents may come about. Elsewhere observations have been published by the writer,[1] showing that their effect on a plumb line may remain sensibly constant for hours at a time while deflecting the line from its vertical position. The stability of the currents is made possible by the large cross section of the vertical shafts, 10 feet by 22 feet to 10 feet by 30 feet outside of timbers, thus giving a large air body, and the constancy of the rock temperatures, and the supply of heat through the shaft walls. When we take account of the fact that a force equivalent to a horizontal pressure of 10 grains on a 60-pound plumb-bob suspended by a line 4,000 feet long will displace the bob one tenth of a foot from its normal position, it is easy to see how apparently slight causes may produce appreciable error in azimuth.

[1] See *Engineering and Mining Journal*, April 26, 1902, also *Electrical World*, April 26, 1902.

General attention was first attracted to this problem by the very noticeable divergence of two long plumb lines hung in shaft number five of the Tamarack mine. Of course, divergence alone would not affect azimuth, but the question confronting the surveyor is whether the divergence may not be due to some cause which may also displace one or both of the lines in a direction perpendicular to their plane. In the case mentioned it required a great deal of investigation and experiment to fasten the responsibility on the currents of air.

It is remarkable how many persons are ready to accept as an explanation of such divergence the statement that there is an excess of gravitation on each bob horizontally toward the end wall nearest to which it hangs. All are familiar with the picture in the text-books of the plumb line hanging near the face of a precipice being deflected from the vertical by the attraction of the mountain mass. The idea so strikingly conveyed by this picture while qualitatively correct seems in most minds to be quantitatively wrong. There is an excess of attraction on the bobs as stated, but its amount is far too insignificant to account for any observed divergence. At Tamarack number five it could account for no more than one one-thousandth of a foot. The convergence of vertical lines in that instance is over three times this amount.

The fact remains that the surveyor who must thus transfer an azimuth from the surface down a very deep shaft has a problem the proper handling of which must involve a careful study of the local conditions, particularly in regard to air circulation.

Increased depth tends to lessen the output of a given shaft, and in the effort to prevent this, and also to reduce hoisting charges, loads have grown larger, likewise the speed at which they are hoisted. Loads of nearly seven tons are being raised on some of the inclines at speeds up to 40 miles per hour.

A considerable item in the hoisting charges is the cost of maintenance of the skip road, the expense per ton for this purpose increasing with the length of the shaft. The usual skip road is carried on heavy timbers which are placed transversely along the foot, and support large stringers to which the rails are spiked. The expense of maintenance of this road for great depths is such that some of the mines are substituting for it a road consisting of a rail attached to a concrete stringer which is borne directly on the rock of the footwall.

The stringer proper is about 13½ inches wide by 14 inches high. Beneath it and supporting it is a mass of concrete 16 inches to 18 inches wide extending to the rock. The depth of this supporting mass varies considerably, depending on the irregularities of the rock face to which it is attached. It ranges all the way from 4 inches to 2½ feet, and there is no reason why these limits might not occasionally be exceeded. The stringer and supporting mass are structurally one piece, being molded together. The stringer portion is reinforced by means of a 1⅛-inch steel cable passing longitudinally through its interior. The rail is attached by bolts which are spaced three feet apart, and hold it by means of clips which grasp the rail flange. The bolts pass through the stringer into a rectangular opening about three by four inches in section, which passes quite through the concrete, and affords access to the lower end of the bolt.

In building the road a form of plank is constructed having the cross section of the stringer, and a length of 15 feet. To its lower side are nailed the cores for the bolt openings just mentioned. The top is left open. From one to three of these forms are supported in place underground by suitable means, and to the sides are nailed boards extending downward to the rock. These make the form for the supporting part underneath the stringer proper. When all is firmly secured the rock face is thorougly washed, and the concrete is filled in, beginning at the lower end. The mixture used is one part Portland cement, three of sand and five of crushed rock, and it is tightly rammed in place. As the filling proceeds the top of the form, or the cover, is completed by nailing on short pieces of plank. In the roads which have been built at a slope of 70° the structure is anchored to the footwall by heavy bolts spaced 8 feet apart. When the concrete has hardened sufficiently the form is removed and the rail is bolted in place. The road is then complete.

The first of these roads has been operating some three years, and has proved so satisfactory that the others have followed. Expectation regarding cost of maintenance has been fully realized, and, in addition, it has been found that the original cost of the road is less than that of wooden construction. Its disadvantages seem to be confined to the fact that, due to the unyielding nature of the support, the effect of a blow must be absorbed between wheels and rail, resulting in a perceptibly greater deterioration of both. However, this is far more than compensated by the decreased cost in other directions and the fireproof character of the road.

In the effort to counteract the increased costs due to great depth the economical generation, distribution and use of power have naturally received large attention. The long period of operation necessary makes it worth while to expend large amounts of money in installing machinery of large capacity and high economy. The extreme in the direction of high-duty engines is represented at present in the Nordberg quadruple expansion, two-stage air compressor operating at the Champion mine. The engine is equipped with Nordberg's regenerative feed water heating system. It holds the world's record for minimum consumption of heat per foot-pound of work delivered, having shown a duty of 195,000,000 foot-pounds, being about 9 per cent. in advance of its nearest competitor.[2]

But one mine is so situated to be able to make use of water power. The Victoria, located near the Ontonagon River, has a considerable water power available which has been utilized in a novel manner. Distance from source to points of application is not great and the power for both mine and mill is distributed by compressed air. Moreover, the falling water operates directly on the air without the intervention of water-wheel or other machine.

Roughly speaking, this 'hydraulic air compressor' consists of a large underground chamber cut from the solid rock and having a length of 281.5 feet, a width of 18 feet and a depth of 26 feet. In normal operation the lower portion of the chamber is filled with water to a level of 14.5 feet from its roof. The remainder, having a capacity of over 80,000 cubic feet, is filled with air at 114 pounds gauge. The water outlet is through a tunnel 18 feet wide and 10 feet high, the bottom of the tunnel being a continuation of the horizontal floor of the chamber. This tunnel opens into an inclined shaft about 18 by 20 feet in cross-section, the discharge level of which is 271 feet above the water level in the chamber. The outlet for air is a 24-inch pipe leaving the chamber through the roof directly above the tunnel outlet.

[2] See description and report of test in paper on 'A High Duty Air Compressor,' by Professor O. P. Hood, *Transactions American Society of Mechanical Engineers*, 1906.

The inlet for both water and air is through three vertical shafts, each 5 feet in interior diameter, opening through the roof of the chamber at the end opposite to the outlets. Each shaft is tightly lined with concrete, and is continued down into the chamber by means of a somewhat flaring steel casing to a distance of about 15 inches below the water level. Just beneath, and reaching somewhat into the opening of the casing, is a conical boss of concrete. From water level in chamber through the air- and water-tight shaft to the water level at the intake above is 343 feet. This is 72 feet greater than the distance between the water levels at the outlet end, and this difference expresses the available head of water.

At the intake water is admitted through an annular funnel over a hollow ring from the flat inner periphery of which project 1,800 three eighth-inch tubes into the annular opening of the funnel. The water must flow over the mouths of these tubes, and in doing so it produces the aspirating effect which entrains the air in small bubbles. The air comes mainly through the tubes from the hollow ring which has suitable intake pipes extending above the water level. The mixed water and air fall through the vertical shaft, and are discharged radially from the annular outlet formed by the concrete boss and steel casing below. The current through the chamber is slow, and the air disengages itself, collecting in the top of the chamber, and displacing the water. By its pressure on the water surface in the chamber it maintains the 271 feet of difference in level between that surface and the one at the discharge of the outlet shaft.

While running at less than full capacity the water level in the chamber is prevented from being depressed further than 14.5 feet from the roof by means of a blowoff pipe, 12 inches in diameter, which opens somewhat below the normal water level. When the water has been pushed down sufficiently air enters this pipe, and its escape relieves the excess of pressure. When the blowoff is in operation the appearance at its mouth greatly resembles the eruption of a powerful geyser. The stream of spray, due to the entrance of water with the air from the chamber, is thrown sometimes to a height of 400 feet.

The capacity when all intake shafts are operating is about 5,000 horse power. So far but one intake is used. This under test at near its maximum capacity showed an efficiency of better than 82 per cent. while delivering 11,930 cubic feet of air per minute at 128 pounds absolute pressure.

All machinery at the stamp mill and at the mine, whether on the surface or underground, is operated by compressed air. Beside utilizing cheap power the compressor has obvious advantages over the usual machine in the absence of parts to get out of order and in low cost of attendance.[3]

The foregoing examples are presented to Section D as illustrating the type of problems which are arising in connection with the extensive operations at great depths on the low-grade lodes of the Lake Superior copper district.

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Michigan College of Mines,
December, 1906

[3] See description by A. L. Carnahan in the *Mining World* of August 25, 1906, and by C. H. Taylor in *Mining and Scientific Press*, August 18, 1906.

WHY HAS THE DOCTRINE OF LAISSEZ FAIRE BEEN ABANDONED?[1]

Perhaps the most remarkable change which economic opinion has undergone during the last fifty years has been the change from the extreme *laissez faire* doctrines of the classical economists to the modern doctrines of governmental regulation and social control. And yet there has been very little attempt to explain why *laissez faire* has been so generally abandoned. Its abandonment has been gradual and almost unconscious, not so much the result of any rival abstract doctrine, as the cumulative effect of experience, which in hundreds of individual cases has brought men face to face with the practical limitations of the let-alone policy. The movement is fast bringing us back to the old view by virtue of which economics was first named *political* economy.

The revival of governmental activity in economic affairs is due to causes which are partly political and partly economic. This paper has to do chiefly with the economic causes and we shall, therefore, merely note in passing the chief political aspects of the problem. One reason for the extension of governmental control of industry is the growing strength of governmental control in general and of popular confidence in it. *Laissez faire* was a natural doctrine in a time when governments were weak and inefficient. Change of power has brought change of the theory of power. Compulsory workmen's insurance we find in the strongly developed German Empire; railway rate regulation follows increased power and centralization of government. It may even be said that much of the modern government regulation of industry resulted from the attempt of governments to extend its powers in self-defense. It has been felt, for instance, that if the government did not control the railroads, the railroads would control the government. Government regulation here has taken on the aspect of a struggle for supremacy. Just as England feels the

[1] Address of the vice-president before Section I.—Social and Economic Science—at the New York meeting of the American Association for the Advancement of Science.

necessity of having a navy equal to the combined navies of several other European powers, so governments feel that they must overtop the corporate aggregations of wealth with which they may have to cope.

Were there space we might discuss the question how far the movement toward governmental interference can profitably be pushed. The doctrine of socialism lies at the extreme opposite pole from the doctrine of *laissez faire,* and we are moving toward socialism dangerously fast. Yet there are insuperable obstacles to the success of socialistic projects. Governmental power and efficiency are limited and, when one class of society attempts actually to rule another, there is always a tendency to corruption, inefficiency, lack of adaptability to new conditions and abuse of power. Socialism can not be put in practise without opposition, and to maintain itself socialism must hold the opposing class in subjection. Nominally this subjection would be a benevolent paternalism, but in political history it is the universal experience that the party in power, to entrench itself against attacks, soon usurps more power, employs indefensible and oppressive methods and tries to establish itself in the enjoyment of special selfish privileges.

Our present purpose, however, is to study, not the political, but the economic, side of the problem. The doctrine of *laissez faire* is that governmental interference, in economic matters at least, is unnecessary and harmful. Sometimes it is added as a corollary that not only should government let individuals alone, but also that individuals should let each other alone. 'Live and let live' and 'Each for himself' are the mottoes of this type of individualism. The advocates of extreme *laissez faire* maintain that one class is not justified in imposing its tastes upon another. They say, we must not meddle with our neighbors' affairs, even if they are wasting their lives in what appears to us trivial, useless or positively harmful gratifications. Those who love art, science or literature have no right, we are told, to criticize those who are bored by these things, but love prize-fighting, fast horses, fast society or high living.

The reasoning by which these individualistic doctrines were supported may be briefly stated in two propositions: first, each individual is the best judge of what subserves his own interest, and the motive of self-interest leads him to secure the maximum of well-being for himself; and, secondly, since society is merely the sum of individuals, the effort of each to secure the maximum of well-being for himself has as its necessary effect to secure thereby also the maximum of well-being for society as a whole.

In the light of the experience of the last fifty years, it is not difficult to see wherein each of these two propositions is in error. First, it is not true that each man can be trusted to pursue his own best interests. Some men need enlightenment, owing to ignorance of what constitutes their best interests, and others need restraint, owing to lack of self-control in following them. The necessity for both enlightenment and restraint has always been recognized in the case of children, and an examination of actual conditions will show that they apply—often with equal force—to adults.

Liberty is certainly indispensable in a healthy society, but liberty insensibly verges upon license. While most of us would still agree that sumptuary laws are ill-advised, there is certainly good ground for maintaining that the liquor traffic should be put under some restraint, even if only by high license. It is not true that the drunkard is the best judge of what is for his own well-being and that of his family, and it is still less true that even when he thoroughly recognizes his failings he will have the self-control to act upon that knowledge. Hence the liquor problem becomes a social as well as an individual question. Again it is not true that ignorant parents are justified in imposing their ideas of education upon their children; hence the problem of child-labor, instead of concerning only the individual, as was at one time thought, has important and far-reaching relations to society as a whole. The same principles apply to the restraint of gambling, vice, the suppression of indecent literature, the compulsion upon landlords to make tenements sanitary, and many other forms of governmental regulation.

Even where governmental intervention is impracticable or inadvisable, there will still be good reason for attempting betterment of conditions through the influence of one class upon another; hence come social agitations and the efforts of one class to educate or instruct another. On this principle are based the great modern movements for human betterment as exemplified by the Society for the Study and Prevention of Tuberculosis, the Society for Sanitary and Moral Prophylaxis, the National Civic Federation, the American Institute of Social Service, the National Child Labor Committee, temperance societies, college settlements, district nurse associations and other organizations.

Strange as it may seem to those of us interested in these movements to-day, the fact is that a generation ago many of them would have been regarded by the dominant Manchester School not only as impracticable, but as unnecessary and possibly harmful. The adherents of this school seemed to treat the difference between knowledge and ignorance as a mere difference in opinion, with which the government has no more concern than with difference of religious creeds. It is certainly true that the attempts of governments to impose what is regarded by the ruling class as the 'true religion' upon the entire people have always proved ill-advised; the recognition of this has produced the modern sentiment of religious toleration. But we are carrying toleration too far when we refuse to correct errors which science demonstrates to be false. There are doubtless millions of persons to-day who jeer at the idea that indiscriminate spitting is dangerous to public health, but it would be silly to allow their ignorant prejudice to prevail. The bacteriologist knows what the ignorant do not know, and every effort should be made to pass down this knowledge to the masses as soon as possible after it is discovered. We can not let any dogma of *laissez faire* prevent us from checking suicidal ignorance.

The world consists of two classes—the educated and the ignorant—and it is essential for progress that the former should be allowed to dominate the latter. But once we admit that it is proper for the instructed classes to give tuition to the uninstructed, we begin to see an almost boundless vista for possible human betterment. Instead of regarding the present state of society as a normal and desirable one because each man naturally 'seeks his own best interests,' we permit ourselves to judge each actual case by our own ideal standard. This standard may differ widely from the average of actual usage. We must always distinguish between the ideal or *normal*, and the real or *average*.

The average represents merely conditions as they are; the normal represents conditions as they ought to be. Yet nothing is more common than confusing the two. In fact, in most anthropometric or physiologic tables, the word 'normal' is used almost synonymously with 'average.' The normal height of man, his normal weight, his normal length of life, his normal diet, strength, etc., are all identified with the average.

In this way all question of possible improvement is begged. We are stopped at the outset from asking, for instance, whether men in general are too stout, for the average weight of mankind is *assumed* as 'normal.' The absurdity of such procedure becomes apparent as soon as we consider cases in which, by common consent, the average and the normal are held to be distinct. For instance, the average adult man certainly does not have normal teeth, for they are usually half decayed; nor normal hair, for he is usually half bald; nor normal posture, for he is usually round-shouldered. Average health is below normal health, average morality below normal morality. In the absence of evidence we have no right to assume that the average and the normal are identical, even when we lack the data on which to base an opinion. It is only recently, and in consequence of the movement against tuberculosis, that experts have come to realize how widely different is the average air we breathe from air which is normal for human respiration, and that investigation has shown the average diet, in America at least, to be abnormally nitrogenous. In view of such revelations we should be open-minded enough to accept evidence—should it be offered—that the average span of life is less than half the normal span, and the average efficiency less than half the normal efficiency.

Those who habitually confuse the normal and the average are prevented from seeing the possibility of progress. They take the position, as unscientific as it is obstructive of progress, that 'whatever is is right,' presumptively at least, and brand every one who deviates from the average as an eccentric or a crank. The confusion between the normal and the average thus leads to the confusion between the eccentric and the pioneer. An eccentric or a crank is properly a person who deviates from the *normal,* and is almost the opposite of the pioneer, who deviates from the *average,* but toward the normal.

Discrepancies between the average and the normal may apply—in fact, do apply—to the economic side as well as to other sides of life. But this the *laissez faire* doctrine denied. The world as it is was thought to be nearly, if not absolutely, the best world possible. One example of this complaisant assumption was in the use of the term 'utility' to signify the intensity of desire that men have for things. So far as I know, the only writer who has attempted systematically to distinguish between the desires of men as they are and as they should be, is Pareto, who for this purpose suggested a new term—ophelimity—to replace 'utility' as applied to man's actual desires, reserving for the term 'utility' its original sense of what is intrinsically desirable. Thus, to an opium fiend opium has a high degree of ophelimity, but no utility. Economists have not yet laid sufficient emphasis on the distinction between true utility and what Pareto calls ophelimity. A whole range of problems of social betterment is opened up through the distinction. Economists have received with derision the suggestions of reform of Ruskin. But, however impracticable his specific proposals, his point of view is certainly saner than that of most economists; for, as Ruskin has pointed out, it is absurd to regard as equivalent a million dollars of capital invested in opium culture, and a million dollars invested in schools.

But there remains to be considered a second fallacy in *laissez faire.* Not only is it false that men, when let alone, will always follow their best interests, but it is false that when they do, they will always thereby best serve society. To Adam

Smith it seemed self-evident that a man served society best who served himself best—though he would certainly have admitted that the rule had exceptions in the case of thieves, assassins and others who are obviously enemies of society. But the extent to which the classical 'economic harmonies' were pushed by some writers, while not including such persons as thieves among beneficent workers, was, nevertheless, astonishing. Herbert Spencer's advocacy of freedom of private coinage is well known, though any one familiar with 'Gresham's law' knows how chimerical such an institution would be. A still more astonishing suggestion is that which Molinari is reputed to have made at one time, namely, that even the police function of government should be left to private hands, that police corps should be simply voluntary vigilance committees, somewhat like the old-fashioned fire companies, and that rivalry between these companies would secure better service than that now obtained through government police!

If we stop to classify the social effects of individual actions, we shall find that they fall into three groups: (1) those actions which benefit the individual himself and have no effect upon others; (2) those actions which benefit the individual and at the same time benefit society; (3) those actions which benefit the individual while at the same time they injure society. It is the third group which the *laissez faire* doctrinaires have overlooked, and especially that part of the third group in which the injury to society outweighs the benefit to the individual. As Huxley said:[2]

> Suppose, however, for the sake of argument, that we accept the proposition that the functions of the state may be properly summed up in the one great negative commandment—'Thou shalt not allow any man to interfere with the liberty of any other man'—I am unable to see that the logical consequence is any such restriction of the power of government, as its supporters imply. If my next-door neighbor chooses to have his drains in such a state as to create a poisonous atmosphere, which I breathe at the risk of typhoid and diphtheria, he restricts my just freedom to live just as much as if he went about with a pistol threatening my life; if he is to be allowed to let his children go unvaccinated, he might as well be allowed to leave strychnine lozenges about in the way of mine; and if he brings them up untaught and untrained to earn their living, he is doing his best to restrict my freedom, by increasing the burden of taxation for the support of gaols and workhouses, which I have to pay.
>
> The higher the state of civilization, the more completely do the actions of one member of the social body influence all the rest, and the less possible is it for any one man to do a wrong thing without interfering, more or less, with the freedom of all his fellow citizens.

In the examples given by Huxley, the acts complained of are injurious not only to society, but to the individual. But even when the act of an individual is actually for his own benefit, it may not be for the benefit of society. The paradox that the intelligent actions of a million individuals, each attempting to better his condition, may result in making the aggregate condition of the million worse, is illustrated by considering the effect of individual action in the case of a burning building. When a theater is on fire, thousands of frantic individuals are struggling to get out. In the panic, it is doubtless to the best interest of any particular individual to struggle to get ahead of the others; if he does not, he is far more apt to be burned. And yet nothing is more certain than that the very intensity of such efforts in the aggregate defeat their own ends. The reason is that the effect of the effort is chiefly relative; so far as one pushes himself forward he pushes others backward.

Numerous examples exist of actions which benefit the individual but injure so-

[2] 'Life and Letters of Thomas H. Huxley,' by Leonard Huxley, Vol. I., pp. 384–5, Appleton, New York, 1900.

ciety, or benefit a part of society but injure society as a whole. Thus, the city of Chicago, in tapping the Great Lakes for its new sewerage system, has tended to influence the level of those lakes and thereby affect economically a large territory, including several states of the Union and also Canada. It has been estimated that the level of the lakes may be affected as much as six inches.

One reason for federal interference in irrigation is that the water supply is often controlled by citizens of one state, while the land belongs to another state or to the United States, and cooperation between the two is difficult to secure. Water, in the arid lands of the west, is a prime requisite, and without it the lands have no value. From one point, Mt. Union, in the Yellowstone Park, three rivers begin—the Missouri, the Columbia and the Colorado—flowing into the Gulf of Mexico, the Pacific and the Gulf of California, and through a large number of states and a vast extent of territory. The mutual interests of the riparian owners and those affected by irrigation could scarcely be adjusted merely through the play of individual interests.

Similarly, the act of one individual in destroying forests influences climate and water supply and thereby affects other individuals in distant parts. Where individuals in the community are allowed to seek their own interests the destruction of forests in some regions inevitably follows.

A like effect was seen a few years ago in the case of the seal dispute between the United States and Great Britain. The play of individual motive in this case tended to the actual extinction of seals, and could only be curbed by the mutual agreement of nations to prevent pelagic sealing.

Individual action can not be trusted to provide fire-proof or slow-burning construction as required in a crowded city; for the individual, although interested in protecting himself from his neighbors' fires, is not interested in protecting his neighbors from his own fires; hence the necessity and justification for city fire ordinances. Similarly, soft coal, in such cities as Denver, St. Louis and Pittsburg, constitutes a veritable nuisance to the entire city; and yet the individual factory owner is undoubtedly following his own best interest in not substituting hard coal or using expensive smoke-consumers. Such protective measures would redound greatly to the benefit of the community, but only slightly to his own benefit; hence the necessity and justification for smoke ordinances. Individual action would never give rise to a system of city parks, or even to any useful system of streets. And where parks exist, as in the case of Battery Park, New York, there is a constant tendency for those seeking their individual interests to encroach upon them. In Hartford and other cities certain parks have in this way gradually disappeared, much to the damage of the public.

In the cases mentioned, of a conflict between social and individual interests, legal restraints become necessary. But there are many examples in which, for one reason or another, legal restraints are impracticable. This is particularly true in cases where a number of nations are concerned. There can be no question, for instance, that the standing armies and great navies are an almost intolerable burden in Europe, and that their existence has tended to increase the cost of our own army and navy, three thousand miles away. Nevertheless, in the absence of any central international authority or mutual agreement to bring about disarmament, it must be confessed that it is to the interest of Germany or France each individually to keep up its military equipment to a level comparable to that of its neighbors. Yet the aggregate effect of international competition for military power is to cancel itself out; the advantages and disadvantages are purely relative. The nations are in a mad race each to excel the other. Their object being purely one of relative advantage, such advantage can be shifted from one to the other, but can not accrue to all. A general increase in relative advantage is a contradiction in terms, so that in the end the racers as a whole have only their labor for their pains.

An economic example of the same international character, and one which has received very scant attention, is found in the increase of the monetary metals. The production and distribution of gold and silver is the effect of individual action, each person seeking his own best interests. Yet the aggregate effect upon these individuals may be injurious. The injury referred to is not the imaginary injury of an 'unfavorable balance of trade' which was the bugbear of the mercantilists, but the exact opposite. A nation which increases its stock of money is always and necessarily a loser. This increase costs the nation either labor of mining or commodities sent out of the country, and for this cost there is no return whatever. To assume that the increase of money is itself a valuable return is to commit the fallacy of inflationism. Money is a very peculiar commodity. A general increase of other commodities is an advantage to society, but a general increase of money is not. The inflationist reasons that if a government can enrich one person by printing paper money and bestowing it upon him, it has only to do the same for everybody in order to enrich the nation. The paper-money delusion is too well understood to require comment. It is, however, not always perceived that precisely the same reasoning applies to all inflation, even the inflation which nature herself creates when she unlocks her hoards of buried treasure. The United States now has $33 of money per capita as against $22 a few years ago, but we are no better off on that account. The smaller amount of money is as useful in exchange as the larger amount. There are, of course, transition evils in contracting or expanding the currency, but so long as the price level remains constant or certain, the absolute number of dollars of the circulating medium is a matter of indifference. It follows that any effort expended in increasing the stock of money is wasted effort, an effort without a return. This waste is a necessary concomitant of monetary individualism.

A not dissimilar case, and one which is now causing much discussion, is that of railroad rates. Those who have examined the working of competition in railroad transportation recognize the fact that this competition is of the variety called 'cut-throat' competition, and that no stable or normal rates for transportation, under which capitalists will consent to invest in railway-building, can occur through such competition. Those who advocate competition as a cure for the evils of railroad rates do not appreciate the mechanics of the problem. The effect of competition is to bring rates down to the cost of operation; it leaves no provision for interest on capital sunk in the enterprise. If the cost of operation is one cent per ton-mile, whereas two cents are required to include enough revenue to pay interest on original cost, rates under competition will inevitably sink below the two-cent level to the one-cent level. For if we assume that the two-cent rate is for a moment the ruling rate, it is clear that it would pay any individual competitor to cut under that rate in order to divert traffic away from his rivals. But as soon as he cuts below it, all the others must do likewise or lose their traffic. This competition is merely self-defense, and yet its ultimate effect is to injure, not benefit, all of the roads who engage in it. It is *cut-throat* competition. In order that rates may be maintained at the two-cent instead of the one-cent level, either competition must be absent, or it must be partial or imperfect. In the actual railroad world competition is usually present at some points and absent at others. The consequence of this mixture of competition and monopoly is that rates will be determined differently for some points than for others, and this constitutes what is called local discrimination. In a régime where monopoly is present, discrimination, not only of this local character, but discrimination as to persons and as to commodities carried, is a natural and inevitable result. It is not, of course, a desirable result; but it is no more undesirable than is the cut-throat competition which is the other horn of the dilemma. This cut-throat competition discourages the investment of capital in new railroads, and the shippers and consumers must in the end suffer. This dilemma between the evils of monopoly and of competition leads to governmental regulation, though the efficacy of this remedy is not all that could be desired. It is not our purpose to discuss the best solution of so difficult a question. We are merely concerned in pointing out that this railroad-rate problem is partly due to cut-throat competition and that cut-throat competition is one more example of the suicidal effects of blindly following individual self-interest.

Numerous other examples might be given; we shall, however, content ourselves with one. As John Rae has pointed out, there exists a species of subtle competition in private expenditure, due to social rivalry—the desire for distinction through wealth. It has frequently been remarked among ladies' social clubs which begin with simple entertainments, that each successive hostess attempts, almost unconsciously, to surpass her predecessor in the entertainment offered. Beginning with tea and cake, the club ends with elaborate and expensive collations, until it produces a heavy drain upon the resources of its members. In precisely the same way, on a larger scale, there is laid a heavy burden upon us all through the social rivalry of individuals. If we study the history of Newport or similar fashionable resorts, we find that social racing has gradually resulted in setting a pace which only the most wealthy can keep up, and that even for them expenditure represents cost rather than satisfaction. This cost often takes the form of producing fictitious values on articles merely because they are 'exclusive.' As John Rae says:[3]

> A dish of nightingale's brains could scarcely be a very delicious morsel, yet Adam Smith quotes from Pliny the price paid for a single nightingale as about £66. According to Suetonius, no meal cost Vitellius less than £2,000 * * * Thus Adam Smith reckons the cost of some cushions of a particular sort used to lean on at table, at £30,000.

Nor do we need to draw our examples from ancient Rome. The 'History of Luxury' by Baudrillart will show the tendency to produce luxury out of social rivalry in all ages. It was only recently that an American in London gave a dinner party which was said to have cost $8,000. The table was placed in a large Venetian gondola set in the midst of an artificial lake, while in a smaller gondola near by a band was stationed.

Much has been said of late about the importance of living the simple life, but so far as I know there has been no analysis to show why it is not lived. This analysis would reveal that the failure to live it is due to a kind of unconscious cut-throat competition in fashionable society. When San Francisco was destroyed by earthquake and fire, much comment was made upon the fact that many did not feel their losses as

[3] 'Sociological Theory of Capital,' by John Rae, ed. by C. W. Mixter, MacMillan, 1905, p. 247.

much as might have been anticipated. One reason for this result is doubtless found in the fact that the losses were not relative. Had a single individual found himself suddenly reduced from a palace to a tent, his sense of loss and discomfiture would have been great. He could no longer return social entertainment among his former associates; he would feel 'out of it' and envy would gnaw at his breast. But after the San Francisco catastrophe there was little place for envy; all were in the same boat. There was no relative loss, there was only the absolute loss of creature comforts, and strange as it may seem to one who has not considered it, the absolute loss is the smaller of the two.

It is hard to overestimate the tax which is laid upon society through social racing. We are not conscious of this weight, because, like the weight of the atmosphere, it is always pressing upon us. The New York business man buys a silk hat as a matter of course. He does not think of its cost as a tax laid on him by society. He is satisfied because the hat fills a want, and he does not consider how that want originated. It is only when the tax varies by change of place, just as when atmospheric pressure varies by ascending a mountain, that he is at all aware of its existence. If he removes to a smaller town where social racing is less intense and the leaders in the race are unable to set so high a pace, he finds the tall hat no longer *de rigueur*. He drops off this and numerous other expenses and feels himself that much better off. A gentleman recently refused a salary of $7,000 in New York, preferring $4,000 in a smaller town, feeling that he could buy no more real satisfaction with the former than with the latter. The extra $3,000 meant simply that it would cost more to keep up with his neighbors.

The burden of social racing is laid not only on the rich but upon all classes. A milliner in New Haven recently thought to avoid competing with existing fashionable millinery establishments by catering to the trade of shop girls. To his surprise, he found that the tyranny of fashion was quite as strong among them. He attempted to put on sale a large number of $5 and $6 hats, but found great difficulty in disposing of them, whereas the few $15 and $16 hats met with a very ready sale. The shop girls wanted these hats to 'be in the swim.' Recently in France a whole family committed suicide because they had lost the capital which they considered necessary to keep their social position.

Many ingenious arguments have been made to justify luxury and in some of them there may lie truth. The fact that luxurious expenditure can be so readily cut down in hard times provides a sort of buffer against want and famine. The relations of luxury to the growth of population deserve careful study. But whatever the indirect benefits of luxury, certain it is that it forms a tax upon society, and a heavy one. It seems also true that where luxury is greatest civilization decays.

Were there more space we might discuss remedies for this social racing; but we must content ourselves with merely describing the phenomenon. It exemplifies the manner in which the self-seeking of each may create a burden for all.

From this and the other examples which have been reviewed we see that the mechanics of individualism is not so simple as the individualists have assumed. The old individualism requires two corrections: first, the individual may often be interfered with in his own interest, because either of his ignorance or his lack of self-control; secondly, even when an individual can be trusted to follow his own best interests, it can not be assumed that he will thereby best serve the interests of society. A recognition of these two facts is essential not only to clear thinking, but as preliminary to any practical solution of the great problems of human betterment. We are doubtless to-day in danger of too much socialistic experimentation; but nothing can be gained and much may be lost by ignoring or condoning the opposite evils of individualism. In fact, the menace of socialism can best be met if we understand and acknowledge the evils which it is intended to remedy. The preliminary to remedy is diagnosis, and an accurate diagnosis will save us from the error of both extremes—the extreme, on the one hand, of an overdose of socialism, and the extreme, on the other hand, of omitting all medication whatever.

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SCIENTIFIC BOOKS

Sociology and Social Progress: A Handbook for Students of Sociology. Compiled by THOMAS NIXON CARVER, Ph.D., LL.D., David A. Wells Professor of Political Economy in Harvard University. Boston, Ginn and Company [1906]. Pp. vi + 810; 8°. List price, $2.75; mailing price, $2.95.

This is a timely and valuable book. In these days when social questions are attracting the attention of all, even the scientific specialists, and when an undigested mass of contemporary literature is being poured forth upon the public wholly incapable of appraising it, it is of the greatest importance that the utterances of the masters of thought, science and literature bearing on the subject should be made accessible to all as guides to public judgment. To do this is the purpose of this volume, and even a partial enumeration of the authors and works that have been drawn upon is sufficient to indicate the value of the compilation. The most important are: Comte's 'Positive Philosophy' (Harriet Martineau's English condensation), Buckle's 'History of Civilization,' Darwin's 'Descent of Man,' Adam Smith's 'Theory of Moral Sentiments,' Bagehot's 'Physics and Politics,' Fiske's 'Outlines of Cosmic Philosophy,' Herbert Spencer's 'Data of Ethics,' Kidd's 'Social Evolution,' Tarde's 'Imitation,' Galton's 'Hereditary Genius,' Machiavelli's 'Prince,' Aristotle's 'Politics.'

When we remember that about two thirds of the space is devoted to these works and a fair share to others ranking second only to these, we can well pardon the introduction of a number of lesser works and even some quite insignificant ones.

The selections from large works, which is no easy task, are judiciously made. For example, the three most important subjects treated by Buckle, viz., the influence of physical laws on society, the rôle of intellectual development, and the influence of religion, literature and government, are introduced here without abridgment. Darwin's chapters on sexual selection in relation to man are given in full with the exception of the scientific details, so that it is quite readable. And so of the rest. Those who read these works in their youth and retain only a vague impression of them, have an opportunity here to refresh their minds with the cream of them, and those who never read them at all can gain from this digest a fairly adequate idea of them.

But Dr. Carver has intended that the book, as its title implies, should be something more than a mere compilation. In the first place, he has supplied an introduction to it of his own, in which he sets forth as clearly as has ever been done the true scope and method of sociology. His treatment is thoroughly sane. He is an economist of the modern school which has arisen from the recent revised definition of value, and which brings the great sciences of economics and sociology into sympathetic touch with each other. If he lays somewhat undue stress on social progress, he only does what others, including the present reviewer, have done before they had devoted themselves to a serious study of the conditions of social order. The doctrine which he specially emphasizes as his own, and which he had earlier set forth, is expressed in these words:

Every great historical epoch and every variety of social organization must be explained on the basis of factors and forces now at work, and which the student may study at first hand.

This is now called sociological uniformitarianism. It was strongly hinted at by Sir Charles Lyell himself ('Principles of Geology,' eleventh edition, Vol. I., p. 167). It is called by Gumplowicz 'the eternal uniformity (*Wesensgleichheit*) of social processes' ('Rassenkampf,' p. 172), and is discussed at length by him. It has been insisted upon by Bernès and Regnano in Europe and by Ross and Small in America, and was applied by Schleicher to language.

Conformably to this philosophical introduction, Dr. Carver has undertaken to classify his materials under certain logical heads. The late Dr. G. Brown Goode described a museum as 'a collection of labels illustrated by specimens.' The materials of this work have about the same importance relatively to these heads as do the specimens of a museum to the labels. He divides the matter of the book into three parts, the first relating to the nature, scope and method of sociology, the second to its bearing on social progress, while the third part embraces the various factors of social progress, which he still further subdivides into physical and biological, psychical, social and economic, and political and legal.

This classification may have value for some minds, but doubtless chiefly for that of the compiler, and the ordinary reader will not generally know, and will care less, where he is in the scheme when he is reading any of the interesting essays that the work contains. It would have been just as well to arrange them in the alphabetical order of the authors, or still better in the chronological order of the works.

As already said, the authors cited are nearly all either famous or of a high order, and some of the essays whose authors are either contemporary or somewhat less well known are among the best selections. The essay entitled 'War and Economics in History and Theory,' by Edward Van Dyke Robinson, certainly answers this description. There are, however, a few of the articles whose appearance in this roll of honor is matter for regret. Only one such need be mentioned, viz., Drummond's Struggle for the Life of Others, from his book 'The Ascent of Man,' 1894, the very title of which was plagiarized from the address of Dr. Frank Baker, as president of the Anthropological Section of the American Association for the Advancement of Science at the Indianapolis meeting, in August, 1890, published in the *Proceedings*, Vol. XXXIX., p. 351, and also in the *American Anthropologist* for October, 1890, Vol. III., No. 4, p. 297, and with which Drummond must have been familiar. But this might have been pardoned if the book itself, or most of what is true in it, were not a plagiarism from Haeckel and other authors. Even this offense, however, is less grave than the utterly unscientific and mainly false attitude of the author in the application and interpretation of his facts.

From the standpoint of book-making this volume has its defects. Not to mention its ugly, unesthetic style of binding, it is one of those books in which the user is always lost and constantly compelled to revert to the contents to find what he wants. This could easily have been remedied by head-lines showing 'who is talking' on any page. But such things are 'trifles light as air' by the side of the sterling merits of the work.

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VARIATIONS OF THE BONES OF THE FACE[1]

This work (which should have been reviewed sooner) is the continuation of Professor Le Double's great undertaking which is to give us a complete account of the variations of the human skeleton. His last work, which was noticed in these pages, treated of the variations of the cranial bones. This one, therefore completes the head. In the preface the author gives some account of his labors and maintains that none of his propositions

[1] 'Traité des Variations des Os de la Face de l'Homme et leur signification au point de vue de l'Anthropologie Zoologique,' par M le Dr. A. F. Le Double, Paris, Vigot Frères, 1906.

has been refuted. He dwells very justly on the correlation of malformation (or perhaps merely variation) and disease. In his conclusions at the end of the work there is much that is interesting. He is severe on Lombroso. In fact, it is easy enough to demolish the fantastic exaggerations of that school; but we are not yet convinced that the idea of a criminal type is absurd.

Dr. Le Double writes as follows concerning variation: "In the actual conditions of our methods of investigation no truly scientific theory of heredity seems to me possible. There is not one of the explanations proposed to us that is not to some extent a matter of discussion." This is true and well said. We incline to think that he is rather too ready to accept the very general view of atavism. If a peculiarity is inherited we have a right to ask for the line of descent, and this line must not diverge from the lines used to explain other peculiarities. In point of fact this has not yet been shown. On the contrary, the threads tend to get snarled very badly.

We had not meant, however, to go into any discussion. Ours is the pleasanter purpose of calling attention to the thoroughness of the analysis and the wealth of material to be found in this book. None but a trained anatomist has any suspicion of the vast number of variations found in the bones of the face. We cannot even begin to enumerate them. Suffice it to say that the book is indispensable to the anatomist working in this department of science. The opening chapter on the nasal bones is a very attractive one. Then comes the chapter on the lachrymal, from which we would extract a table as a very striking illustration of the necessity of large series for statistics. The table shows the cases of absence of this bone seen by different observers:

Krause,	8 in	100 skulls.
Macalister,	2 in	150 skulls.
Bianchi,	5 in	350 skulls.
Merkel and Kallius,	1 in	1,000 skulls.
Zabel,	3 in	200 skulls.
Adachi,	6 in	121 skulls.
Le Double,	1 in	100 skulls.

It is to be noted that Adachi's observations were on Japanese. This raises the further question of the effect of race on variation. The whole subject is a very fascinating one.

THOMAS DWIGHT

Amerikanisches Hochschulwesen. Eindrücke und Betrachtungen. By Dr. W. BÖTTGER, Privatdozent at the University of Leipsic. Pp. 70. Leipsic, Wilhelm Engelmann. 1906.

This little book is a critical comparison of the conditions in American and German universities and schools for higher education, and is written especially with the aim of presenting to German readers that which is of merit in American institutions. The author's experience was gained by one year's residence in America, during which time he occupied the station of research associate at the Massachusetts Institute of Technology. By nature and training a keen observer and student of conditions, he has gained a remarkably thorough appreciation of the methods and ideals of American higher education as typified in the Massachusetts Institute of Technology and Harvard University. As he himself emphasizes, he has not immediately embodied his ideas in print, but has first allowed them to mature during several months after his return to his work in his native country. He thus can give a fairer survey of the subject and not one distorted by the accidental being mistaken for the typical, as has been done by some who have essayed to express themselves upon this subject.

America is, according to Dr. Böttger, the land of experimentation in educational matters; it is only during the last thirty years that the American universities have developed to their present importance. Their life is, therefore, younger and thus more energetic.

The length of time occupied in the preparatory schools by the youth of both countries is about equal—an average of twelve years; yet the graduate of the American preparatory school has only the training requisite for entrance to the highest class of the German 'gymnasium.' The reason for this is, naturally, that the American boy is not so severely pressed in his studies, whereby more opportunity is given for his very essential physical development.

The first years of the American college are still in the nature of preparation for his special line of study; thus the transition from a condition of dependence in the preparatory school to one of independence in the higher school is a gradual one. Much stress is laid by the author upon the American system of supervision of the students' work as compared with the German system of complete freedom. The advantages of the American method are evident—that many students are guarded against mistakes which would entail serious consequences. Contrasted to this, the German system suddenly throws the student wholly upon his own responsibility—upon entrance into the university his choice of studies is unrestricted, and he takes no examination until his final one, when he deems himself prepared to try for the degree of 'doctor' from the university. The German contends that his system possesses the advantage that those who do not make the proper use of the freedom accorded to them will sooner or later fall out, so that only those will reach the goal who properly understand the privileges and duties of the students' freedom. This argument, however, presupposes that the student already has that which he must first acquire. In America, the same goal is striven for, but with fewer losses, in that the transition to the condition of independence is made gradual.

The difference in the ideals of education in the two countries may be summed up in the following: In America the aim of the higher education is to increase the efficiency of the average man; while in Germany the stress is laid upon bringing the best to the highest development. The author seems to reach the conclusion that due precaution is exercised in America to prevent the invasion of the less diligent into the ranks of the learned professions, while, at the same time, pains are taken to develop the less gifted, who are, on this account, the more in need of education.

The author discusses at some length the matters of state and private schools, the organization of instruction, the cost of instruction and scholarships, athletics and the love and loyalty of the alumni of a school for their alma mater. Many matters of interest are treated which can not be touched upon in this review. The author has dealt very lightly with our failings and has devoted himself chiefly to pointing out to his countrymen, with whom education is an established science, that which has been attained in a country where education is still in the state of development. It is a matter of interest to learn those points in our system which are deemed commendable, or worthy of study, by a student trained in the old world educational ideals. This work is of additional interest as treating the subject especially from the viewpoint of education in the exact natural sciences; previous writers in the main having treated the subject in its relation to general culture.

Arthur A. Blanchard

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Geology for November–December has for its frontispiece a reproduction of a photograph of the late Professor Israel C. Russel. The first article is a sketch of his life by G. K. Gilbert. Following this is an article by James Geikie, "On the so-called 'Postglacial Formations' of Scotland." In this are discussed geographical and climatic changes, from evidence gathered in the Scottish mountains. As the most representative deposits known as 'postglacial,' he speaks of 'raised beaches, estuarine and fluviatile terraces, lacustrine alluvia and peat mosses.' He considers the term 'postglacial' misleading, because glacial conditions disappeared from different regions at widely different times. The following succession of events is given: (1) after the disappearance of district ice-sheets and mountain-valley glaciers, the sea retreated considerably, and the climate became milder; (2) subsidence and return to cold climate; (3) retreat of sea beyond present coast line and return to dry genial conditions; (4) partial subsidence with change of climate to cold and wet; (5) final retreat of sea to present level. The next article is on 'The Three Paleozoic Ice Ages of South Africa,' by Ernest H. L. Schwarz. Evidence is offered of three ice ages at widely separated times. The Permian glaciation is already fairly well known. Of the other two, one is probably Devonian, the other Archean. The field evidence is said to be convincing. 'The Texture of Igneous Rocks' is taken up by Whitman Cross, J. P. Iddings, L. V. Pirsson and H. S. Washington. They attempt to make the classification according to textures more systematic, and to get rid of the prevalent vagueness and inexactness. Terms with exact meanings are proposed and the whole put into systematic shape. 'Natural Mounds' is the title of an article by Maurius R. Campbell. The mounds occur on flat surfaces and are low and broad, and very symmetrical. They vary from 10 to 140 feet in diameter and from a few inches to 5 or 6 feet in height. They are of very wide occurrence. Various hypotheses of origin have been offered, but many of them have little foundation. Of eleven possible modes of origin the writer eliminates all but the one which ascribes them to burrowing animals—ants or rodents. The subject of 'Rock Folds due to Weathering' is taken up by the same author. He shows how great the expansion is in ordinary weathering and how this often forces the surface rocks to buckle. 'The Geology of the Lower Amazon Region' is based upon Katzer's work and that of some others and was written by Charles Schuchert. As the title indicates, it is a discussion of the general geology and stratigraphy of the region and adds considerably to our knowledge of Brazil. The Devonian and Carboniferous are discussed in considerable detail, and lists of fossils given for correlation. The last article is by George Davis Louderback on 'The Relation of Radioactivity to Vulcanism.' The important bearing of the recently developed knowledge of radioactivity on the problems of the geologist is shown and discussed in some detail. The writer believes that while much of the interior heat of the earth may be explained by radioactivity, the special phenomena of volcanoes may not be so explained.

The Journal of Comparative Neurology and Psychology for November contains the following articles: 'The Mode of Connection of the Medullated Nerve Fiber with its Cell Body,' by Oliver S. Strong. A plate is given illustrating the form of the axone between the cell body and the medullary sheath. 'On the Centers for Taste and Touch in the Medulla Oblongata of Fishes,' by C. Judson Herrick. Fishes like the catfish which detect their food by the simultaneous action of both taste and touch in the barblets and outer skin offer an interesting problem in the examination of the centers of correlation within the brain for these diverse sensory tracts coming from the same cutaneous areas. The analysis of these centers in the medulla oblongata of Ameiurus shows that the gustatory nerves from visceral surfaces effect the usual secondary connections with the visceral musculature, but those gustatory nerves which come from the outer skin make their secondary connections with the primary tactile centers in the funicular nuclei, so that a common efferent path from the latter correlation center serves for both senses. 'Modifiability of Behavior in *Hydroides dianthus* V,' by Ada Watterson Yerkes. The experiments show that this annelid worm readily learns by experience. Two short papers on the 'Behavior of *Gonionemus*' are contributed by Max Morse and Robert M. Yerkes respectively. An editorial on the relation of the newer work of the American school of comparative neurologists to human neurology and a few book reviews complete the number.

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 182d meeting of the society on November 14, Mr. F. E. Wright presented informally the results of a comparative study of various methods in use for determining the relative quantities of mineral constituents in rocks.

The meeting was devoted to an account of the Tenth International Geologic Congress, held in Mexico City, September 7 to 14, 1906.

Mr. GEORGE OTIS SMITH described 'The Excursion before the Congress.'

Mr. S. F. EMMONS: 'The Excursion to Jorullo.'

Mr. G. F. BECKER: 'The Sessions in Mexico City.'

Mr. T. W. STANTON: 'The Excursions after the Congress.'

At the 183d meeting of the society, held on November 28, Mr. Geo. H. Ashley exhibited a diagram illustrating the occurrence of contorted shales lying below a massive sandstone in relatively flat-lying rocks, suggesting that this effect may have been produced by the action of gravity subsequent to erosion. Mr. M. R. Campbell and Mr. G. C. Martin expressed the opinion that such structures are more likely to have originated as a result of folding prior to the deposition of the overlying strata.

Mr. Geo. B. Richardson exhibited specimens of fossil bones collected in October, 1906, at Rous' gravel pit in the northern part of El Paso, Texas. The remains, comprising teeth of a mammoth and a horse and the jaw bones of a tapir, had been secured and identified generically by Mr. Walter Koch, who presented them to the speaker. They have since been determined by Mr. J. W. Gidley of the U. S. National Museum to represent the following forms: *Elephas columbi, Equus complicatus* and *Tapiris* (*haysii?*). The bones were found in cross-bedded sand and gravel at two horizons, thirty and sixty feet below the surface of a hill, an erosion outlier of the bolson plain which lies at the base of the Franklin Mountains.

The find is of particular interest because it indicates the age of at least part of a great mass of unconsolidated beds adjacent to the valley of the Rio Grande, and furnishes another link in the long chain of evidence of a moist climate during the early Quaternary in the Cordilleran region. Deep well records show the presence of over 2,200 feet of unconsolidated material in the vicinity of El Paso, but whether all of this is Quaternary or the basal part is Tertiary remains to be determined.

In the discussion Mr. W. T. Lee mentioned the occurrence of the teeth of a Pleistocene species of *Equus* thirty miles northwest of El Paso, in beds formerly called Miocene, and Mr. C. A. Fisher reported similar fossils from the Pecos Valley.

Regular Program

The Geological Map of North America: Mr. Bailey Willis.

This map will be issued with the Comptes Rendus of the International Geological Congress held in Mexico City, September 7 to 14, 1906. It is further planned by the U. S. Geological Survey to issue a revised edition with descriptive text.

The Elevated Beaches of Labrador: M. L. Fuller.

The speaker reviewed the work of Bell, Packard, Low and Daly on the raised beaches and terraces of the Labrador coast, noting the doubt which a recent trip threw upon certain postulations of change of level based on some of the beaches and terraces. Some of the sea caves appear to be the result of weathering agencies rather than of marine erosion, while a great majority of the rock benches are manifestly due to differential subaerial erosion controlled by normal and concentric jointing and modified by glaciation. The preponderance of the beaches along the sides of the fiord-like inlets, and their eastward slope in certain instances, lead the speaker to consider some of them, at least, to be possibly due to causes other than marine erosion, representing, for instance, accumulations of the morainal terrace type along the sides of valley ice lobes during the closing stages of the last ice invasion. The method employed by Daly—the determination of the lowest limit of undisturbed glacial erratics—is recommended to future investigators as a more reliable means of determining changes of level than observations on the so-called beaches.

The Colorado Desert and Salton Sea. M. R. Campbell.

Discussed from the standpoint of engineering by Mr. E. W. Parker.

At the 184th meeting of the society, held on December 19, Mr. George P. Merrill, the retiring president, presented an address, illustrated by lantern slides, entitled "The Composition and Structure of Meteorites compared with those of Terrestrial Rocks."

At the close of Mr. Merrill's address the fourteenth annual meeting of the society was held for the purpose of electing officers, and the following officers were elected for the ensuing year.

President—Mr. Waldemar Lindgren.

Vice-Presidents—Mr. M. R. Campbell, and A. H. Brooks.

Secretaries—Messrs. F. E. Wright and Ralph Arnold.

Treasurer—Mr. Joseph A. Taff.

Members at Large of the Council—F. L. Ransome, T. W. Stanton, George H. Ashley, E. O. Ulrich, George B. Richardson.

ARTHUR C. SPENCER,
Secretary

DISCUSSION AND CORRESPONDENCE

THE CARNEGIE FOUNDATION

TO THE EDITOR OF SCIENCE: I am sorry to conclude, after a perusal of the last number of SCIENCE that has reached me (October 26, 1906) that everything is not going just right everywhere just now. My grief is greatly softened, however, by the fact that the columns of that always instructive and sometimes entertaining journal seem to be pretty 'wide-open' to people who 'object.' I can hardly tell what a boon this will be to some of us whose activities are now mostly restricted to some form of mild and, I trust, inoffensive 'kicking.'

I have been waiting for some months for somebody to enter a protest against the phraseology of the published notices of the awards of pensions from the 'Carnegie Foundation for the Advancement of Teaching.' As the same 'peculiar wording' has been used in every such notice that I have seen, I infer that it must have been purposely selected and formally adopted by some one. 'The retirement of Professor ——— *is made possible*' (the italics are mine) by the action of the trustees of the Carnegie Fund, etc.

When I first read this phrase in the announcement of Dr. Harris's retirement from the Bureau of Education I thought it only an amusing oversight, a bit of ill-considered composition, or a case of 'the types made us say'; but when I read it later of Professor Morley and again of Professor Dolbear (I think) I saw that some other than the printer's devil was having a hand in it. For in my humble and far-away judgment the italicized words, as applied to the retirements named above, are about the most infelicitous that a diabolical ingenuity could select. To one who doesn't know the man or his work, or the big place he fills in the 'educational heart' of the great republic, the words clearly imply that the president, and everybody else, for that matter, have been wanting for years to retire Dr. Harris, but that it was impossible to get rid of him until the Carnegie fund came to the rescue. One must conclude that through the same beneficent charity Western Reserve University is now relieved of Professor Morley, the most distinguished member of its faculty; and that Tufts breathes again, free from the incubus of Dolbear. I am not holding the trustees of the Carnegie fund responsible for these words nor am I questioning their singular appropriateness in *the majority of cases with which the trustees will have to deal*, but to the inventor of the phrase, whoever he may be, I submit that there ought to be an alternative, for use when the above seems to be not just the right thing to say—something that will not mislead the intelligent but uninformed reader.

About a quarter of a century ago it was my pleasure to sit through several sessions of the physical section of the British Association, presided over by Lord Kelvin. Following the usual English practise, he always said a few words after the presentation of each paper, conveying the thanks of the section to its author. The custom is often purely formal and generally the words have little real meaning, but it was delightful to see that the distinguished chairman, following his natural leaning towards 'precision,' had devised two expressions, quite similar in form but, to a reflective listener, very different in meaning, which he made use of as judgment dictated, in the discharge of this part of his duty as presiding officer. In one case he would say, "I am sure the section will join me in thanking Mr. ——— for his most interesting and important communication on this subject," while in another it would be, "I am sure the section will join me in thanking Mr. ——— for his communication on this *most interesting and important subject*."

Can not the editor of the Carnegie pension announcements evolve something like this?—and then everybody will be happy.

T. C. M.

DRESDEN,
November 12, 1906

IS THERE DETERMINATE VARIATION?

PROFESSOR KELLOGG has presented some very interesting facts and arguments regarding the variation of *Diabrotica soror,* under the above title (SCIENCE, November 16, p. 621); but I venture to think that the dilemma has more horns than he has credited to it.

He shows (Fig. 7) that the Sierra Morena collection contains a very large proportion of fused-spots specimens. This material is from a locality about three miles from Stanford University campus. Now why is it not possible that a distinct Sierra Morena strain exists (perhaps the prevalent form of higher levels throughout the region), and that this has in recent years invaded the campus of Stanford University? If this is likely, or even possible, the whole matter may assume a different aspect.

If there exist different strains of *D. soror,* some free-spotted and some with a prevalence of fused spots, it is altogether likely that they differ in other characters, *e. g.,* power of resistance to particular forms of disease. If, in certain parts of the world, people with light complexions have supplanted those with dark, we are not obliged to assume that complexion is in itself a common cause of survival, or else abandon the idea of selection. We know, on the contrary, that the familiar color-characteristics accompany many others, some of which, singly or in combination, may have a high selection-value.

T. D. A. COCKERELL

UNIVERSITY OF COLORADO,
BOULDER, Colo.,
November 18, 1906

SPECIAL ARTICLES

THE ADVANCING MALASPINA GLACIER[1]

THE Malaspina Glacier lies at the seaward base of Mount St. Elias in Alaska, where a number of large valley glaciers descend from the St. Elias Alps, and coalesce at the mountain base to form a great ice plateau some fifteen hundred square miles in area. The characteristics of this piedmont ice plateau, the Malaspina Glacier, have been made known to us mainly through the splendid work and descriptions of the late Professor Russell.[2]

One of the striking features of the Malaspina Glacier, as described by Russell and others, was its smooth surface due to the general absence of crevassing. So well developed was this characteristic that the glacier has served as a highway of travel for a number of expeditions having for their object the ascent of Mount St. Elias. Twice Russell himself used the Malaspina for this purpose; Prince Luigi Amedeo, Duke of the Abruzzi, made use of the same highway on his successful ascent of St. Elias; and Mr. H. G. Bryant also traveled across the glacier toward Mount St. Elias. Each of these expeditions crossed the Malaspina Glacier on the side toward Yakutat Bay where my studies have been carried on. Other expeditions have crossed the glacier further west. In these expeditions it was found possible not only to move freely over the ice, but also to draw loaded sleds across it.

A second characteristic feature of the Malaspina Glacier is the presence of a moraine-veneered margin, developed by melting of the ice and concentration of the included rock fragments at the surface. In places, this veneer of moraine soil is so thick, and the ice under it so stagnant, that forests have developed upon it.

In the summer of 1905 I looked down upon the Malaspina Glacier from several high points in Yakutat Bay; and late in August

[1] Published by permission of the director of the United States Geological Survey. I wish to acknowledge in this work the assistance of Lawrence Martin and B. S. Butler, in 1905, and of the latter, together with O. VonEngeln, J. L. Rich, and R. R. Powers, in 1906. A paper, with photographs and map illustrating the changes described below, will appear in the forthcoming number of the *Bulletin* of the Geographical Society of Philadelphia.

[2] *Nat. Geographic Mag.,* Vol. III., 1891, pp. 53–204: Thirteenth Annual Report U. S. Geol. Survey, 1891–2, Part II., 1893, pp. 1–91.

two of my associates, Lawrence Martin and B. S. Butler, made an expedition to the western margin of the Floral Hills, where they had a clear view, and took photographs of the eastern margin of the Malaspina Glacier. At that time no unusual conditions were noticed.

In the summer of 1906, I made a second expedition to Alaska, having for its object the crossing of the Malaspina Glacier from east to west. It was my purpose to follow Russell's route as far as the Marvine Glacier, crossing it about where he did, then cross the route of Prince Luigi and Mr. Bryant, and proceed thence westward to the western margin if possible, in the meantime making side trips up the tributary glaciers and to various parts of the margin of the Malaspina.

This plan was most unexpectedly interfered with. The Marvine Glacier, which in 1890 Professor Russell crossed with ease, carrying his entire mountain-climbing outfit, was transformed from a smooth ice surface to a labyrinth of crevasses, across which we found it totally impossible to carry supplies. Even the passage of the glacier unburdened would have been a task which no one short of expert Alpinists could attempt; and I doubt very much if even they could possibly cross the glacier from side to side.

The evidence of our photographs of 1905, and of the views which we had from the neighboring mountains, clearly demonstrate that this remarkable change in the glacier has occurred since August, 1905. In 1906 we passed along the eastern margin of the Malaspina Glacier from the point of emergence of the Marvine Glacier from its mountain valley west of Blossom Island down the Kwik Valley to the sea. Thence southwestward along the shores of Yakutat Bay to Point Manby we saw the ice front from a boat. During his retreat in 1891, Professor Russell traveled freely from Point Manby to the Kwik River along this seaward margin of the glacier. Now it is impassably crevassed throughout the entire distance.

As we passed along the margin of the glacier, we found abundant evidence that the forward movement which has broken it is still in progress. The ice was even then being broken into blocks; ice fragments were falling from its face; moraine was tumbling down the front and into the crevasses; new streams were emerging from the moraine-covered front; and during the interval of a month which elapsed between our traverse up the glacier margin and our return, there were numerous changes in detail.

In places where the moraine on the glacier was occupied by forest, the crevassing had greatly disturbed the tree growth. The trees, some of which must have been at least fifty years old, stood at all angles and were frequently seen to fall down the ice front and into the crevasses. That the ice movement was wholly of the present season was proved by the fact that all the overturned trees had developed leaves before the disturbance affected them.

By this advance of the Marvine tributary to the Malaspina Glacier, the eastern portion of this piedmont glacier has been transformed to a sea of crevasses. The crevassed area starts with the width of the Marvine Glacier at its emergence from the mountain valley, but expands toward the sea into a bulb-shaped area of crevasses which includes the entire portion of the Malaspina Glacier bordering upon Yakutat Bay. Thus the crevassed area cuts across the routes followed by Bryant and Prince Luigi, so that at the present time a journey to Mount St. Elias from their point of starting (the Osar River) is out of the question.

We were unable to closely examine the tributaries of the Malaspina Glacier west of the Marvine; but from a distant view it is evident that no such disturbance as that caused by the advancing Marvine Glacier is present to the westward, as far as one could see. The Seward Glacier, next west of the Marvine, is, however, badly crevassed near the point of its emergence from its mountain valley; and one of my party, Benno Alexander, who was with Prince Luigi, asserts that it is far more broken than in 1897. Whether this represents the beginning of an advance similar to that of the Marvine can not now be stated.

The Hayden Glacier, which joins the Malaspina just east of the Marvine, shows no notable change from its condition in 1905; nor does the Lucia Glacier, which is the next glacier east of the Hayden, and which at the present time does not quite join the Malaspina. The glacier next east of the Lucia, however, has entirely changed its condition since we saw and crossed it in 1905. This glacier, the Atrevida, was crossed by Professor Russell on his first expedition in 1890. In August, 1905, we made several trips to its margin and one out upon it, while Messrs. Martin and Butler crossed it from side to side on their way to the Floral Hills. Where we crossed the Atrevida, in 1905, it was an undulating, moraine-veneered, apparently nearly stagnant glacier which could be crossed with great ease, and at all points, entirely without danger from crevasses. It was our intention to enter upon the Malaspina Glacier along this route; but, to our astonishment, we found that in ten months, between August, 1905, and June, 1906, the conditions had totally changed. We were unable to ascend even the margin in 1906, and views from above the glacier, on both the east and west sides, prove it to be broken from side to side, and from far up its mountain valley down nearly to its terminus in the moraine-veneered, alder-covered, bulb-shaped expansion beyond the mountain front. In its lower portion the Atrevida coalesces with the Lucia Glacier and thus we have the anomaly of two glaciers side by side, one of which shows no change, while the other is absolutely different from its condition only ten months before.

There are a score or more notable glaciers in Yakutat Bay, all but two of which are essentially as they were in 1905. Of these two exceptions one lies in a small mountain valley immediately north of the Turner Glacier. In 1905, this small valley glacier was apparently stagnant at its end, which was approximately a quarter of a mile back from the coast. Ten months later, in June, 1906, this unnamed glacier, which we will call the Haenke Glacier, was found to have advanced well out into the fiord, and to have joined the ice cliff of the Turner Glacier. By this addition the ice cliff of Turner Glacier is lengthened fully a mile. The Haenke Glacier was also profoundly crevassed during its forward rush. There is a glacier similar to the Haenke in the valley next north of it, and less than a mile distant, which shows no change from its condition in 1905.

The great Hubbard Glacier is the next in the fiord and it shows no change; but just east of it is a small glacier, which we named the Orange Glacier, in 1905, and over whose bulb-shaped terminus we walked freely, paying special attention to it because of an interesting series of concentric colored moraines which then covered it. We also walked up this glacier some five or six miles in a half day, and in that distance found no undue crevassing. In June, 1906, on the other hand, this glacier was crevassed from as far up the valley as we could see (about the point we reached in 1905), well down into its bulb-shaped terminus, which was shoved up much higher than in the previous summer. Its surface is so broken that in 1906 we were not able to even approach the colored moraine area over which we walked so easily in 1905. In the place of this moraine covered area of 1905 was clear, crevassed and pinnacled ice.

These facts prove that some of the glaciers of the Yakutat Bay region have been subjected to a paroxysmal thrust of great force. The thrust has been sufficient to break the glaciers not only in their mountain valleys, but also far down in their hitherto nearly stagnant bulb-shaped termini. In the case of the Marvine Glacier, the crevassing extends a distance of at least twelve or fifteen miles. Other glaciers, even those that are the nearest neighbors to the advancing tongues, have not yet been subjected to this forward thrust.

This is not the place to fully discuss the cause of this striking and unique change in the glaciers. Suffice it to say, that in seeking for a cause, we have felt obliged to discard the operation of normal climatic variations. It happens that in 1899 this region was visited by a series of exceedingly severe earthquake shocks during which the coast line in Yakutat Bay was greatly deformed, in one place being uplifted forty-seven feet. The hypothesis which we advance in explanation of the changes briefly outlined above, is that during this, or possibly some previous earthquake, vast quantities of snow and ice were shaken down from the mountains upon the gathering grounds of these glaciers, starting a wave of advance whose thrust is now being felt by some of the glaciers, the thrust being sufficiently powerful to crowd forward and break up even the nearly stagnant termini. The selective action of this process, by which some of the glaciers are caused to advance and others not, may be only apparent, for it is possible that the wave of advance has not yet affected glaciers which will ultimately begin to move forward. It is highly probable, however, that in some instances the supply ground of the glaciers did not have conditions favorable to the large accession of snow necessary to cause a rapid crowding forward.

The future progress of this interesting phenomenon should be carefully watched. In order to discover and definitely prove its cause, we need to have a series of observations extending through successive years to see what happens to those glaciers which have just advanced, and to determine whether other glaciers are influenced in the same way, and exactly how they are affected. There is no region of active glaciers known to me which promises to yield more important results than this, if carefully watched for the next few years.

RALPH S. TARR
CORNELL UNIVERSITY,
ITHACA, N. Y.

SCIENTIFIC NOTES AND NEWS

DR. EDWARD L. NICHOLS, professor of physics in Cornell University, has been elected president of the American Association for the Advancement of Science for the meeting to be held next year at Chicago. The New York meeting, held from December 27 to January 1, under the presidency of Dr. W. H. Welch, professor of pathology in the Johns Hopkins University, has had no equal in size and probably no parallel in its service for the advancement and diffusion of science. The full report of the general secretary will be printed in the next issue of SCIENCE, and reports of the proceedings of the national scientific societies which met in affiliation with the association will follow.

THE council of the British Association has now appointed the presidents of sections for the meeting of the association to be held at Leicester next year. Of Section A (mathematical and physical science) the president will be Dr. Love, Sedleian professor of natural philosophy at Oxford; Section B (chemistry), Professor A. Smithells, professor of chemistry in the University of Leeds; Section C (geology), Dr. J. W. Gregory, professor of geology at Glasgow; Section D (zoology), Dr. W. E. Hoyle, keeper of the museum in the Victoria University, Manchester; Section E (geography), Mr. George G. Chisholm; Section F (economic science and statistics), Professor W. J. Ashley, dean of the Faculty of Commerce in Birmingham University; Section G (engineering), Professor Silvanus Thompson; Section H (anthropology), Mr. D. G. Hogarth; Section I (physiology), Dr. A. D. Waller, director of the Physiological Laboratory in London University; Section K (Botany) Dr. J. B. Farmer, professor of botany in the Royal College of Science; and Section L (educational science), Sir Philip Magnus.

PROFESSOR THOMSON, of Cambridge, M. Moissan, of Paris, Professor Golgi, of Pavia, and Professor Ramòn y Cajal, of Madrid, were present at Stockholm on December 10 to receive the Nobel prizes awarded to them.

DR. HENRY RUTGERS MARSHALL, of New York City, has been elected president of the American Psychological Association, and Dr. H. N. Gardiner, of Smith College, has been elected president of the American Philosophical Association.

MR. F. H. NEWELL, chief engineer of the Reclamation Service has been elected president of the Washington Society of Engineers.

GRAF VON ZEPPELIN, known for his work in aeronautics, has been given the honorary doctorate of engineering by the School of Technology at Dresden.

DR. ADOLF LIEBEN, formerly professor of chemistry at Vienna, has celebrated his seventieth birthday.

Professor E. Hersey Sneath has resigned the chair of theory and practise of education in Yale University owing to ill health.

The special board for biology and geology at Cambridge University has adjudged the Walsingham medal for 1906 to W. E. Agar, for his essay on 'Researches into the Embryology of the Dipnoi,' and to W. L. Balls, for his essay entitled 'Studies of Egyptian Cotton.'

Professor H. W. Fisk, formerly professor of mathematics at Fargo College, Fargo, N. D., Messrs. P. H. Dike, formerly of the U. S. Coast and Geodetic Survey, and E. H. Bowen, formerly instructor in physics in the University of Maine, are at present engaged in magnetic work under the auspices of the department of terrestrial magnetism of the Carnegie Institution of Washington.

Dr. H. E. Annett, professor of comparative pathology in the University of Liverpool, has gone to St. Vincent in order to study certain diseases of animals for the government.

Dr. Acland, having been appointed by the Royal Colleges of Physicians and Surgeons official visitor to the examinations of the Cairo Medical School, has left London for a few weeks in connection with the appointment.

Dr. von Lecoq, a scientific emissary of the Prussian government, has arrived safely at Srinagar, after making important excavations in Chinese Turkestan.

The Friday evening meetings of the Royal Institution of Great Britain will be resumed on January 18, when Sir Andrew Noble, Bart., F.R.S., will give an address on 'Fifty Years of Explosives.' On February 1 Sir Almroth E. Wright, M.D., F.R.S., will speak on 'The Methods of Combating the Bacteria of Disease in the Interior of the Organism.'

Dr. Edward J. Nolan, recording secretary of the Academy of Natural Sciences of Philadelphia, is engaged on a history of the Academy from its foundation in 1812. He is desirous of having access to any correspondence or documents, apart from the official records of the society, which may relate to its work, especially during the first half century of its existence. If such papers be confided to Dr. Nolan, they will be discreetly used and returned safely to the owners.

The Women's Medical Association of New York City will hold a memorial meeting at the Academy of Medicine on January 4, as a tribute to the memory of the late Dr. Mary Putnam Jacobi. There will be addresses by Dr. William Osler, Dr. Charles L. Dana, president of the Academy of Medicine; Professor Felix Adler, Mrs. Florence Kelley and others.

The death is announced of Mr. John Ward, a British geologist, known for his valuable work on the carboniferous faunas.

Dr. Ernst Pfitzer, professor of botany at the University of Heidelberg, has died at the age of sixty-one years.

The U. S. Civil Service Commission announces an examination on January 16, 1907, to fill a vacancy in the position of forest planting assistant, at $700 per annum, in the Forest Service of the Department of Agriculture.

The College of Physicians of Philadelphia announces that the next award of the Alvarenga prize, being the income for one year of the bequest of the late Senor Alvarenga, and amounting to about $180, will be made on July 14, 1907.

According to foreign journals, a wealthy landed proprietor named M. Audrac, who died recently at Le Luc, near Draguignan, has left the Pasteur Institute the whole of his fortune, valued at the equivalent of $250,000 at least. Interviewed on the subject, Dr. Roux, the distinguished director of the institute, stated that he had received a visit from a lawyer, who informed him that a will had been found bequeathing the whole of the property to the institute. The reserve, however, was made that another document might possibly come to light making various bequests or otherwise disposing of part or whole of the property; consequently, Dr. Roux says that some time must elapse before the Pasteur Institute can know definitely how it stands with regard to the inheritance.

The Magnetic Survey yacht *Galilee*, engaged in the magnetic survey of the Pacific Ocean under the auspices of the Carnegie

Institution of Washington, left San Diego, California, on December 22 on her third cruise, which will extend to the end of the year 1907 and will embrace about 25,000 to 30,000 miles. It was found necessary to defer the proposed circumnavigation cruise and instead to pursue a course indicated by the following ports of call: Nakuhiva (Marquesas Islands), Tahiti, Apia, Yap, Shanghai, Hongkong, Yokohoma, Honolulu, Dutch Harbor (Aleutian Islands), Sitka and return to San Diego, from which port it is possible that another circuit situated between the Galapagos Islands and Marquesas Islands may be undertaken. The vessel, as during the past year, is again commanded by Mr. W. J. Peters, who is assisted by Messrs. J. C. Pearson and D. C. Sowers, magnetic observers, and Dr. George Peterson, surgeon and recorder. The sailing master, as during the past two cruises, is Captain J. T. Hayes.

THE Vienna Geographical Society has celebrated the fiftieth anniversary of its foundation under the presidency of Archduke Rainer.

THE preliminary program of the second International Congress on School Hygiene, of which King Edward is patron, has been issued. The session will be from August 5 to August 10, 1907, at the University of London, South Kensington. The work of the congress will be divided into eleven sections, each presided over by an authority on the subject dealt with.

WE learn from *The British Medical Journal* that news has come from the Sesse Islands, Victoria Nyanza, of Professor Koch's expedition for the study of sleeping sickness. The expedition is divided into two parts. One, under the direction of Professor Koch himself, is working at Kion, the other, under Professor Beck, at Boumangi. Natives are said to be coming to both stations from the remotest provinces, the average number attending being some three hundred a day. Professor Koch, it is said, will publish nothing as to his method until he has had more definite results than have yet been obtained. The members of the expedition live in rooms destitute of the comforts of civilization, which serve as bedroom, dining room and laboratory.

WE learn from *The Athœneum* that Messrs. Heffer & Sons have purchased the mathematical library of the late Professor Joly, of Dublin, and the botanical library of the late Professor Marshall Ward, of Cambridge, catalogues of which are in preparation.

THE London *Times* states that the 52d annual dinner of the Society of Engineers took place on Wednesday night, December 12, at the Hotel Cecil. The president, Mr. Maurice Wilson, Assoc. M. Inst. C. E., was in the chair, and among those present were Sir Alexander Kennedy (president of the Institution of Civil Engineers), Dr. Glazebrook (president of the Institution of Electrical Engineers), Commander Caborne, C. B., Colonel L. Blakeney Booth, Mr. Maurice Fitzmaurice, Mr. R. St. George Moore (the president elect), Mr. J. W. Wilson (vice-president), Professor S. H. Cox, Mr. D. D. Butler (honorary treasurer), and Mr. Perry F. Nursey (secretary). Sir Alexander Kennedy, in proposing 'The Society of Engineers,' said that it was now the day of the engineers, and they had better make the best of it they could. Clearly the essential thing was that they should be as good engineers as they could, and one of the things which had helped them to be good engineers hitherto, and was helping them now, was their extraordinary tendency to form themselves into technical societies. One of the pioneers was certainly the Society of Engineers, which was third in age of the engineering societies, and which he hoped might live and flourish for many years to come. The president, in responding, said that their membership had increased during the past year, and their financial position was exceedingly sound, sounder, perhaps, than it had ever been. They had unfortunately lost their oldest honorary member, Sir Edward Reed, who was elected nearly thirty years ago and who was naval constructor about the time of the transition from wooden walls to armour-clad ships. Their ordinary meetings had shown an increase in their average attendance during the past year, and they had had read to them numerous interesting papers, not the least interesting of them being one by their indefatigable honorary secretary, who had contributed his twenty-fourth paper to their transactions. Their members could be found all over the world, and included men of all nationalities. Mr. R. St. George Moore proposed the toast of 'Kindred Institutions,' for which Dr. Glazebrook responded, and the toast of 'Our Guests' was submitted by Mr. J. W. Wilson and acknowledged by Mr. Maurice Fitzmaurice.

In 1905 the total value of our mineral production was $1,623,877,127, as compared with $1,360,883,554 in 1904. As heretofore, iron and coal are the most important of our mineral products. The value of the iron in 1905 was $382,450,000; the value of the coal, $476,756,963. The fuels increased from $584,043,236 in 1904 to $602,477,217 in 1905, a gain of $18,433,981, or 3.16 per cent. Anthracite coal showed an increase in value of $2,904,980 from $138,974,020 in 1904 to $141,879,000 in 1905. The increase in value of the bituminous coal output over 1904 was $29,480,962, a combined increase in value of coal of $32,385,942 in 1905, or 7.3 per cent. The gain of $262,993,573 in the total value of mineral production is due to gains in both metallic and non-metallic products, the metallic products showing an increase from $501,099,950 in 1904 to $702,453,108 in 1905, a gain of $201,353,158, and the non-metallic products showing an increase from $859,383,604 in 1904 to $921,024,019 in 1905, a gain of $61,640,415. To these products should be added estimated unspecified products, including molybdenum, bismuth, tungsten, and other mineral products, valued at $400,000, making the total mineral production for 1905 of $1,623,877,127. Besides the usual table and summary of quantities and values of the country's mineral output by products, the volume contains this year, for the first time, a summary, in tabulated form, of the value of the mineral products by States. These tables were compiled by Mr. Wm. Taylor Thom.

UNIVERSITY AND EDUCATIONAL NEWS

New York University has received a gift of about fifteen acres of land adjoining the south line of its grounds and extending towards 180th Street. The value of the property is said to be between $200,000 and $300,000.

Mr. Andrew Carnegie has given to the College of Physicians of Philadelphia $100,000 towards the erection of its new building, on condition that a like sum be subscribed, of which $80,000 has already been received.

The department of archeology of the University of Pennsylvania has received a gift of $40,000 from Mr. Eckley Brinton Coxe, Jr. The donor has specified that of the gift $8,600 a year shall be paid for five years to the new curator of the department of Egyptology, Dr. D. Randall McIver, who is now in Egypt, where he has been instructed to begin excavations.

We learn from *Nature* that Gonville and Caius College, Cambridge University, having decided to close their chemical laboratory at the end of the present academic year, a syndicate was appointed on November 8 to consider the assignment of a site for the extension of the chemical laboratory. The conclusion arrived at is that, of the sites available, the one site which is not liable to considerable objection lies between the chemical laboratory and the new medical schools, with a frontage next Pembroke Street.

The University of Turin celebrated on October 27 the five hundredth anniversary of its foundation, this celebration having been postponed for two years, owing to the disastrous fire. In honor of the occasion a history of the university has been published.

Dr. William A. Noyes, head of the department of chemistry in the Bureau of Standards, and secretary and editor of the American Chemical Society, has been elected professor of chemistry in the University of Illinois.

The council of King's College, London, has appointed Mr. H. de Sadow Pittard, M.A., Ph.D., as assistant lecturer in mathematics, and Mr. A. W. Sikes, D.Sc., M.D., F.R.C.S., as demonstrator in physiology.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, JANUARY 11, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE NEW YORK MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE meetings of the American Association for the Advancement of Science and of the national scientific societies affiliated with it, held during convocation week, demonstrate a further advance in the organization of science in this country. The number of scientific men gathered together was larger than ever before in America or in any country except Germany, and still more significant is the fact that there were manifest on all sides devotion to high ideals of scientific research and a spirit of sincere cooperation. It is a fine thing that in a country and period supposed to be concerned chiefly with material success there should come together more than two thousand scientific men and women, nearly all of whom have chosen and now pursue their life work from love of science and of truth, who are devoting themselves to the increase of knowledge and the application of this knowledge to human welfare.

There are, of course, difficulties as well as advantages in a meeting of such magnitude. At the first of the convocation-week meetings, held in Washington four years ago, there was some conflict of interests; for example, three sessions devoted to the reading of botanical papers were held simultaneously. At the second large meeting, held in Philadelphia two years ago, the friction was slight, but questions as to the advisability of such joint meetings and of the usefulness of the American Association were occasionally raised. At the present meeting the general spirit of harmony and good fellowship was obvious. There are, of course, many open questions and many differences of opinion—it would be a sign of atrophy rather than of growth if such did not exist—but everywhere appeared a willingness to solve the questions and compromise the differences by the use of kindliness and common sense.

It was indeed a cause of regret that it was possible to attend only one of the simultaneous meetings and to talk at leisure with only a small part of those present, but this is a drawback which evidently would not be overcome if the societies met simultaneously in a dozen different places. The specialization demanded by the increase of knowledge and the limits of the capacity of a single man does not set sharp boundaries between the sciences, but rather tends to obliterate them. A society of physiological chemists can not meet apart from physiologists and chemists, or a society of vertebrate paleontologists apart from geologists and zoologists. There were held in New York joint meetings of special interest of botanists and zoologists, of mathematicians and astronomers, of philosophers and psychologists, and of other groups. The opportunity was also taken for the meetings of many committees and boards, on which men of science working in different directions are represented.

There was in no case any conflict between a section of the association and a special society; in all cases joint programs were arranged. It was indeed suggested in the council that the officers of the sections and the sections themselves are superfluous, but this probably represents the opinion of a small minority. The special societies are far more competent than the sections to arrange special programs and to elect their members. The association has acknowledged this, its council having passed resolutions empowering the sectional committees to turn over special papers to the special societies and instructing the permanent secretary to nominate as fellows members of the affiliated societies having the same standards as the association. But the sections still have important functions. They give an organization which provides for representation in the council and for reference from the council of special questions. The sections, as a rule, embrace a group of sciences, and thus serve as centers of affiliation and organization. They are also the best means of keeping the association and the sciences in touch with the wider public. A program such as that of the section of physiology and experimental medicine appears to be in every way satisfactory. It had a vice-presidential address of general interest and a discussion on a subject common to the sciences concerned, but left all special papers to the societies meeting in affiliation.

The wide dispersal of American men of science and the time of meeting are more serious difficulties than the unification of interests at the meetings. The solution will probably be found in meetings at more than one time in the year and at more than one place. The association held this summer a most pleasant and profitable meeting at Ithaca; it has authorized those sections which so desire to hold meetings during the summer of 1907, and plans a general summer meeting in some New England town in the summer of 1908. Two meetings a year will partly solve the difficulties of travel, for one of them would naturally be held near the Atlantic seaboard and one farther to the west. Still, the time will probably come when it will be desirable to hold two or more meetings simultaneously in different regions, having perhaps a general congress of scientific men once in three or five years.

The association should be competent to make the arrangements whenever and wherever meetings of scientific men are needed. There are many details, such as railway transportation, which can be most economically arranged at a central office, and there should be in each center academies, societies or committees which stand in relation to the association. The Smithsonian Institution or the Carnegie Institution should provide a central office for the association, and if neither of these institutions appreciates the privilege of doing this, funds should be secured for a building. The council at New York passed an important resolution instructing the committee on policy to report means by which "the efficiency of the organization of the association may be increased and the office of the permanent secretary made more important." There is every reason to believe that the secretary of the association should and will occupy an office at least equal in usefulness, dignity and salary to the secretaryship of the Smithsonian Institution. In this age and country it is a higher honor to be directly responsible to the organized body of American men of science than to be subject to a board of regents or trustees, the majority of whom have but a vague conception of the methods and ideals of science.

The general secretary, in his report of the meeting printed below, estimates the attendance at *not less than* 1,500. This is a safe estimate, for he tells us that while 106 members of Section C registered, the attendance of chemists was estimated at 300. In so far as similar conditions hold for other sections, this would indicate a total attendance of about 2,700 scientific men and women, and this figure would not include the large number of local members who attended some of the functions or sessions without registering or the ladies who accompanied members. This may be an overestimate, but certainly the lecture rooms of Columbia University were as a rule crowded. Thus on Friday afternoon people were standing and turned away from the discussion before the naturalists on 'The Biological Significance and Control of Sex' and the address by Professor William James on 'Surplus Stores of Energy.' Yet each of the lecture rooms holds about 300, and there were many other meetings at the same time. There were about 2,000 people at the exercises at the American Museum of Natural History on Saturday.

To ascertain the exact attendance at the meetings is not in itself a matter of importance. A great many members of the association do not register because there is no reason to do so except as a matter of record. It is, however, unfortunate that such a large proportion of scientific men attending the meetings are not members of the association. There has indeed been a great improvement in recent years, the membership having increased about threefold since the preceding New York meeting of 1900. At that time the official membership was 1,721, whereas on December 1 it was 4,498, and over 400 new members were added at the meeting. This increase is largely due to the fact that at the preceding New York meeting arrangements were made by which this journal is sent free of charge to members without any advance in the membership fee. For the sum of three dollars an American scientific man may be a member of the association and receive the weekly issues of SCIENCE, whereas in Great Britain it costs $12.50 to belong to the British Association and to receive *Nature*. Yet the purchasing power of money is greater there than here. A committee on increase of membership was established by the council, and it is to be hoped that this committee will bring to the attention of all men of science the privilege and the duty of allying themselves with their fellow workers in our national scientific organization.

The most unsatisfactory aspect of recent meetings of the association has been the lack of adequate recognition and appreciation by the general public. In a democracy, science, literature and art should not depend on patrons and privileged classes for support and recruits, but on the whole people. The American Association is now accomplishing more than the British Association in its work as a body of scientific men, but it falls far behind it in a function nearly as important, namely, the diffusion of science and the promotion of general appreciation of scientific work. At the annual meetings of the British Association from one to two thousand associates and ladies, the leading people of the community, join the association, subscribing to its funds and attending its meetings. The London *Times* and other newspapers devote daily pages to accurate reports. At and before the New York meeting about 500 leading citizens of New York City joined the association, including our greatest living man of letters, many of the prominent editors, lawyers, physicians and clergymen of the city, and a number of those prominent in the commercial development of the city and the county. This is a distinct advantage both for them and for the association, and similar and increased efforts in this direction should be made at subsequent meetings.

On the other hand, the reports in the press were simply scandalous, in spite of the fact that the editor of each of the leading papers consented at the request of a personal acquaintance to arrange for adequate reports. Because one of the speakers mentioned the name of Mr. John D. Rockefeller, three fourths of the space given to the whole meeting on that day with its two or three hundred important papers, was devoted to him. The *Sun* reported in detail the alleged discovery of a method to do away with old age and death, cabled from France, while almost ignoring the scientific discoveries and advances of the meeting. A newspaper which would discharge an editor for making a 'break' in literature or art will expose its scientific nakedness without shame. But it must be remembered that it is our scientific men and our scientific organizations, rather than the newspapers themselves, on which the ultimate responsibility rests.

The principal events of the meeting are briefly recorded in the report of the general secretary, which follows, and accounts of the scientific sessions will be printed in subsequent issues. Probably the most important action taken was the establishment of a section on education. An amendment to the constitution to this effect had been proposed at the New Orleans meeting and was supported by a petition signed by 171 of those interested, including many leading educators. Such a section will have two somewhat distinct functions. It will be concerned, on the one hand, with education as a science and, on the other, with the teaching of the sciences. In both cases the alliance with the association is fortunate. Those who are aiming to make education a science can profit by association with those who are following sciences that are more exact, while a majority of the fellows of the association are engaged in teaching. There are also important questions of educational administration which can properly be brought before the section. Lastly, it is desirable on both sides that those who are engaged in the teaching of the sciences in the schools and colleges should be brought in contact with those who are carrying on research work. It may further be noted that steps were taken at the New York meeting to form a federation of the local societies of teachers of the mathematical and natural sciences. The constitution of the association was also amended so that psychology should be officially recognized in its scope. The Psychological Association, one of the strongest of our special societies, has long been affiliated with the association, and to it will doubtless be left the psychological program of the section of anthropology and psychology.

New committees were formed on seismology, on the bibliography of science and on the celebration of the fiftieth anniversary of the publication of the 'Origin of Species' in 1909. In that year the British Association will meet in Canada, and a cordial invitation was presented to take part in that meeting. A letter was read announcing that the Walter Reed memorial fund, the movement for which originated at the Washington meeting, now amounts to over $24,000, and toward completing the fund of $25,000, the sum of $100 was appropriated from funds in the hands of the permanent secretary, as a mark of appreciation by the association of Reed's great services in the suppression of yellow fever. A resolution was passed urging the passage of bills creating forest reserves in the Appalachians and the White Mountains.

Space permits no reference here to the scientific meetings, before which about 800 papers were presented, but these will be subsequently reported. A few words should be said of the general functions and arrangements. It was recognized by all that Columbia University offered admirable headquarters. Earl Hall not only supplied good facilities for registration, but also provided excellent offices and rooms for writing, conversation and smoking. These were increased by the opening of the adjacent Faculty Club, where two smokers were held. The restaurant charges in the university commons were low and the service as good as could be expected for such large numbers. The rooms for the special meetings were often taxed to their full capacity, but were usually adequate, and there was practically no confusion or conflict. Equally satisfactory were the arrangements at the College of Physicians and Surgeons, which is the medical school of Columbia University, the University and Bellevue Hospital Medical College, which is the medical school of New York University, the Rockefeller Institute, the New York Botanical Garden and the American Museum of Natural History.

Especially to be noted as bringing together large groups of members were the reception given by the president of Columbia University in Earl Hall following the address of the retiring president of the association; the luncheon at the City College, preceded by timely addresses and followed by an inspection of the beautiful and well-arranged new buildings; the extremely interesting ceremonies connected with the unveiling of ten busts of pioneers of American science, presented to the American Museum by Mr. Morris K. Jesup, and the reception at the museum in the evening, given jointly by the museum and the New York Academy of Sciences, with an admirable exhibit of scientific progress arranged by the academy. Nearly every society and group of scientific men had their own smokers and dinners, so that in spite of the large numbers present, there was opportunity for each one to see those who work in his own field. The arrangements of the meeting prove that while the winter may not be so convenient as the summer in giving opportunities for informal metings and social intercourse, much can be accomplished in this direction.

The long line of great men who have filled the presidency of the association was at the New York meeting continued by a president who in rare degree unites dignity, tact and good sense, whose ideas and fit words come directly from broad sympathies and noble achievement. He is able to hand on the office to one eminent in his science, a leader in all movements to advance science and to promote the objects of the association from within and from without. The place of meeting is also fortunate. It will have been forty years since the association last met at Chicago, and in that period the city and the state have become notable for scientific activity. Chicago is nearer to the center of scientific population than New York, and there is every reason to believe that the meeting next year in that city will rival or excel the great New York meeting.

REPORT OF THE GENERAL SECRETARY

The fifty-seventh meeting of the American Association for the Advancement of Science was held at Columbia University, New York City, December 27, 1906, to January 2, 1907.

The registered attendance of association members was 934. This places the meeting as the fifth in order of size. It was exceeded in attendance by the meetings at Philadelphia in 1884 (attendance 1,261), Boston in 1880 (997), Washington in 1902–3 (975) and Montreal in 1882 (937).

The average attendance at the last twenty meetings of the association preceding the New York meeting was only 458, about one half as great as at the New York meeting.

The above figures are from the registration at the central registration office up to five o'clock on December 31. The registration of attendance of three sections of the association was known to be incomplete at that time on account of an accident which interfered with the arrangements for registration. It is believed that the total number of persons in attendance at the meetings of the association and affiliated societies was not less than 1,500, placing it first in rank among meetings of the association.

The following shows the registered attendance by sections: A—Mathematics and Astronomy, 80; B—Physics, 104; C—Chemistry, 106; D—Mechanical Science and Engineering, 25; E—Geology and Geography, 115; F—Zoology, 181; G—Botany, 139; H—Anthropology, 57; I—Social and Economic Science, 24; K—Physiology and Experimental Medicine, 61; no preference indicated, 42.

The geographical distribution of the 934 members who registered is as follows: 300 from New York; 110 from the District of Columbia; 96 from Massachusetts; 82 from Pennsylvania; 44 from New Jersey; 44 from Ohio; 30 from Connecticut; 25 from Illinois; 17 from Rhode Island; 17 from Canada; 14 from Maryland; 14 from New Hampshire; 12 from Michigan; 11 from Virginia; 11 from Missouri; 10 from Wisconsin; and less than ten from each of twenty-four different states. Two persons registered from England and one each from Ireland, Cuba and Japan.

Four hundred and eleven new members were added to the association at this meeting. Of these about 350 are due to the activity of the local committee, which had, moreover, previously sent nominations in of about 125 members.

Ten papers were furnished to Section A. These papers were presented before the one joint session with the Mathematical Society and the Astronomical Society or were merged in their programs.

Section B had ten papers on its program, and the American Physical Society had 27 papers. The papers were presented in joint sessions.

Section C met in joint session with the American Chemical Society, at which 123 papers were presented. The large number of papers required that the section be divided into sub-sections of inorganic, organic, industrial, agricultural and sanitary, physical and biological chemistry, each presided over by a special chairman who added to the interest of the meeting by an address on his special branch of chemistry. Although the registered attendance of Section C was but little more than 100, the secretary of that section states his belief that about 300 chemists were present at the meetings.

Nine papers were presented before Section D (Engineering).

Section E held two independent sessions for the reading of papers; it also held four joint sessions with the Geological Society of America, the sessions being held in two sub-sections. Ten papers were presented to the section, and more than sixty papers were presented to the Geological Society.

Section F held joint sessions for the reading of papers with the American Society of Zoologists, at which about eighty papers were presented. There was also held a joint session with Section G for the reading of papers on plant and animal breeding. The secretary of the section says that the attendance at the meetings was from 100 to 300.

Section G held three independent sessions for the reading of papers. About thirty papers were presented. Members of the section also took part in one joint session with Section F, and attended largely the meetings of the Botanical Society of America, one of which was held at the Botanical Garden in Bronx Park.

The program of Section H and its two affiliated societies contained forty-nine papers.

The programs of Section I, including the joint session with the Anthropological Association and Section H, contained eighteen papers.

Section K, together with its affiliated societies, held four sessions.

Much more than one half of the papers of the meeting belonged to the affiliated societies rather than to sections of the association. From this point of view the meeting was a confederation of affiliated societies to a greater extent than it was a meeting of a group of sections of a compact organization.

The subject of the address of the retiring president of the association, Professor C. M. Woodward, was 'The Science of Education.' A vice-presidential address was delivered before each section.

The relations between the sections and the affiliated societies were entirely harmonious, the officers cooperating with each other in making arrangements for meetings.

Columbia University placed its buildings at the disposal of the association and the greater number of sections met at the university. Certain meetings were held elsewhere, notably, the meetings of Section K at the Rockefeller Institute and at the College of Physicians and Surgeons. Appropriate thanks were tendered by the president of the association at the closing general sessions to the local committee, the authorities of Columbia University and to many others who, in their capacities as hosts, contributed to the success of the meeting.

The following general events contributed to the success of the meeting and the pleasure of those in attendance: (1) a reception by the president of Columbia University at Earl Hall on Thursday evening; (2) a general meeting with addresses and luncheon at the College of the City of New York at noon on Saturday; (3) on Saturday afternoon exercises at the American Museum of Natural History, connected with the unveiling of the busts of ten American men of science, presented to the museum by Mr. Morris K. Jesup; (4) on Saturday evening a gathering at the museum to see the collections and special exhibits arranged for the occasion. The cultivation of acquaintance among the members was also promoted by many smokers, dinners and other special events which brought various groups together. The reception at Earl Hall on Thursday evening was probably the only occasion at which one found so large a gathering of members at any one place as to give the idea that this meeting of the association was an unusually large one. Probably most members did not realize that they were attending one of the largest if not the largest meeting of many years.

The most important actions taken by the council and the association were as follows:

1. The addition of a new section to the association, namely, Section L—Education. A petition in favor of the formation of such a section was presented to the association to which were affixed the signatures

of 171 men, including many of the most prominent educators.

2. The title of Section H was changed from 'Anthropology' to 'Anthropology and Psychology.'

3. The permanent secretary was authorized to publish hereafter the programs of the affiliated societies as a part of the official program of the association.

4. Section E, and other sections desiring to do so were authorized to hold meetings in the summer of 1907.

5. A standing committee of fifteen on seismology was appointed. The committee consists of G. K. Gilbert, U. S. Geological Survey; Cleveland Abbe, U. S. Weather Bureau; L. A. Bauer, Carnegie Institution; C. E. Dutton. U. S. Army; H. F. Reid, Johns Hopkins University; Otto Klotz, Observatory, Ottawa, Ont.; W. W. Campbell, Lick Observatory; A. C. Lawson, chairman, California State Earthquake Commission; R. S. Tarr, Cornell University; L. M. Hoskins, Stanford University; C. G. Rockwood, Jr., Princeton University; W. H. Hobbs, University of Michigan; W J McGee, St. Louis; John F. Hayford, U. S. Coast and Geodetic Survey; and T. A. Jaggar, Jr., Harvard University.

6. A standing committee of five on the bibliography of science was appointed, one of whose duties shall be to cooperate with the International Catalogue of Scientific Literature. The committee consists of J. McKeen Cattell, R. S. Woodward, Jas. Lewis Howe, Wm. Trelease and C. B. Davenport.

7. A Darwin Memorial Committee of ten was appointed to consider the manner in which the American Association for the Advancement of Science may suitably commemorate the fiftieth anniversary of the publication of the first edition of the 'Origin of Species.' The committee consists of H. F. Osborn, chairman, L. O. Howard, E. G. Conklin, A. C. Lane, D. T. MacDougal, J. McK. Cattell, J. M. Coulter, H. B. Ward, F. Boas and C. B. Davenport, secretary.

8. Grants for research were made as follows: $100 to the Concilium Bibliographicum; $100 to Professor Frederick E. Clements for aid in the study of the relation of Alpine plants to their environment; $100 to J. Arthur Harris for aid to complete a statistical investigation of the influence of environment on the characteristics of organisms. The last two recommendations were made on the understanding that the grants will be expended under the supervision of the standing committee on the relations of plants to climate.

9. The Entomological Society of America was admitted to the privileges of affiliation.

10. The council recommended that the committee on the policy of the association be requested to consider means by which the efficiency of the organization of the association may be increased and the office of the permanent secretary be made more important, and that the consideration of these matters be made to include the following points: procuring a permanent secretary to devote his entire time to the work of the association and to receive greatly increased pay; the matter of raising the dues of the association; the matter of dropping the entrance fee of the association; the matter of changing the time of convocation week and of the meeting of the association; and the relation of the association to the affiliated societies.

11. It was decided to hold the next regular meeting of the American Association for the Advancement of Science, at Chicago in the winter of 1907–8, and not to hold a summer meeting in 1907.

12. It was recommended to the next general committee that a meeting be held in the summer of 1908, preferably in some New England town, and that the regular

meeting during convocation week in 1908–9 be held in Baltimore.

The officers elected for the ensuing year were:

President—Professor E. L. Nichols, Cornell University.

General Secretary—President F. W. McNair, Michigan School of Mines.

Secretary of the Council—Professor Wm. Harper Davis, Lehigh University.

Vice-presidents of the Sections:

A—Professor E. O. Lovett, Princeton University.

B—Professor Dayton C. Miller, Case School of Applied Science.

C—Professor H. P. Talbot, Massachusetts Institute of Technology.

D—Professor Olin H. Landreth, Union College.

E—Professor J. P. Iddings, Chicago University.

F—Professor E. B. Wilson, Columbia University.

G—Professor C. E. Bessey, University of Nebraska.

H—Professor Franz Boaz, Columbia University.

I—Dr. John Franklin Crowell, New York City.

K—Dr. Ludvig Hektoen, Chicago University.

L—Hon. Elmer E. Brown, U. S. Commissioner of Education.

Secretary of Section B—Professor A. D. Cole, Ohio University, for five years.

Secretary of Section I—Professor J. P. Norton, Yale University, for five years.

Secretary of Section L—Professor Edwin G. Dexter, University of Illinois, for five years.

The Sectional Committee of the New Section, 'L' (Education)—President, David Starr Jordan, Leland Stanford University; President Charles S. Howe, Case School of Applied Science; Professor Thomas M. Balliet, New York University; Professor E. L. Thorndike, Columbia University; Professor C. M. Woodward, Washington University.

The minutes of the opening general session in Earl Hall on December 27, together with the addresses, are appended to this report.

JOHN F. HAYFORD,
General Secretary

PROCEEDINGS OF THE OPENING SESSION

PRESIDENT C. M. WOODWARD IN THE CHAIR

PROFESSOR WOODWARD: The American Association for the Advancement of Science will please come to order. In opening this meeting, ladies and gentlemen, and before I perform my simple official duty of introducing the new president, I can not avoid saying what a joy it is, what a delight it is, to meet such a congregation of scientific men. As one looks down the fifty-nine years which this association has lived and has seen how from a few hundred it has grown to four thousand members, and how many splendid things it has performed in one way or another all in the interests of science and making that science meet the needs of man in this great community, it is a time for congratulation; and I feel especially proud to be for the moment a representative of such an organization. And with this word of greeting to you and of congratulation for the association, I take great pleasure in presenting to you the president-elect, who will preside over our deliberations and lead us forward during the few days that we now meet together, Doctor W. H. Welch, of the Johns Hopkins University, who is to be president for the coming year—Doctor Welch.

DOCTOR WELCH: *Ladies and gentlemen*, this is not the first meeting of the association over which I have had the honor to preside, inasmuch as the old custom of holding a midsummer meeting was revived last summer. Before this larger gathering of the association I desire to renew the expression of my thanks and of my profound appreciation of the honor conferred upon me by election to this high office. I interpret this honor above all as a recognition of the position held to-day by medicine in its relations to general science, and it is especially as an honor conferred upon the science of medicine that I beg to acknowledge your action in selecting me to preside over this association of scientific men and women.

We shall now have the pleasure of listening to words of welcome from the president of Columbia University, Dr. Butler.

PRESIDENT BUTLER: *Mr. President, ladies and gentlemen:* It is a pleasure, and no small satisfaction, speaking on behalf of my colleagues in the university—trustees, faculties, alumni and students—to offer you a cordial and hearty welcome to our home. Nowhere in America, perhaps, could you by any possibility receive a warmer or more sympathetic welcome than here. We are so fortunate as to count among our university membership a very considerable proportion of the membership of your association, and we look to them to lead and to guide and organize our university policies and opinions in the field of the sciences of nature and of man. Whatever facilities we may have for making you comfortable and happy are wholly and entirely at your service, and our part will be played to our own satisfaction if, when the hour of adjournment comes, you shall feel that nothing has been left undone that thought or care could do to provide in every way for the success of this gathering and for this most important meeting.

I might perhaps stop here, because, after all, there is not much to be gained by repeating a formal invitation that is once given with sincerity and heartiness; but I can not resist the opportunity to say just a word in addition to my word of welcome. I am one of those who now for nearly thirty years has observed at first hand the slow, and then the rapid, advance of the sciences to their present place in the school and college programs of this country. It has been my fortune to listen to and sometimes to participate in the discussions and debates which have accompanied that advance. So far as I now recollect, every vote that I have had to give has been given in its favor. But now at the end of this period I can not help feeling, as I observe from reading the literature of the subject that the same feeling is shown in England, in France and in Germany, that we have not yet succeeded in so organizing the sciences as instruments of general education as to fulfil the high expectations which some of us formed for them nearly a quarter of a century ago.

This is a subject which I respectfully commend to the study of this most representative and competent body. What is it that remains to be done, and what should we here do, so to correlate and organize and present the subject matter and the methods of the sciences as to increase their effectiveness as educational instruments?

I say with great frankness that if we did not know that we are going through what is doubtless a period of transition, the movement in which we have all been participating has cost us something and gained not much. But we are going through a period of transition. Perhaps we are expecting too much and too soon, but my purpose in saying what I now say is to bring to your attention the fact that to many of us the consideration of the educational effectiveness of the sciences touches the whole field of human interest, of human knowledge and of human activity.

There can be little doubt that the sciences of nature and of man, properly organized and presented as educational instruments, are destined to be classified as true humanities. I can not help feeling that in addition to their power to instruct and inform they have a power to refine, to uplift and to guide; but I am quite confident that as yet we are very far short of having so organized this material as to attain these ends.

I hope very much that the next decade may see intensive study of this aspect of these scientific problems and of scientific work; and that out of it all may come, not a larger place in the educational program for the sciences, because that would hardly be possible—but a more effective and more uplifting and a more humanizing result of teaching the sciences, in order that we may pass on to the next generation this new educational instrumentality organized and perfected for true educational work, which never can be limited to the passing of information from hand to hand or mere instruction in method by master to pupil.

I throw out this suggestion, because here, in this association, are men and women devoting their lives to the study and investigation and presentation of truth, to whom in the colleges and universities, and to whom in the nation at large, we must look for the formulation of the answer to just such questions as this. To them I commend the question as to the proper organization of the sciences as instrumentalities in general education.

I say again you are heartily and completely welcome to this university and whatever hospitality it can offer and every act of friendliness which you will give us the privilege to show.

President Welch responded to the address of welcome as follows:

Ladies and Gentlemen: In behalf of the members of the American Association for the Advancement of Science and of the Affiliated Societies I thank you heartily, President Butler, for your cordial words of welcome, and I assure you and your colleagues that it is most gratifying to us to have the opportunity of meeting in this city and at this university.

It was not until nearly forty years after its foundation that this association met first in the city of New York, whereas this third meeting in New York follows only six years after the preceding one in the same place. These events in their periods of sequence indicate in a measure the rate of growth of science in this city and its increase of attractiveness to men of science. While for a century and more there have been eminent scientific men in New York City, it is, nevertheless, true that for a long period of time letters and science were not represented in this city in a degree at all commensurate with its position in other respects, and New York thereby lacked a note of distinction in its civic and educational life possessed by several smaller cities and even small towns in this country.

In recent years, indeed in the short interval since our last meeting here, the conditions have changed rapidly, and New York is taking a position in education and the promotion of science more nearly approaching its leadership in commerce and other material interests. The most powerful instrumentality in bringing about this great advance has been Columbia University with the influences which have gathered about it, and it is most gratifying to the members of this association to witness, where we are now assembled, the marvelous growth of this great university. A worthy share in this development of the higher learning has been borne also by New York University, and in our visit to the new buildings of the College of the City of New York, where a general meeting of the association is to be held on Saturday, we shall have the opportunity to behold the visible evidences of the most enlightened liberality of a municipality in support of higher education for the people.

Those interested in natural science will find nowhere a more impressive illustration of municipal liberality in support of an institution for the instruction of the people and the advancement of natural knowledge than the American Museum of Natural History, where on Saturday evening we are to be the guests of the trustees of the museum and of the council of the New York Academy of Sciences, this latter organization ranking also among the important forces contributing to the development of science and of the scientific spirit in this city, and adding much to the interest of our meeting by the admirable exhibition and demonstrations of recent scientific progress which it has arranged in the museum. No feature of our meeting will afford greater pleasure and inspiration than the ceremonies attending the unveiling of the busts of American men of science presented to the museum by Mr. Morris K. Jesup, to whose generosity, public spirit and individual efforts American science is so deeply indebted. I wish to acknowledge at this time the courteous thoughtfulness of the trustees of the museum not only for their hospitality but also for selecting the time of our meeting for these interesting ceremonies.

The Metropolitan Museum of Art, with its magnificent collections in art and archeology, like the American Museum of Natural History, sets an example to the national government in the cultivation of the sciences and the fine arts.

Our botanical members at their meeting at the New York Botanical Garden will find there, as well as in the adjacent Zoological Park, in the opportunities afforded for the study of science and for the delight of the people, another and kindred illustration of the wise liberality of this city.

The New York Public Library, with its magnificent new building approaching completion, is another splendid foundation, resting upon both private and public munificence, which ranks among the great educational institutions of this city, whose growth and widened usefulness in recent years are significant of the progress of learning and science.

The holding of meetings of one of our sections and of affiliated societies in the new building of the Rockefeller Institute for Medical Research will afford opportunities to inspect laboratories unsurpassed in their arrangement and equipment for investigation in those branches of medical science to which they are devoted. In this connection I may state that New York City leads the world in the application by its department of health of the great discoveries of the last quarter of a century in bacteriology to the prevention of disease.

Time forbids further illustration of recent scientific progress in New York City. I have cited as examples mainly institutions with which the association will be brought, during our meeting, into some personal contact through their hospitality, and they will suffice to demonstrate the rapid and most gratifying development of higher education and of interest in the sciences of man and of nature in this city.

President Butler in his opening remarks has touched upon a subject most timely and important for the consideration of such men and women as constitute the membership of this association, and we shall do well to ponder carefully words on this theme coming from so high an authority upon education. While expressing sympathy with the aims of the natural and physical sciences, recognizing their importance and supporting them, as he has done, he also expresses the opinion that science in the general scheme of education and as an educational instrument has not fulfilled the expectations which he and others entertained regarding it a quarter of a century ago. While it is not possible to discuss this subject adequately on this occasion, I may be permitted to say a few words regarding it.

It is doubtless true that during the relatively short time since the natural sciences were admitted to the curriculum of a liberal education the teaching of these sciences has not attained to that agreement of opinion and fixity of method which centuries of use as instruments of education have secured for the classical languages and mathematics. It must be admitted that methods of teaching the natural sciences have often been unsatisfactory and have, therefore, yielded unsatisfactory results. The subject is one for serious consideration, to which many of our teachers are, I think, alive, and it is a satisfaction to announce that the council at its meeting this morning, in response to a wide-spread and influential demand, recommended the formation of a new section of this association to be called the 'section on education,' which, we may hope, will contribute to the best methods of teaching the sciences.

It may also be admitted that exaggerated claims have sometimes been made as regards the position which the natural sciences should hold in the scheme of general education and as regards the extent and kind of mental discipline, culture and knowledge which, when pursued in such a scheme, they are capable of imparting. Without attempting to assign to these sciences their exact share in a plan of liberal education, and this share, I need hardly say, I deem an important one, I should be sorry to see eliminated from the education of even those looking forward to scientific pursuits the study of the languages, history and philosophy, which give a culture not to be derived solely from the study of the natural sciences and which should add greatly to the intellectual pleasure, satisfaction, breadth of vision and even efficiency of the man of science. Natural science should take its place in a plan of liberal education by the side of the older learning, the so-called humanities; each affords a kind of culture not to be obtained from the other, and any scheme of higher education which does not recognize the equal value of both kinds of culture is one-sided.

The full recognition of the part thus assigned to natural science in liberal education requires an adjustment on the part of the exclusive advocates of the traditional system handed down from the middle ages to new ideals of what constitutes liberal training, but in this field science has won its victory and will not be dislodged. It is, however, not enough to be content with this victory. Science should see to it that in its own field it becomes an instrument of education certainly not less powerful than the older humanities, and President Butler has very properly urged the need of improvements in this direction.

Standing here, as I do, as a representative of medicine in an association devoted to all the sciences of nature the relation of medicine to general science comes prominently to my mind. Medicine has been called the mother of the sciences. There was a time when the leading cultivators of natural science were physicians and when the medical faculties of universities were the homes of about all the science that then existed. In subsequent history physicians have played no small part in the development of the natural and physical sciences. Such important contributors to physical science as Black, Young, Mayer and Helmholtz were all actively identified with the medical profession, and are important figures in the history of medicine as well as of physics. From the study of chemical and physical phenomena of living animals and of man, whether by chemists, by physicists or by physicians, have come important additions to the sciences of chemistry and of physics, and medicine is constantly finding new and important applications of chemical and physical discoveries. We realize to-day as never before the fundamental unity of the biological sciences, and answers to the deepest and most far-reaching problems of medicine, not less than of other biological sciences, are to be sought in the properties of living matter, wherever it exists, whether in plants or in animals or in man.

Medicine has during the past half century entered irrevocably upon the true paths of science. Dogma and transmitted authority are no longer its guides, but it seeks for truth by the only methods found fruitful for all science—experiment, observation and just inference.

The domain which has been opened to medicine during the last quarter of a century by the introduction of new methods and resulting discoveries in the causation of infectious diseases has greatly increased our power to cope with disease, and the masterful pioneers in this new field, Pasteur and Koch, rank among the greatest benefactors of mankind. The wider recognition by governments and by the people of the humane, the economic and the social value of this power of preventive medicine to check incalculable suffering and waste of energy from disease is urgent. One of the most gratifying exemplifications of the useful functions of this association is the initiation by the section on social and economic science of an influential movement for the establishment by the national government of a bureau or department of public health.

This leads me, in closing, to say a few words concerning the scope and aims of this association. Our retiring president, in his introductory remarks, spoke of the great growth of the association, which has more than doubled its membership during the six years since we last met in New York. At its foundation and for many years afterward this association supplied all that was demanded of a national society representative of the various natural and physical sciences. In later years specialization, at once a cause and a result of the great progress of science, has led to the formation of many special scientific societies of national scope, and the end is not yet in sight. It became evident several years ago that the association, in order to retain its usefulness, if not its life, must adjust itself to the new conditions, and this it did by taking the position of a central organization of science with which the various special societies, while remaining autonomous, should become affiliated and constituent units. It can not be doubted that the broad conception underlying this readjustment is the correct one and that its application has already been attended by a large measure of success.

There remain, however, certain difficulties to be overcome and certain problems to be solved before this association shall have attained that ideal of organization and of usefulness to which we may reasonably look forward. The need of such a central, national organization as a coordinating, unifying, harmonizing influence, as an authoritative representative and exponent before the public of scientific opinion and of scientific workers, as an instrument to secure cooperation among scientific investigators, to influence public opinion, to advance the interests of science as a whole as well as to inaugurate and to secure support for special scientific undertakings and lines of investigation, and as a means of securing that most desirable purpose, placed first among the objects of the association in its constitution, 'to promote intercourse between those who are cultivating science in different parts of America'—the need, I say, of a central organization with these aims and others which might be specified demands, I think, a wider and deeper appreciation among the scientific men and women of this country. Especially should our leaders in science realize, as many of them do, the great possibilities of usefulness of this association and work actively for the promotion of its welfare. Larger financial resources are needed to perfect the organization and to enable the association to cultivate more fruitfully the fields which it already occupies and to enter new ones. Within the near future the membership should rise to at least ten thousand. With its history of honorable achievement and its present success this association may confidently look forward to a future of greatly increased power and usefulness for the advancement of science.

I now declare open this fifty-seventh meeting of the American Association for the Advancement of Science, and in so doing I express the hope, which our program indicates to be indeed an assurance, that the sessions and social functions of the association, of the various sections and of the affiliated societies may be full of interest, pleasure and profit to those in attendance.

Our program now calls for announcements by the general and local secretaries. Has the local secretary any announcement to make?

Doctor Cattell: The local arrangements are announced in the general program, which may be obtained in the entrance hall. Notices in regard to excursions and receptions which concern only sections or affiliated societies will be made in those societies' meetings. Perhaps the only thing that needs to be said is that ladies accompanying members are very cordially invited to the receptions that have been arranged. These include the reception by President Butler, of Columbia University, in Earl Hall, at 9 o'clock this evening, a luncheon and general meeting on Saturday at the College of the City of New York, the unveiling of ten busts of pioneers of American Science at the American Museum of Natural History, on Saturday afternoon, and in the evening a reception given by the trustees of the museum and the New York Academy of Sciences, with an exhibition of scientific progress by the academy.

Doctor Welch: We will now hear from the general secretary, Mr. Hayford.

Mr. Hayford: A letter which has just been received from the secretary of the building committee of the United Engineering Societies, which is the holding corporation for the Engineering Society's building. (Reading) "It gives me pleasure as secretary of the United Engineering Society, which is the holding corporation for the Engineering Societies' building at No. 29 West 39th Street, to extend an informal invitation to the members of the American Association for the Advancement of Science to visit our building during the week of the meeting." Signed by F. R. Hutton, secretary.

Doctor Welch: Our printed program contains the detailed announcements. It is in order to move an agreement on the hours of the meeting.

Doctor Smith: I move that the hours of the meeting of the various sections be as specified in the printed program. (The motion being seconded, the president put it to a vote, which being unanimously in the affirmative, the motion was declared carried.)

Doctor Welch: The session is now adjourned. The association will meet again on Tuesday morning at ten o'clock in this hall.

THE POLICY OF THE CARNEGIE INSTITUTION[1]

Since the trend of development of the institution still hinges to some extent on the relative merits of large projects carried on under the direct supervision of the institution itself and of small projects committed to individuals whose affiliation with the institution may be only temporary, a large amount of attention has been given to this question during the year; much more in fact than to any other. It is a matter of daily correspondence, of daily interviews and of daily importunities. With a desire to see all sides of this question and to hear

[1] Concluding part of the report of the president, 1906.

all arguments thereon, the president has solicited much of this correspondence and many of these interviews. He has received a wealth of highly esteemed advice and suggestion along with much more that must be characterized either as impracticable of application or as fraught with grave danger if applied.

A considerable portion of this advice and suggestion would make instructive reading if printed, although they are in large degree conflicting and need, obviously enough, here and there, correction for personal equation; but, aside from greater concentration on matters of detail, they do not differ essentially in the aggregate from the advice and suggestion given by members of the advisory committees whose reports are printed in the earlier year books of the institution. Hence it does not seem worth while to add to the bulk of printed discussion along this line, even in the cases of correspondence whose authors would doubtless approve publication of their views. The president desires here, however, to express his warm appreciation of the counsel on this question given him confidentially by many colleagues in the academic and scientific world. Whether this counsel has been pro or con as regards his own views an effort has been made to weigh it fairly.

In the meantime there have been some opportunities for reflection on the various aspects of the question, while the institution is accumulating experience which, though not as yet conclusive in its bearings, furnishes important indications of the lines along which development may be expected to be effective or ineffective. It seems desirable, therefore, to state here some of the provisional conclusions to which observation, experience and reflection have forced me, not without opposition, in some cases, to preconceived notions.

Categorically these conclusions are the following:

First, that the institution may not advantageously enter the fields now occupied by colleges and universities. It should be no part of the function of the institution to endow scholarships and fellowships for indigent students, nor to supply helpers, assistants, apparatus, libraries, museum collections, etc., for purely educational work, nor to supplement meager salaries of college and university professors whose work is primarily educational. This conclusion and the specifications enumerated seem so axiomatic that their statement would be quite superfluous here if the institution were not daily importuned for aid in one or more of these and many similar ways. Some eminent minds maintain, indeed, that since the object of the institution is, in the last analysis at any rate, educational, these numerous ways of promoting education should not be overlooked, for the sphere of effective influence of the institution, it is argued, may be thus widely extended. The experience of the institution thus far, however, appears to be in direct opposition to this view. We are learning how the giving of aid by one institution to another, even indirectly, tends to sap the independence and to diminish the available income of both. Moreover, we encounter by this method the endless difficulties arising from diverse interests and divided responsibilities, along with the inevitable bitterness of disappointment from those who feel that the distribution of funds has not been equitable amongst the fields of research or amongst the institutions supplying the investigators.

Secondly, that the institution may not advantageously seek to scatter its resources simultaneously over all available fields of research. It should rather choose a limited number of fields of activity at any epoch and concentrate its energies on these until they are brought to a satisfactory degree of completion. This conclusion seems likewise almost axiomatic, since it is determined essentially by a limited income. Many, if not a majority, however, of highly esteemed colleagues oppose this conclusion, and argue that a distribution of income in small grants to widely scattered investigators will be more productive in immediate results and of more ultimate benefit to society. But this argument does not appear to be supported by the experience of the institution. It is impossible, of course, to draw precise inferences from this limited experience; but after a careful examination of the facts at hand I think it safe to state that no direct return may be anticipated from more than half of the small grants made up to the present time for minor researches and for research assistantships. Moreover, it appears to me that this is as high a percentage of efficiency as may be reasonably expected from miscellaneous applicants for aid, since a majority of them will be men and women of enthusiasm and promise merely rather than of demonstrated ability to carry researches to successful conclusions.

In weighing this matter the educational value of such widely scattered aid should not be overlooked. Many a meagerly equipped laboratory or library may be thus strengthened and many young men and women may be thus trained for work of research. The possession of a piece of apparatus, or a rare volume, or the opportunity to pursue early in life a year or two of uninterrupted scientific investigation, is, doubtless, of inestimable value to a few individuals. But the obvious objection to such a disposition of resources is that it serves only to supplement the educational work of colleges and universities. They already occupy this field, and it appears unwise as well as unfair to encroach on their domain even in a supplementary way. A less obvious objection is that arising from the diverse interests and the divided responsibilities which such a course entails. It may be observed also that as regards themselves the experience of colleges and universities appears to be inimical to such a course, for we have not heard of any of them proposing to use its income, or any considerable share thereof, in building up departments of educational work in other institutions. But the fundamental objection to such a disposition of funds is that it promotes research only indirectly, whereas the primary object of a research institution should be to promote research directly. A research institution should aim to take up investigations which, by reason of their expense or magnitude, are not likely to be carried to completion in other ways. And in the pursuit of this work it should be free to choose the best ways and means; it should not be hampered by a host of applicants backed by endless recommendations of doubtful validity.

Thirdly, that the institution may advantageously limit much more narrowly than hitherto the award of minor grants. It should seek to eliminate the amateur, the dilettante and the tyro as far as possible from the list of eligible applicants, and concentrate attention chiefly on those who have already demonstrated ability to produce results. This policy will restrict the range of operations of the institution to some extent, but it will diminish the hazard to a greater extent, and will permit a degree of thoroughness of work not otherwise attainable.

One of the most serious objections to giving aid to numerous small projects lies in the fact, amply shown by experience, that the estimates of the cost thereof are generally vague and almost always too small for the accomplishment of good work. Many, if not most, authors of such projects proceed without plans and specifications, often ignoring somewhat contemptuously such estimates of cost and probable outcome as may be supplied in most cases by judicious forethought. The projects are so small that it does not appear essential to individual investigators to consider carefully their cost and bearing. Indeed, only investigators of considerable experience are able to use adequate forethought in this respect. But when one contemplates not a single small project, but the aggregate of a large number of them, the need for carefully drawn plans, specifications and estimates is seen to be as important as in the case of any large project.

By limiting the fields of activity in this direction it will be possible for the institution not only to make a choice amongst promising investigations, but likewise to make a choice amongst tried investigators. This appears to me to afford a workable compromise also between the extremes of a limited number of large projects and an unlimited number of small projects—a compromise whereby the essential advantages of both extremes may be secured and their inherent disadvantages avoided.

But while it appears desirable to limit the range of activity of the institution at any epoch, it appears still more desirable to insist on a high standard of efficiency determined by the quality and the quantity jointly of results attained. To secure this end the institution must not only seek to aid mainly eminent investigators, but it must seek to aid them for such periods and to such an extent that their best efforts may be enlisted. The grantee should be able to feel that his connection, though temporary, with the institution is creditable, and, reciprocally, that the aid he accepts implies higher obligations than those attaching to an educational scholarship or fellowship. In many cases within the experience of the institution grantees appear to have regarded the system of small grants as a sort of lottery, involving neither credit to nor responsibility from either party to an award. Experience of this and similar kinds is inevitable, however, in the formative stages of the institution; for the distinction between a research institution and an educational institution is not yet so clearly defined that contemporary society can avoid attributing to the former the eleemosynary function which is being slowly eliminated from the latter.

In conformity with the views here set forth the president is disposed to recommend that in general minor projects be aided only when they can be carried on by investigators of known competence; that such investigators become for the time being affiliated to and advisers of the institution, and that they be designated as research associates of the institution. The periods of affiliation of such associates must be determined, of course, by the circumstances of individual cases. But it may be observed that as a rule these periods will be from two to five years, or more, since few investigations well worth undertaking by the institution can be brought to satisfactory conclusions in shorter intervals of time.

It appears worthy of note, from the point of view of evolution, that the institution finds itself occupied with two principal divisions of activities, namely, those arising from its internal affairs and those arising from its external affairs. On the one hand, we are busily engaged with many investigations, in many diverse fields, carried on under widely varying conditions. On the other hand, we are equally busily engaged with a multitude of external relations which are usually more or less conflicting and often incompatible. Thus the development of the institution may be likened to the struggle of an organism which is trying at once to discover its proper functions and to adjust itself to the conditions of its environment.

It is worthy of note, also, from the same point of view, that this struggle is inevitable to a great degree, and that it is only out of the resulting chaos of opinions as to ways, means and methods, and out of the experience of the institution itself, that definite and approved lines of action and policy may be attained.

In view of these circumstances, it seems essential to warn our allies of the academic world and the public at large against the danger of expecting more from the institution than is possible of accomplishment in a limited time and with a limited income. Although the work of the institution is in a peculiar degree novel and untrammeled, it is yet subject, properly enough, to the restrictions set by human experience and by contemporary society. Hence, if the reviewer of the year books finds reason to complain of a bewildering array of technical details, he should reflect that this array is far less than a host of investigators would like to have it. If the humanist or the scientist finds reason to complain that little or no aid has been given to him or to his special field of research by the institution, he may derive comfort from the fact that he is one of an overwhelming majority necessitated by the limitations of available resources. And if the bibliophile has found reason for dissatisfaction in the distribution of the publications of the institution, he may be disposed to be lenient with the latter on learning that he is one of many thousands soliciting favors.

Out of this plexus of internal and external relations and interrelations it is the duty of the administrative branch of the institution to evolve, so far as practicable, such a degree of order and system as will best promote productive and thorough work of research, and at the same time to restrict, so far as practicable, an unproductive or wasteful expenditure of energy and resources. Although progress towards an adequate fulfilment of this duty must be of necessity slow in order to be sure, it is believed that distinct advances are accumulating, and that the obvious difficulties and dangers which beset the development of so novel an institution are only such as may be overcome by a reasonable application of time and patience.

R. S. Woodward

Carnegie Institution,
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SCIENTIFIC BOOKS

The Principles of Heredity. By G. Archdall Reid. London: Chapman and Hall.

The problems presented in the study of heredity are so diverse and so intricate that they should be illuminated by data drawn from all fields of biological science. Possibly, the phase of the subject which has been the least systematically studied is that of the evidence bearing on heredity afforded by disease, and the publication of a volume by a medical man of high scientific attainments, which embraces this neglected data is to be welcomed. The existence of statistical records makes it possible to utilize the observations made on the inheritance of diseases, and, in this particular field, 'The Principles of Heredity' is a contribution deserving of much consideration. Dr. Reid, the author, has made notable contributions to the study of evolution and heredity in earlier works, as 'The Present Evolution of Man' and 'Alcoholism. A Study of Heredity.'

The analysis of the subject of heredity is now changing from the stage of general treatment to a very critical one, based on measurements and experiments, as well as on the closest microscopic examination of the hereditary substance and its behavior during initial stages of development. This makes it difficult for any writer to satisfy present standards. It will be appropriate to examine Dr. Reid's work with this situation in mind.

The title of the book, 'The Principles of Heredity,' leads the reader to expect something more than appears in it. The biological student, be he medical man or layman, will be disappointed that the author does not mention, in any adequate way, in his chapter on 'Theories of Heredity,' the recent work of students of cytology as laying the foundations for a scientific study of the subject. The compensation for this insufficient treatment of a fundamental aspect of the subject is to be sought in the introduction of new matter from the field of observation of the medical man. But, this is not altogether satisfying.

The results achieved by the application of biometrics to studies of heredity, and by experiments, along lines suggested by Mendel's researches, are also omitted. The chapter on 'Theories of Evolution' and the subsequent consideration of the theories mentioned are likewise disappointing. This is not, as it appears to the reviewer, because the statement of the theories is necessarily brief, but because it is superficial and dogmatic.

The presentation of the subject is more argumentative and speculative than closely analytical. The pages abound in great vigor of statement, but the positiveness of the author's position on controverted matters detracts from its worth. The book appears to be written with little scholarly reserve, and one misses in it the fine balance of statement which has been set as a standard by earlier writers in the same general field, even as far back as Lamarck, and so fully exhibited in Darwin's works and in the last edition of Weismann's 'Lectures on the Evolution Theory.' Therefore this book, although introducing much new matter, does not appear to rise to the level of current standards in the serious discussion of the principles of heredity.

A few quotations will serve to illustrate the vein in which the discussion is carried on: "At first sight it would appear an easy matter to test the truth of the Lamarckian doctrine of heredity. We might, for example, amputate the tails of a pair of parent dogs, and then observe whether puppies, subsequently born, were tailless. But the theory is not held in this crude form, at any rate by the scientific supporters of it." The suggestion that observations upon the progeny of one pair of parent dogs might give results of scientific value is too superficial, and should be accompanied with a reference to the work of experimenters on this very point, such as the following by Weismann of the effects of amputating the tails of both parents, through twenty generations of mice. Many similar instances occur, where a general statement is made without reference to any recent data. The author's discussion of the inheritance of acquired characteristics, although we agree with his main contention, is not up to the standard of that of a number of writers of the past few years.

In portions of the book the author goes at his task like a special pleader. The outline of Chapter XII. on 'The Argument from Disease' is characteristic: "The Lamarckian doctrine is certainly untrue. It is equally untrue that hereditary tendencies may be easily changed by the direct action of external forces —Professor Cossar Ewart's observations."

Page 14: "The Bathmic theory of heredity and evolution may then be ruled out of court. We are left with the Lamarckian and Neo-Darwinian doctrines. One or the other or both combined must furnish the true explanation of evolution." The use of *must* in this connection is an illustration of what is meant by the author's positive form of statement, as is also 'The Lamarckian doctrine is certainly untrue.' The author might properly reach these conclusions after a suitable examination of the data bearing upon the matter, but what one misses is the essence of philosophical opinion, and the reader is to be excused if he reaches the conclusion that the writing is a weak discussion of underlying principles. While a limited class of readers may like to have debated questions so directly disposed of, the large body of scientific readers will take exceptions to this summary way of dealing with them.

After one page of general statement the author says, page 15: "We need not multiply instances; the Lamarckian doctrine should now be plain to the reader." But to one at all acquainted with Lamarck's writings, the impression remains very strong that Lamarck's doctrine can not possibly be plain to the reader from the author's presentation of it.

The book not only lacks evidences of seasoned thought, but of familiarity with the more recent literature bearing on the discussion of heredity, and, on the whole, is a disappointing analysis of the subject. Nevertheless, we believe it will be of service on account of the new point of view adopted and the citing of evidences bearing on heredity furnished by disease. Doubtless, this volume will assist materially in getting medical men to pay more attention to the matters discussed in it. If this be the case, the purpose of the book, as stated by the author in his preface will be justified: "I have addressed the volume mainly to medical men. The evidence relied on is drawn largely from medical sources; medical men form the largest body of scientific workers; they deal continually with questions of heredity, a knowledge of which is of great importance to them; but in a measure they have neglected the systematic study of the subject. Little or no instruction is given in it to medical students. There does not even exist a text-book to which they may refer. But a knowledge of heredity is becoming essential to the educated doctor. I have sought to supply the want. I hope, however, the professional biologist and the general reader will not find the work devoid of interest."

William A. Locy

Postelsia, The Year Book of the Minnesota Seaside Station, 1906. St. Paul, Minnesota. 1906. Pp. 364. Small octavo.

Four years ago the first volume of this unique publication was issued, and now we have a second volume so like the first in paper, print, illustrations and bindings that it seems a fit companion for it upon the shelves of the botanist's library. Like its predecessor, the present volume contains seven papers, with a half-page 'Word of Introduction' from Professor MacMillan, the director of the Minnesota Seaside Station. The first paper, 'Observations on Plant Distribution in Renfrew District of Vancouver Island,' by C. O. Rosendahl, occupies more than one third of the volume. In it the writer first discusses the marine formations, then the formations of the beach, and the formations of the forest country, and follows with an annotated systematic list of the pteridophytes and spermatophytes of the region. His conclusion is "that the flora of Vancouver Island, in so far as it can be judged by observations confined to a limited area of the same, is typically boreal, with an admixture of more arctic forms than the latitude, the elevation above sea-level, and present climatic conditions would indicate." The second paper, by F. K. Butters, on 'The Conifers of Vancouver Island,' describes thirteen species as occurring spontaneously on the island, viz., *Taxus brevifolia,* a shrub or small tree; *Pinus contorta,* a small tree; *Pinus monticola,* a tree 30 meters or more in height; *Picea sitchensis,* 'tidel and spruce,' attaining 60 meters in height and two meters in diameter; *Tsuga mertensiana,* 'mountain hemlock,' a tree of the alpine regions; *Tsuga heterophylla,* 'western hemlock,' a tree nearly as large as the tidel and spruce; *Pseudotsuga taxifolia,* 'Douglas fir,' a large tree of 'magnificent proportions'; *Abies grandis,* 'white fir,' a tall, slender tree; *Abies amabilis,* 'white fir,' 'a tall tree with a straight, slender trunk'; *Thuya plicata,* 'cedar,' a large tree, 'not infrequently five meters in diameter at the base'; *Cupressus nootkatensis,* 'yellow cedar,' a tree of moderate size; *Juniperus communis sibirica, 'juniper,'* a dwarf, trailing shrub; *Juniperus scopulorum,* 'western red cedar,' a small tree. The author thinks it 'probable that further exploration of the higher mountains of the interior will reveal from one to three other species of the *Abietineae.*' In the third paper, A. W. Evans makes an annotated list of 71 species of *Hepaticae* collected principally by members of the Seaside Laboratory. 'Some Western Helvellineae' is the fourth paper by D. S. Hone. It is followed by a paper by R. F. Griggs describing a new genus of kelps, *Renfrewia,* related to *Laminaria* and *Cymathere,* from the Vancouver coast near the Seaside Laboratory. But one species, *R. parvula,* has been discovered. Isabel Henkel's 'Study of Tide-pools,' and Professor C. W. Hall's 'Geological Features of the Minnesota Seaside Station' are interesting geological papers with excellent half-tone illustrations. A full index closes this most interesting volume, which in the words of the editor "will be a souvenir to those who know the Vancouver coast and love the memories of the happy days and nights under the sheltering roof of the 'Sea Palms,' or beside the white water." To others it will certainly justify the hope that it 'will have some scientific and permanent value.' Charles E. Bessey

The University of Nebraska

SOCIETIES AND ACADEMIES

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 419th meeting was held on November 3, 1906, President Knowlton in the chair and twenty-seven persons present.

Dr. Theo. N. Gill presented the first paper of the meeting, on 'The Work of Pterophryne and the Flying-Fishes.' The combination of two such different fishes as sargasso fish, or *Pterophryne,* and the exocœtoid flying-fishes is the index of a curious history.

In December, 1871, one of the most accomplished naturalists of the nineteenth century, Professor Louis Agassiz, made a famous voyage for discovery to Brazil and while traversing the Gulf Stream, found a globular nest-like mass of sea-weed filled with eggs, which he examined and noticed in a letter published in the *American Journal of Science.* These eggs were identified as those of the sargasso fish, now generally known as *Pterophryne histrio.* In 1887 many more such masses were found in the sargasso meadows off the coast of Africa, and, accepting the identification by Agassiz, Professor Leon Vaillant explained how the *Pterophryne* made the nest. In 1894 Professor K. Möbius described still more in detail the eggs and nest and likewise assumed the correctness of the identification of the mass as a nest made by *Pterophryne.* He described in detail and figure the bipolar filaments of the eggs found in connection with such masses and found, by examination of the ovaries of a *Pterophryne,* that the ovarian eggs had no filaments and were smaller than the eggs found in the nest-like masses. He consequently postulated that the filaments must be acquired during the passage of the eggs through the oviduct. Such were the opinions up to last year. It was then found that the *Pterophryne* had nothing whatever to do with the nest-like masses of sargasso!

In the fall of 1905 Dr. Hugh Smith invited the speaker to call at his office and see some eggs that had been laid by a *Pterophryne* in an aquarium under his own observation. To his surprise, those eggs had no filaments and were smaller than those that had been described by Möbius. Subsequently Dr. E. W. Gudger sent him a notice of similar eggs and explained that they were extruded in a long jelly-like mass, in fact like that issuing from an angler (*Lophius piscatorius*).[2] It became evident, therefore, that the nest-like masses of sargasso must be made by a very different fish from the *Pterophryne.* The only eggs like those found in the sargasso weed are those of flying-fishes. In fine, the nest-like masses of sargasso are not made by any fish at all, but by the eggs themselves. The eggs must be laid on the fronds of the weed and the long motile tendril-like filaments clasp the finely cut branches of a frond till a globular mass is brought together. As Professor Agassiz had not noticed any bipolar filaments on the eggs examined by him, Dr. Gill thought it was possible that a lot of the eggs of a *Pterophryne* might have drifted in a mass with flying-fish eggs. The different times of oviposition of the sargasso-fish and the maker of the nest-like masses, it is true, were an objection to such a hypothesis, but it was assumed that there might be exceptional coincidence. To test the hypothesis, Dr. Alexander Agassiz, the discoverer of the peculiar oviposition of the angler, sent eggs taken from outside of a nest-like mass figured by him and they proved to have the filaments characteristic of flying-fishes. That hypothesis must, therefore, be abandoned, and the one crediting the formation of the nest-like masses to the flying-fish alone be accepted for the present at least.

Dr. Hugh M. Smith in discussing the subject described the spawning of *Pterophryne*

[2] See 'A Note on the Eggs and Egg-laying of *Pterophryne histrio,* the Gulf-weed Fish,' by E. W. Gudger, in Science, December 22, 1905.

in captivity and exhibited one of the characteristic egg-rafts and a photograph of another. During September, 1906, three fishes that had been under his observation in the aquarium at Woods Hole for some time spawned. The rafts were quite transparent, somewhat elastic, and non-adhesive, and floated at or near the surface in a partly collapsed condition; they were from 45 to over 90 cm. long, about 7.5 cm. wide and about 6 mm. thick, with abruptly tapering ends; the eggs are .6 mm. in diameter and exceedingly numerous, thickly infiltrating the jelly. One fish that produced a band of eggs 45 cm. long on September 24 laid another string 92 cm. long on October 8. The consecutive ripening of the ovaries is doubtless normal, but there is at least one observation (at Woods Hole, several years ago) of the simultaneous discharge of two egg-bands. In none of the cases where these fishes have spawned in captivity have the eggs been fertilized, and the embryology remains unknown.

The second paper was by Dr. M. W. Lyon, Jr., on 'Local Races of Bornean Squirrels.' He exhibited eight different forms of squirrels of the *Sciurus prevostii* group found in northern and western Borneo, pointing out their characters and showing how the extreme forms were connected by forms possessing intermediate characters. Material at the present time is not sufficient to show complete specific intergradation. A large river proved a very effectual barrier in separating two very distinct races. In the discussion which followed, Dr. Hitchcock asked if two or more of the exhibited races coexisted in the same locality. Dr. Lyon replied that such was not known to be the case, and that as a rule among mammals when two species of the same genus inhabit the same area, they usually belong to very different groups in that genus or to different subgenera. He thought that the squirrels under discussion should form a distinct subgeneric group. Dr. Gill deplored the increasing number of genera and subgenera based on slight characters. Dr. Stiles remarked that in some groups of animals a good criterion for establishing genera had been found when two forms possess a character not found in a third form; two distinct genera are then present, but the author he referred to makes no use of subgenera in this scheme. Dr. Stejneger said that whether definite natural groups were called genera, subgenera or comprehensive species was merely a matter of definition of terms, the facts expressed remained the same. Dr. Lyon thought the multiplication of genera and species was deplored by the non-specialist but was of much assistance to the specialist in any large group of organisms.

The third paper was read by Mr. Karl F. Kellerman, on 'The Use of Copper in Sanitation.' It is evident that a scourge of polluting algæ is an emergency condition. The employment of copper sulphate to eradicate this pollution is satisfactory both from the theoretical and the practical standpoint. Similarly, copper treatment of unfiltered supplies for the purpose of controlling or stamping out waterborne diseases should be considered an emergency contingency and should never be more than a temporary expedient which local conditions might make necessary until permanent means of purifying the water could be established. The question of sterilizing or disinfecting a water supply even two or three years ago was looked upon with great disfavor, but is now recognized by sanitary engineers to be necessary or at least desirable in some cases.

Many chemicals have been proposed to effect sterilization of this character, among which, besides copper, may be mentioned electrolytic chlorine, ozone, lime, silver chloride and various specially named trade products. The selection of the most suitable chemical and the method of applying this chemical for continuous disinfection of a water supply are problems for the future. Such treatment probably will be carried on in connection with filtration, both because of the greater opportunity for removing the chemical employed and because of the necessity of keeping the water free from sediment and similar impurities.

As with the algæ, the different species of bacteria vary in their sensitiveness to copper. Many of the saprophytic bacteria usually present in water are highly resistant, some, which are usually regarded as water bacteria, are extremely sensitive; *Bacillus typhi* and *Bacillus coli* seem somewhat between the two extremes.

Emphasis should be laid upon the intimate relation between sewage disposal and water purification. Proper sewage disposal, whether the varying conditions mean that this should be interpreted as sterilization or merely slight improvement, is logically the first step in the problem of securing and maintaining a safe and potable water supply.

The use of copper in sterilizing a sewage or in improving the quality of the effluent from a sewage purification plant is in many ways comparable to the use of copper for the purpose of removing pathogenic bacteria in a municipal water supply. Briefly, the use of copper in sewage treatment should be restricted to occasionally treating crude sewage from small communities where the initial expense of installing a sewage disposal plant is prohibitive, in improving the effluent of sewage disposal plants of poor quality and in the emergency of an unusual and serious accident to a sewage disposal works.

M. C. MARSH,
Recording Secretary

THE AMERICAN CHEMICAL SOCIETY
NORTHEASTERN SECTION

THE seventy-second regular meeting of the section was held in the rooms of the Trade Club, 77 Summer Street, Boston, on Friday, December 21, with President L. A. Olney in the chair. About forty-five members were present.

Dr. W. C. Bray, of the Research Laboratories of the Massachusetts Institute of Technology, presented a paper by Professor Arthur A. Noyes and himself upon 'An Improved Scheme of Qualitative Analysis for the Tin Group, including the Detection of Platinum, Gold, Selenium, Tellurium and Molybdenum.' The work upon which the paper was based was begun at the Massachusetts Institute of Technology some six years ago, under Professor Noyes's direction, and has been continued to the present time. The analytical scheme is to be universally applicable, and to include all the elements. It will permit of the detection of quantities as small as one or two milligrams of any element in a mixture. The innovations of the system are especially this quantitative character of the work and the systematic method devised for preparing the solution of the substance for analysis. In this paper only that part of the scheme which related to the tin group was considered. It was shown that in the cases of the sulphides of arsenic, antimony and tin, 1 milligram of any one of the three, could be detected in the presence of 500 milligrams of either of the others. In the case of platinum, gold, selenium, tellurium and molybdenum, the quantities which could be detected ranged from one half to one milligram in 500. Confirmatory tests have been introduced in all cases where the original test might be misinterpreted through faulty work on the part of the analyst. Demonstrations of several of the tests were made, to show the characteristic reactions: Thus, the separation of selenium and tellurium by solution in concentrated hydrochloric acid, and reduction and precipitation of the selenium in the cold solution by sulphurous acid—a bright red precipitate separating. The tellurium may then be precipitated by diluting the solution and adding potassium iodide, whereby a black precipitate falls. The precipitation of gold by means of oxalic acid, yielding a purple precipitate, and the detection of molybdenum by use of potassium sulphocyanide and metallic zinc, whereby a brilliant red color was obtained, which soon faded to pale yellow as the reducing action of the zinc continued, were also shown. In the discussion of the paper the speaker brought out certain objections to the usual Marsh test, and showed the advantages of the proposed procedure, in which the magnesium-ammonium arsenate precipitate is redissolved in hydrochloric acid, and the arsenic again thrown out with hydrogen sulphide. The method of separating tin and antimony was also dwelt upon.

FRANK H. THORP,
Secretary

DISCUSSION AND CORRESPONDENCE

GASTROLITHS

In extension of a recent note by Mr. Geo. L. Cannon on the occurrence in southern Wyoming and Colorado of sauropodan gastroliths[1] similar to those first reported by me from the Big Horn Mountains,[2] I wish to state that a recent search through the Yale collections has not revealed any further examples directly associated with Dinosauria.

Meanwhile, however, I have been shown, by Mr. Barnum Brown, four additional specimens obtained in Colorado in proximity to various dinosaur skeletons. These gastroliths are very highly polished, and it is noteworthy that one of them contains a fine bryozoan and traces of other marine organisms identically similar to those noted in one of the examples from the Big Horn Mountains. In fact these specimens are so entirely similar in both texture and the fossils contained as to make it quite certain they were derived from the same source, namely, a marine formation containing siliceous nodules in much the same manner as the English chalk cliffs—or to cite a somewhat different instance, certain moss agate-bearing carboniferous terranes of the Laramie Mountains.

Nor is it improbable that the original source of these fossil pebbles may soon be found, and thus still more closely locate the actual habitat and haunts of the sauropods. At least it already appears that the dinosaurs of the Big Horn and Colorado localities either picked up the fossiliferous nodules on the same ancient shore line, or else from streams which cut this line along some inland range of hills or mountains and carried the pebbles forward into the lower river courses. In the latter and more probable case the dinosaurs would have found the transported fossil bearing pebbles intermingled with a variety of others just as in the two occurrences now observed.

It should be definitely stated that the dinosaurian gastroliths are unmistakable objects. Pebbles which are merely waterworn rarely have other than ground glass surfaces, and where a partial polish approaching a sheen is present, this does not markedly extend into depressions, and is much interrupted by various irregularities. So also the wind-worn pebbles of steppes, although I have seen near the glacial ice sheet limit of eastern Nebraska large boulders of Sioux quartzite presenting small polished areas due to æolian action; while in the case of the readily determinable desert *patina* the glassy surfaces are of a somewhat different origin. But from all these the dinosaurian gastroliths will never fail of recognition. Except for the original inequalities of the pebble outline, though even these may be nearly eliminated, their entire surface may exhibit a higher polish than wind or water ever produces. Indeed the smoothness and sheen are, I may say, from my section making, such as is difficult to equal, and best compared to the finest results obtained by the use of the buffing wheel.

It is evident that the dinosaurs were more or less given to selecting the pebbles they swallowed, choosing the brighter colored ones, much as do the ostriches and other birds. And it appears that pebble-swallowing is quite widely characteristic of not only reptiles and birds, but occasionally of other vertebrates. The fur seal, a mammal of 'strong instincts, but little intelligence,' and with shore habits perchance not utterly dissimilar from those of the ancient pebble-swallowing plesiosaurs, as stated by Lucas, quite frequently swallows among other objects pieces of lava, and chalcedony pebbles, with apparently more or less choice.[3] In none of these other instances, so far as yet known to me, however, are the pebbles long retained and subjected to the high polish seen in the case of the dinosaurs. Strong in gastric secretion, the sauropods perchance sometimes ate, in addition to more succulent plants, primitive or xerophyllous grasses containing considerable silex. But as the gastrolithic habit was doubtless widely characteristic of the entire dinosaurian group, the gastroliths of carnivorous forms will probably be determined, and the degree of attritive

[1] Science, N. S., Vol. XXIV., No. 604, p. 116, July 27, 1906.

[2] *Ibid.*, Vol. XXIII., No. 595, pp. 819–821, May 25, 1906.

[3] 'The Fur Seal and Fur Seal Islands of the North Pacific Ocean,' Part III., page 68.

polishing seen in them will be of some interest in this connection—bearing in mind, of course, that the dinosaurs exhibit such a wide range of skeletal structure that it has been more than once seriously proposed that they can scarcely be all included in the same reptilian order. Nevertheless, if the doubts recently expressed by Dr. Eastman as to any possibility of inferring stomach structure from the presence of gastroliths,[4] find some justification, it is certainly a fact of singular and widening interest that the dinosaurs swallowed and retained and polished far more highly than seen anywhere else in nature, the hardest quartz. The stronger inference by far is that their stomach structure was different from and more complicated than in existing reptiles. And naturally a stomach especially suited to grinding action analogous to that of birds first calls itself to mind. G. R. WIELAND

[4] SCIENCE, N. S., Vol. XXIII., p. 983, June 29, 1906.

DEAFNESS IN WILD ANIMALS

TO THE EDITOR OF SCIENCE: An interesting case of deafness in wild animals came to my attention this season and the conditions seem so simple as to suggest that, possibly, the loss of the sense of hearing or of smell may be no uncommon thing. I should be glad of others' notes on the subject.

In the great semi-arid regions of the west the struggle for existence is so strenuous that the special senses are very highly perfected. Especially is this true of the sense of hearing as evidenced by the enlarged external ear in many forms. The coyote (*Canis ochropus*) is especially marked with this enlarged concha and undoubtedly has, under normal conditions, a very keen sense of hearing. Popular report endows him also with almost supernatural sense of smell. The individual of the species must then labor under a decided handicap if the hearing be destroyed or the sense of smell even slightly impaired. The instance which I cite suggests that such may be often the case.

In cleaning a pair of skulls of this species which I obtained in August last, I found in the case of the male that both ears were crowded full of the bearded seeds of the common fox-tail grass (*Hordeum murinum*) which is such a pestiferous weed in the southwest. The seeds were packed closely into the tympanic chamber and the beards were very much darkened by having remained in the ear some time subjected to the exudations from the inflamed surfaces. In the same individual a fully bearded seed was found in the left nostril worked well up among the folds of the turbinated bone.

The second specimen, a female, taken at the same time, had the grass seeds in both ears but none in the nostrils.

The ear bones showed no sign of necrosis, though the seeds were in direct contact with them. Hearing was undoubtedly destroyed and, in case of the male, the sense of smell must have been impaired.

The animals were taken by strychnine poisoning with a bait of watermelon, a crop the coyote injures extensively in the sparsely settled regions. The specimens came to my hands in the meat; they were in good flesh and pelage. There was no possibility of the seeds having gotten into the ears and nose after death.

The possibility of frequent occurrence of the condition is suggested (1) by the fact that both ears of both animals had been destroyed; (2) by the great abundance and wide range of the species of grass in the case; (3) by the extreme penetrating power of the seed. Each seed tuft is very sharp pointed and is armed with three stout, serrated awns an inch long which force the seed onward with great persistence at each motion of the surface with which it is in contact.

LOYE HOLMES MILLER
STATE NORMAL SCHOOL,
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INTERROGATORY LABELS FOR CERTAIN KINDS OF MUSEUMS

TO THE EDITOR OF SCIENCE: If teachers have learned that it is wise to 'exercise much self-restraint in regard to telling children what they' should 'discover for themselves,' may it not be a wise policy for workers in certain kinds of small museums, and in purely educational departments of museums, to consider the same idea, at least in the labeling of certain exhibits for specific classes of society and for certain purposes?

I do not recall ever having seen an exhibit labeled in such a manner as to indicate that a study had been made of "the art of questioning, by which the children" and the public "are directed, inspired and attain the desired mental growth. Telling the wrong thing and at the wrong time deadens interest and stunts the child's powers. Whenever practicable, the material should be observed first in its natural environment." In such a museum department the labels might describe that environment, giving the locality, etc., but leave the visitor to make some discoveries. By a process of this kind he would be forcibly impressed.

I would not by any means suggest that specimens in museums are over-labeled or that the best forms of existing labels should be set aside, but rather that if a few interrogatory labels and exhibits were tried as an experiment in a case or two, it might open up a new line of possibilities to workers in certain departments of museums and in some kinds of museums.

The quotations above are from a syllabus on nature study for the primary grades for normal college students by William Hittell Sherzer, Ph.D.

Harlan I. Smith

American Museum of Natural History,
December 7, 1906

SPECIAL ARTICLES

NOTE ON THE COMPOSITION OF LIMULUS BLOOD ASH

Within recent years much attention has been given by physiologists and pharmacologists to the chemical condition of automatic tissues. It is generally conceded that a complete knowledge of the chemical reactions going on in the tissues would go far towards clearing up the mechanism of automatism. The rôle played by the inorganic constituents of the plasma has received particular attention. The principal organs used in the study of automatism have been the heart and ciliated epithelium. Little work has been done toward finding out to what extent the automatism of the ganglion cells depends on the chemical changes going on in cells or in the surrounding fluids during the activity of the cells. The respiratory center in vertebrates is not readily isolated for such study. Neither can most of the work done on the vertebrate heart be applied to the ganglion cells themselves, because in most of these experiments the results are complicated by the simultaneous action of the chemicals on the heart muscle.[1]

This may account in part for the existence of the two theories of the origin of the heart beat. It may be that a more exact knowledge of the chemical changes taking place in the heart muscle, the ganglion cells and their surrounding fluids, will explain the reason for the existence of these divergent views. The importance of data on the chemical reactions taking place in the ganglion cells, especially during activity, is obvious. Such data would lead either to important generalizations or to the refutation of generalizations already made.

On account of its unique anatomy, the Limulus heart offers the best organ known for the study of ganglion automatism. The heart may continue to beat rhythmically for days after its removal from the body. The optimum temperature for keeping up this activity is from 15° to 20° C. (Carlson). The ganglion is easily dissected from the heart without injuring its connections with the heart muscle through its numerous nerves. The heart thus serves as a delicate indicator for the activity of the ganglion.

Carlson[2] has made use of this preparation in the study of ganglion automatism. The chemical phase of this investigation requires a knowledge of the composition of the Limulus blood plasma, particularly the inorganic constituents. A qualitative and quantitative analysis of the plasma during both rest and activity, may give a better understanding of the mechanism of automatism. If we find that there are certain changes in the composition of the plasma during activity of the ganglion or heart muscle, we may conclude that

[1] Carlson. Personal communication.

[2] Carlson, A. J., *Am. Journal Physiol.*, 1906, XVI., p. 221; XVI., p. 378.

these changes are causally related to such activity. The change in potential produced by a slight chemical change in a complex solution like the plasma may be of greater significance than a corresponding change in the potential of a simple solution of its inorganic constituents. For even a slight change in the plasma during activity may be sufficient to modify or to neutralize the electrical potential between the proteid complex in the cell and the solution in which it is held. This might result in the precipitation of the proteid complex, or sufficient change to act as a stimulus.

At the request of Dr. Carlson, who has been working on this problem, I made an approximate analysis of the Limulus blood, not knowing of the analyses already made by Genth and by Gotch and Laws.[3] The results of the three analyses agree so closely that all of them are given. No attempt has been made by me to find how the acids and bases are combined except the usual routine methods given by Hoppe-Seyler and the methods adopted by the A. O. A. C.; both of these methods were used wherever they differed.

BLOOD ASH OF LIMULUS.

	Genth.		Gotch and Laws.
NaCl	83.50 %	79.207	85.184
KCl	2.395	4.667	2.707
K_2SO_4	1.686	3.264	.594
$CaSO_4$	3.470	2.159	3.986
$CaCO_3$	1.448	2.950	.275
MgO	5.128	1.959	6.457
$MgCl_2$	1.840	3.848	—
$Mg_2P_2O_7$	.444	1.709	0.236
Fe_2O_3	.081	traces	.029
CuO	.085	0.297	0.508
P_2O_5	—	—	—
SO_3	—	—	—
Cl	—	—	—
SiO_2	—	—	—

BLOOD OF LIMULUS (MCGUIGAN).
Summary of results.

		Per Cent.
Water		91.784
Solids:		
Proteid	5.162	
Ash	2.676	
Other organic constituents	.378	
		8.216

[3] Von Furth, 'Vergleichende Chemische Physiologie der niederen Tiere,' 1903, p. 88.

Ash.

Cl (total)	54.820
NaCl	28.600
KCl	2.930
CaO	2.510
MgO	5.580
P_2O_5	0.340
Fe_2O_3 (not determined)	trace
CuO	0.273
SO_3	1.570
SiO_2	.420
Cl (uncombined with K and Na)	3.281
CO_2	not determined

Although the results agree fairly well, yet there are differences. Whether or not these differences are sufficient to cause an appreciable change in the metabolism of the ganglion cells as evidenced by the behavior of the heart muscle remains to be investigated. Any change in the metabolism of the ganglion cells would be more stimulating if the changes in the composition of the plasma were sufficiently rapid to prevent the acclimatization of the colloids. However, as the sea water itself varies in composition in different localities, in order to get a close agreement in the blood ash, it may be necessary to select animals from the same locality. The ash which I examined was prepared at the Marine Biological Laboratory, Woods Hole, Mass. The proteid and moisture content were determined immediately after the removal of the Limulus from the water. Before any definite conclusions can be drawn from this work it may be necessary to make more analyses.

HUGH MCGUIGAN
WASHINGTON UNIVERSITY

QUOTATIONS

THE GREAT MEN OF FRANCE

THE word *plébiscite* has been for more than thirty years a word of ill-omen in France. One of the most widely circulated of French newspapers, the *Petit Parisien*, has, however, been rehabilitating the word during the last few weeks in a way too striking to be ignored. It appealed to its readers all over the country to vote on the question of the relative preeminence of great Frenchmen of the last century. Fifteen million answers have been received, and what gives value to this striking demonstration is that we have here the opinion of average France, not that of a political *coterie* or of a cultivated *élite*. The world knows now approximately what France thinks of her great men and what her conception is of civic duty, as well as of intellectual and moral distinction. It is the revelation to the foreigner of an idealism certainly unsuspected. Only those observers who have had the privilege of studying the evolution of the French mind and feeling over an unbroken series of years on the spot were aware of the profound transformation which the Republican school system and stable Republican government in general have affected in the points of view of the present generation of Frenchmen.

The winner of the recent contest is Pasteur. Victor Hugo runs him close, having received 1,227,103 votes against 1,338,425 for the world-renowned man of science. But it is characteristic that two men of peaceful pursuits should precede on the list those great Frenchmen who might have appeared at first sight to have most contributed to that special kind of glory known as French. Gambetta follows Victor Hugo with 1,155,672 votes. Then come Napoleon I. and Thiers with 1,118,034 and 1,039,453 votes, respectively. For the sixth place what foreigner would have suggested the name of Lazare Carnot? Yet a moment's reflection will reveal the reasons for his juxaposition with Thiers. The latter has certainly been acclaimed as the 'libérateur du territoire,' and what, after all, was that work of his but the repetition of the incomparable services rendered by Carnot in the organization of the Republican armies of the Revolution? With remarkable persistency, moreover, the French soul to-day vibrates between the primordial patriotic concern as to the defence of the integrity of French soil and its emotion of gratitude in presence of the great peaceful benefactors of the nation in the fields either of science or of art. The order of the names that succeed Lazare Carnot's is the proof of this statement—Curie, the discoverer of radium; Alexandre Dumas *père,* who has charmed several generations not only of Frenchmen, but also of Englishmen; Dr. Roux, the inventor of the diphtheritic serum; Parmentier, the introducer of the potato into France; then Ampère, the father of dynamic electricity; Brazza, the founder of French West Africa; Zola, whose place here thirteenth on the list shows conclusively what France now thinks of his courageous deed as author of 'J'accuse'; Lamartine, a consoling election for those who have always regarded the author of 'The Lake' as the most seductive Frenchman of the nineteenth century; and François Arago, the astronomer and physicist.

This brings us to the sixteenth place, which is held gloriously by Mme. Sarah Bernhardt. But immediately afterwards comes M. Waldeck-Rousseau, MacMahon, the hero of the famous "J'y suis, j'y reste"; President Carnot, who certainly incarnates here a very characteristic conception of civic duty; Chevreul, the chemist; and Chateaubriand, the most eloquently French of all the writers of the last century, unless exception be made for Michelet, who figures twenty-third on this list after de Lesseps. This is a victory which shows how short-lived is French rancour. Ten years ago no *plébiscite* in France would have given such a result, the stupendous energy of the creator of the Suez Canal having been forgotten amid the tempest of the Panama scandals. The next four names are Jacquard, the inventor of the weaving machine, Jules Verne, President Loubet, and Denfert-Rochereau. The list is to be continued until we have before us 502 names. These results constitute a lesson full of instruction not only for the rulers of France, but for foreigners curious as to the temperament and ideals of contemporary Frenchmen.—Paris correspondent of the London *Times*.

CURRENT NOTES ON LAND FORMS

In taking up again the series of 'Current Notes on Physiography,' begun in 1895 in the first of the New Series volumes of Science, the senior reviewer has as associates Professor D. W. Johnson, of Harvard, and Mr. Isaiah Bowman, of Yale. The term physiography was taken, at the beginning of the series twelve years ago, to be the modern equivalent of what has long been known as physical geography, and hence to include a consideration of air and oceans as well as of lands; but as notes on meteorology and oceanography were later handed over to other contributors, and as the attention of the writer of this series of notes came to be more exclusively directed to the forms of the lands, it was found that some readers interpreted physiography as meaning the study of land forms only. To avoid this misconception, the notes now offered are placed under an unequivocal title, for which some writers have suggested the single-word name, geomorphology or geomorphy. However named, the topic here treated is to be regarded only as a large division of physiography, and not as the whole content of that subject. W. M. Davis

WESTLAND, NEW ZEALAND

The chief physiographic features of Westland, a province of the southern island of New Zealand, as described by J. M. Bell ('Geology of the Hokitika Sheet,' Bull. No. 1, N. S., N. Z. Geol. Survey, Wellington, 1906), are: An interior alpine chain, trending northeast, flanked on the west by an uplifted and dissected peneplain, and this followed by a coastal plain, which is interrupted near its inner border by outliers of the dissected peneplain and trimmed along the coast by the sea. The structure of the mountains is synclinal in the main; the highest peaks, 6,000 or 7,000 feet, are of grauwacke or sandstone. The snowfields on the higher slopes and the glaciers which descend into all the higher valleys are much reduced from their quaternary extension. With one exception, the streams issuing from the glaciers do not carry rock flour; this, it is thought, marks the inefficiency of the present comparatively weak glaciers in eroding the rocky beds of their more powerful predecessors.

The present altitude of the flanking peneplain (Wainihinihi) suggests that the alpine chain as well as the peneplain owes its altitude to modern massive uplift, as has been found to be true in so many other ranges. The range is therefore only the less consumed part of an ancient mountain system, of which the peneplain is the more consumed part. This idea is confirmed by the occurrence of occasional elevations which surmount the uplands of the dissected peneplain. The dissection of the uplands has progressed so far in the neighborhood of the principal rivers that the isolated mountainous fragments receive distinctive names. The sides of the larger valleys have been smoothed by glacial action, and in many cases the normal preglacial spurs have been truncated. Cirques in the valley heads, hanging lateral valleys along the main valley courses, and roches moutonnées in the valley floors are frequently seen. The coastal plain, fronting the Tasman sea, is of complex form. In its first cycle a series of marine gravels and clays were elevated and dissected: the dissected plain was then submerged, partly buried under new sediments, and elevated for renewed dissection, with the higher remnants of the earlier plain rising through the surface of the newer one. The present cycle of dissection has been complicated by glaciation. The greatest width of the plain is about fifteen miles, and the elevation of its inner border is 600 feet. I. B.

RIVER TERRACES IN VERMONT

The theory elaborated by Davis some years ago in regard to the origin of river terraces has been tested by E. F. Fisher, who has applied it to the explanation of the terraces found along West River in Vermont, near its junction with the Connecticut. This theory, as stated by Fisher, is that "the river terraces of New England may be accounted for by the behavior of meandering and swinging streams slowly degrading previously aggraded valleys without necessary change in volume, and by the control exerted here and there over the lateral swinging of the streams through the discovery of rock ledges." It is found that this theory alone is adequate to account for the features noted in the region studied.

Professor Fisher presents the results of her studies in a paper entitled 'Terraces of the West River, Brattleboro, Vermont' (*Proc. Bost. Soc. Nat. Hist.*, XXXIII., 1906, pp. 9–42). The lateral swinging of rivers by meanders, cut-offs and short cuts is considered, and the evidence in favor of a fourth process presented. This latter, called the 'partition process,' occurs when for any reason the swiftest current of a stream is withdrawn from a bank of erosion, with the resulting formation of a sandbar or island not continuous with the eroded bank, and therefore parting the stream. The deeper channel eventually acquires the entire stream, the deserted channel and former island are added to the floodplain, and the stream has thus moved laterally a certain distance.

It is shown that West River, swinging laterally by the various processes just noted, and at the same time slowly degrading its previously aggraded valley, has in its downcutting discovered numerous rock ledges, which have exerted a distinct control upon the extent and character of the lateral swinging, and hence upon the erosion which has produced the terraces. The paper is abundantly illustrated by diagrams and plates as well as by maps and sections based on original surveys, and furnishes an important contribution to the theory that river terraces are not necessarily connected with change in stream volume, or with successive uplifts of the region involved.

D. W. J.

FAULT BLOCKS IN THE SIERRA NEVADA

'The Geomorphic Features of the Middle Kern,' by A. C. Lawson (*Bull. Dept. Geol. Univ. Cal.*, IV., 1906, 397–409), deals with a district of subrecent faulting in the southern Sierra Nevada of California. The case appears to be as follows: A great mountain mass, hereabouts reduced to moderate relief, though hardly smooth enough to deserve the name of peneplain, was raised in the huge fault block of the Sierra Nevada, with a gentle descent to the west; it was subsequently more or less dissected. A local fault in the southern part of the mass, bearing north-northeast, with relative uplift on the west and depression on the east, broke the general westward descent of the range and therefore disturbed the westward flow of the rivers, deflecting some of them to new courses along the fault line, and causing all to aggrade their valleys as they approach the escarpment of the uplift. Breckenbridge mountain is the name given to the uplifted and now dissected block, west of the fault line; Walker, Havilah and Hot Springs valleys are aggraded basins lying at the base of the maturely dissected fault scarp. The basins are separated by 'spurs' which come down from the mountains on the east; the spurs appear to be either residual reliefs of pre-faulting form (modified by subsequent erosion), or indications of inequality of faulting, or both. Breckenbridge mountain is described as "an asymmetrical ridge * * * its western slope is exceedingly gentle and descends uniformly towards the great valley [of California]. * * * Its eastern side is a very precipitous mountain front. * * * The mere inspection of the profile suggests immediately that the mountain is a tilted orographic block and that its eastern front is a fault scarp." This vivid description seems to underrate the part played by erosion; for the front of the block as shown in a photograph slopes only about 25°, and is much scored by large ravines. It would, therefore, seem better to describe the front as 'determined by a fault scarp originally, but now much battered and dissected.' It was the neglect of such specific statement of the work of erosion on the fault-block ranges of the Great Basin that contributed to the misunderstanding of their origin by some observers.

'The Geomorphogeny of the Tehachapi Valley System' is another paper by the same author (*ibid.*, 431–462) on a similar problem of greater area and complication. It is of special value because it considers in some detail the pre-faulting topography of its district—a point that has been too generally neglected in studies of this kind.

W. M. D.

TECHNIQUE OF PHYSIOGRAPHIC DESCRIPTIONS

It is a matter of common experience to find difficulty in the appreciation of an article, such as the one outlined in the foregoing note, in which various physiographic features are located with respect to villages like Kernville, Havilah and Vaughn, without the aid of even an outline map. Unimportant villages, presumably unknown outside of their own state—and probably not known all through so large a state as California—have no guiding value to most readers; though in the end there is a satisfaction in knowing that Havilah, for example, appears to owe its opportunity in a mountainous district to the flat floor of an aggraded valley, whose stream was there turned from a previous course down the general slope of the Sierra Nevada into a northward path along the base of a fault-block scarp, and hence is now to be regarded as consequent upon the faulting. Again, it is often the case that a reader must follow through the inductive presentation of a more or less intricate problem—if he does not turn at once to the end of the article in the hope of seeing there a summary which tells him concisely what the writer is driving at—and thus finds himself in the necessity of carrying many items in mind before he knows the conclusion on which they bear. Indeed an inductive presentation, appropriate enough for beginners who have little acquaintance with generalities, may give too much importance to the author's personal experience when employed in articles that are designed for mature readers, already informed as to generalities.

As an alternative by which both of these difficulties may be in large measure avoided, it is worth while to consider a method which has some likeness to one that we all know in geometry, where the theorem is stated at the beginning, in order that all the items of the demonstration may be at once appreciated in their bearing on the end thus placed in view. When applied to physiographic descriptions, this method would require the presentation of the explanatory conclusion at the outset. The conclusion would there be stated, independently of local names, in terms of a systematic general nomenclature, from which the reader could easily build up a mental picture of the larger features of the district concerned; for the systematic nomenclature, already familiar from previous study, would easily bring known forms to mind. Details could then be added at their appropriate positions in the larger masses; and as the description thus proceeded, more and more warrant would be found for the conclusion that had been stated in the first place. Villages and roads, hardly known outside of their immediate districts, would be located as occasion offered with respect to the larger masses and their details, instead of *vice versa*. Even if an outline map is added to indicate route and local names, a separate figure, giving in a general way the graphic equivalent of the general conclusions, is of much service; for the route followed by the observer is a relatively subjective detail, and the local names too often only distract from the main description. It is chiefly through the general features that the distant reader can reach the smaller items.

This method might work injury where the conclusion remains in doubt; for the presentation of a doubtful conclusion at the beginning of an article would probably give it too high rank. The method might be inconvenient in cases where most readers of an article were on the ground, and therefore already familiar with local names. But in such a case as the one treated in the article reviewed above, the conclusion is surely safe enough to deserve presentation in systematic terminology in an opening statement; and the local readers are probably only a small minority of the many far-away students who will profit from Lawson's excellent work.

W. M. D.

REPORT OF THE GEOLOGICAL EXPEDITION OF HON. CHARLES H. MORRILL. SEASON OF 1906

The Morrill geological expedition of the University of Nebraska for the season of 1906 continued the work of the previous season by developing the bone quarry on University Hill, at Agate, Sioux County, Nebraska. This quarry is situated on the eastern extremity of Mr. James Cook's ranch, which is an extensive one, and probably the best known in the state. In addition to the uplands it contains some ten square miles along the valley of the Niobrara. The high bluffs adjacent to and beyond this model ranch are fossiliferous, while at Carnegie Hill and University Hill there are literal bone beds. The discovery of these beds was made some twenty years ago by Mr. James Cook. They were first visited by the Morrill geological expedition of 1892, when a considerable number of bones were collected, several of which have been figured in the 'University Studies,' January, 1897, plate 1, Figs. 5, 6, 7.

Pursuant to invitations from Mr. Cook, the Morrill geological expedition of 1905 spent that summer developing the bone quarry on University Hill. During the summers of 1905 and 1906 the members of the exploring party enjoyed all the privileges and hospitalities of this famous ranch. The members of the party for 1906 were: Harold J. Cook, Eck F. Schramm, Edwin Davis and Paul Butler, students in the University of Nebraska. As in former expeditions the writer was in charge.

By the judicious use of dynamite large amounts of overlying rock were removed and a broad surface of the bone-bearing layer exposed. A large number of bones, jaws and skulls were secured, all being for the most part in a fine state of preservation.

The prize specimens of the season were two large slabs cut from the bone-bearing layer and shipped bodily. They are literally packed with bones and jaws, which will be worked out but not removed from their original position. When done they will be placed on exhibition intact, to illustrate fossil bone beds. The bones of *Moropus* and *Diceratherium* are so abundant in this quarry that they far outnumber all else. Of the rare *Moropus* the Morrill collections now have enough material for a complete restoration. There was found to be considerable variation in the size of *Moropus* bones, some being of elephantine size. Of *Diceratherium* a great number of bones and jaws, but no good skulls were secured.

In August the writer, accompanied by Mr. Harold Cook, spent ten days exploring and collecting relics in and around the 'Spanish Diggings' west of the Rawhide range in Wyoming, where among other things over a thousand stone implements were procured. A few weeks later the writer again visited this spot in company with Dr. M. H. Everett and Edwin Davis at the invitation of Mr. Thomas Black of Willow, Wyo., who not only entertained the expedition in a most hospitable manner but provided teams and conveyance. A wide area was explored and many specimens and implements added to the previous lot. Later a third trip was made to this region by Dr. Everett, who secured additional specimens and data of value. A mild fall, free from the hindrances of rain and snow, has made the continuance of field work possible to the present date. Several days were spent by the writer, accompanied by Dr. George E. Condra, Edwin Davis and Paul Butler, excavating the mound recently discovered by Mr. Robert F. Gilder of Omaha, many human remains of a primitive order being secured. Among miscellaneous acquisitions of the year may be mentioned the skeletons of four modern elephants, camels, bear, etc., secured early in the season by Mr. Henry Eakin.

The Morrill collections will be moved into their new fire-proof quarters the latter part of December, and more than one hundred tons of material now boxed and stored in the basements of various buildings and the steam tunnels on the campus will be placed on exhibition. The benefactions of Hon. Charles H. Morrill make these expeditions possible, and it is planned to greatly increase their extent and scope each year.

Erwin H. Barbour
The University of Nebraska,
December 6, 1906

THE NATIONAL GEOGRAPHIC SOCIETY

Addresses have been arranged as follows:

January 18.—'Camping Expeditions in the Canadian Rockies,' by Mr. Howard Du Bois.

January 25—'Bolivia—a Country without a Debt,' by the Bolivian Minister, Señor F. Calderon. Illustrated.

February 1—'The Rising Pacific Empire,' by Hon. George C. Perkins, U. S. Senator from California.

February 8—'The Guianas,' by Prof. Angelo Heilprin, of Yale University. Illustrated.

February 15—'Ten Years of Polar Work; or, What We Know and What We Want to Know,' by Mr. Herbert L. Bridgman, Secretary of the Peary Arctic Club. Illustrated.

February 19—'Two Thousand Miles in the Saddle through Colombia and Ecuador,' by Hon. John Barrett, U. S. Minister to Colombia. Illustrated.

March 1—'Santo Domingo and Haiti,' by Rear Admiral Chester, U. S. Navy. Illustrated.

March 15—'The Regeneration of Korea,' by Mr. George Kennan. Illustrated.

March 21—'Our Immigrants: Where They Come From, What They Are, and What They Do After They Get Here,' by Hon. F. P. Sargent, Commissioner General of Immigration. Illustrated.

March 23—'Queer Methods of Travel in Curious Corners of the World,' by Hon. O. P. Austin, Chief Bureau of Statistics. Illustrated.

March 29—'Mexico—the Treasure-house of the World,' by Mr. N. H. Darton, of the U. S. Geological Survey. Illustrated.

April 5—'A Popular Explanation of Earthquakes and Volcanoes,' by Dr. G. K. Gilbert, of the U. S. Geological Survey. Illustrated.

April 12—'Captain John Smith and Old Jamestown,' by Mr. W. W. Ellsworth, Secretary of the Century Co.

Announcements will be made later of addresses by Commander Robert E. Peary, U. S. Navy, who has recently attained 'Farthest North,' and by Dr. F. A. Cook, of Brooklyn, who has accomplished the first ascent of Mount McKinley, the highest mountain in North America.

Scientific meetings will be held at the home of the society, Hubbard Memorial Hall, Sixteenth and M Streets, at 8 P.M., on the following dates:

January 11—Annual Meeting. 'Aboriginal Agriculture in Guatemala,' by Mr. O. F. Cook, of the U. S. Department of Agriculture. Illustrated.

January 16—'The U. S. Forest Service,' by Mr. Gifford Pinchot, Forester. Illustrated. The Forest Service has charge of 114,606,058 acres of forest land, worth $400,000,000.

January 22—'The Coal Lands of the U. S. Public Domain,' by Mr. M. R. Campbell, of the U. S. Geological Survey. Illustrated.

February 9—'A Visit to Sumatra,' by Mr. George H. Peters, of the U. S. Naval Observatory. Illustrated.

February 18—'Reclaiming the Desert,' by Mr. C. J. Blanchard, of the U. S. Reclamation Service. Illustrated. The Reclamation Service has a fund of $40,000,000, which is being invested in irrigation works.

February 22—'Reclaiming the Swamp Lands of the United States,' by Mr. H. M. Wilson, of the U. S. Geological Survey. Illustrated.

March 8—'Twenty Years in Beirut and Damascus; or, The Syria of Today,' by Rev. F. E. Hoskins. Illustrated.

March 22—'Utilizing the Surface Waters of the United States for Power,' by Mr. H. A. Pressey, C. E. Illustrated.

April 6—'The South Sea Islanders,' by Mr. A. B. Alexander, of the U. S. Bureau of Fisheries. Illustrated.

April 15—'Photographs of Wild Game taken by Themselves,' by Hon. George Shiras, 3d. Illustrated.

April 19—'A Trip to Argentine and Paraguay,' by Mr. John W. Titcomb, of the U. S. Bureau of Fisheries. Illustrated.

MEDICAL LECTURES AT THE HARVARD MEDICAL SCHOOL

THE faculty of the Harvard Medical School has arranged a course of free public lectures, to be given at the new Medical School buildings, Longwood Avenue, Saturday evenings and Sunday afternoons, beginning January 12, 1907, and ending May 12. Following is a list of the lecturers and their subjects, with dates:

January 12—'Unfavorable Conditions of Modern School Life,' by Dr. Robert W. Lovett.

January 13—'Deformities of the Feet from Shoes,' by Dr. Edward H. Bradford.

January 19—'Round Shoulders and Lateral Curvature,' by Dr. Robert W. Lovett.

January 20—'Costume Deformities in Growing Children,' by Dr. Edward H. Bradford.

January 26—'Bacteria in Health and Disease,' by Dr. Harold C. Ernst.

January 27—'Public Milk Supplies,' by Dr. Charles Harrington.

February 2—'Under What Circumstances should you send for the Doctor,' by Dr. George W. Gay.

February 3—'The Adulteration of Food and Drugs,' by Dr. Charles Harrington.

February 9—'Bacteria in Health and Disease' (No. 2), Dr. Harold E. Ernst.

February 10—'How the Common Infectious Diseases are Spread,' by Dr. George B. Magrath.

February 16—'The Care of the Sick Room,' by Dr. Elbridge G. Cutler.

February 17—'Public and Individual Water Supplies,' by Dr. Charles Harrington.

February 23—'Antitoxins and Vaccines,' by Dr. Theobald Smith.

February 24—'What Surgery can and cannot do,' by Dr. Maurice H. Richardson.

March 2—'The Present Epizoötic of Rabies,' by Dr. Langdon Frothingham.

March 3—'Disease,' by Dr. William T. Councilman.

March 9—'The Hygiene of the Ear,' by Dr. Clarence J. Blake.

March 10—'Disease' (No. 2), by Dr. William T. Councilman.

March 16—'Facts about Rheumatism,' by Dr. Joel E. Goldthwait.

March 17—'Care of Healthy Infants and Prevention of Disease in Early Life,' by Dr. Thomas Morgan Rotch.

March 23—'Some Points concerning Nursing in Scarlet Fever and Measles,' by Dr. John Hildreth McCollom.

March 24—'Tuberculosis in Early Life,' by Dr. John Lovett Morse.

March 30—'Some Facts the Public should know concerning the Feeding of Infants,' by Dr. Maynard Ladd.

March 31—'Significance, in Infancy and Early Life, of Disturbances of the Stomach and Bowels,' by Dr. Charles Hunter Dunn.

April 6—'Some Phases of the Tuberculosis Problem,' by Dr. Arthur K. Stone.

April 7—'Tuberculosis: Methods of Invasion and Dissemination,' by Dr. Theobald Smith.

April 13—'Pulmonary Tuberculosis,' by Dr. Harold C. Ernst.

April 4—'Eyesight and School Life,' by Dr. Miles Standish.

April 20—'Florence Nightingale and the Beginning of Surgical Nursing,' by Dr. J. Babst Blake.

April 21—'Food in Health and Disease,' by Dr. Franklin W. White.

April 27—'The Growth of Children,' by Dr. William T. Porter.

April 28—'Food in Health and Disease' (No. 2), by Dr. Franklin W. White.

May 4—'History of the Treatment of Disease,' by Dr. Maurice V. Tyrode.

May 5—'The Care of the Sick,' by Dr. John T. Bowen.

May 11—'The Hygiene of the Mouth and Teeth,' by Dr. Samuel A. Hopkins.

May 12—'The Physical and Mental Development of Children,' by Dr. James S. Stone.

THE U. S. GEOLOGICAL SURVEY

The annual report of the director of the Geological Survey for the fiscal year 1905–6 states that all work formerly carried on by the survey in the classification and mapping of forest lands other than topographic maps was transferred to the Bureau of Forestry prior to July 1, 1906. The making of the topographic maps of the reserves which are part of the area covered by the general topographic map of the United States will be continued by the survey, and the maps will be available for engineers, geologists, etc.

The report also contains the interesting information that the great Reclamation (Irrigation) Service was severed from the Geological Survey on July 1, 1906, except that the director of the survey continued to act as director of that service. The report further states that it is probable that an entire separation will occur at an early date, and we learn informally that all connection will be terminated between the two organizations before the close of the present fiscal year. Public policy demanded that the Reclamation Service should be organized under some strong existing bureau. This was done, and the service, now well organized, will soon be made an independent bureau.

Eighty-one members of fifty-one educational institutions in the United States were connected with the work of the survey in 1906, and over $200,000 was expended in this cooperative work.

The amount of work done for the survey by men connected with universities, colleges and technical schools was not proportionately as large as in the first decade of the existence of the survey (1880–90), a condition explained by the fact that it is the men who have been trained at the universities, colleges and technical schools who are now members of the permanent staff of the survey.

The publications of the survey are distributed without cost to 426 educational institutions in the United States. Of this number 120 received the maps, folios and topographic sheets.

SCIENTIFIC NOTES AND NEWS

During the presence in New York City of Dr. William H. Welch, professor of pathology at the Johns Hopkins University, to preside over the meeting of the American Association for the Advancement of Science, a testimonial dinner was tendered to him by fifty of his 'scientific friends and admirers in Greater New York.'

IT appears from the Year Book of the Carnegie Institution that Dr. William H. Welch, Dr. Henry S. Pritchett and the Hon. William H. Taft have been elected trustees. Mr. William Wirt Howe and the Hon. Wayne MacVeagh have resigned from the board. Dr. John S. Billings was re-elected chairman, the Hon. Elihu Root, vice-chairman, and Mr. Cleveland H. Dodge, secretary for three years. Appropriations for the ensuing year were made as follows:

Publication fund, to be continuously available	$70,000
Administration	50,000
Grants for departments and large projects	443,200
Grants for previously implied investigations, new minor investigations, and research associates and assistants	98,100
Total	$661,300

AT the annual banquet of the National Geographic Society the first award of its gold medal was made to Commander Peary.

THE international cup balloon race will be held at St. Louis on October 19. It is also announced that an aeronautic congress has been arranged in connection with the Jamestown exposition with Dr. Alexander Graham Bell as president.

DR. WILLIAM DUANE, professor of physics in the University of Colorado, at Boulder, has resigned to accept a position in the Curie Radium Laboratory at Paris. The fund providing for Dr. Duane's work is the gift of Mr. Andrew Carnegie.

DR. W. W. KEEN has been made professor emeritus of surgery in the Jefferson Medical College, Philadelphia, and expects to spend a year abroad. Dr. Keen will celebrate his seventieth birthday on January 19.

THERE was recently given at the Chemists' Club, New York City, a dinner to celebrate the twenty-fifth anniversary of the receipt of the doctorate of the University of Würzburg by Dr. William Hallock, professor of physics at Columbia University and dean of the faculty of pure science.

DR. BASHFORD DEAN, professor of vertebrate zoology in Columbia University and honorary curator of fishes in the American Museum of Natural History, has been elected president of the Society of Vertebrate Paleontologists.

AT the recent meeting of the American Economic Association, Professor J. W. Jenks, of Cornell University, was elected president.

DR. E. W. BENECKE, professor of geology at Strasburg, and Dr. A. von Koenen, professor of geology at Göttingen, have retired from active service.

FRITZ ZERBAN, Ph.D. (Munich), has succeeded to the place of Dr. C. A. Browne, Jr., as chemist at the Louisiana sugar station.

MR. J. B. MOWRY has been appointed commissioner of forestry in Rhode Island.

A RUSSIAN expedition for the exploration of the Arctic regions is being equipped under the leadership of Lieutenant-Colonel Sergeyeff. The expedition, which will last for several years, will start from Yeniseisk and try to reach Bering Strait.

DR. RITZ ROMER has been appointed director of the Museum of the Senckenberg Natural History Society of Frankfort.

PROFESSOR F. CAVARA has been appointed director of the Botanical Garden at Naples.

DR. H. R. MILL has been elected an honorary member of the Vienna Geographical Society.

THE sixth lecture in the Harvey Society course will be delivered by Professor Francis G. Benedict, of Wesleyan University, on January 12, at 8:30 P.M., at the New York Academy of Medicine. Subject: 'Metabolism during fasting.' The celebrated professional faster, Succi, who has been the subject of many classical experiments on metabolism, will be present.

MISS CLARA EATON CUMMINGS, Hunnewell professor of cryptogamic botany in Wellesley College, died in Concord, N. H., on December 28. Professor Cummings had been long identified with the history of the college. Entering as a student in 1876, a year after the first opening, she at once showed so marked a talent for the study of botany, especially for the identification of cryptogamic flora, that she was retained as a permanent member of that department, bearing the title of curator of museum from 1878 to 1879, and that of instructor of botany from 1879 to 1886. After a period of study in Zürich, Miss Cummings returned to the college as associate professor of cryptogamic botany. In 1905 she became Hunnewell professor of botany, with temporary charge of the department. In 1906 her title was changed to that of Hunnewell professor of cryptogamic botany, in recognition of the closely specialized work in which she had reached distinction.

The death is announced, at the age of sixty-eight years, of Mr. Jeremiah Curtin, known for his works on anthropology and travel and to the general public for his translations of the novels of Henryk Sienkiewicz.

Mr. John Ward, a well-known Staffordshire geologist, fellow of the Geological Society of Great Britain since 1874, has died at the age of seventy.

Dr. Arthur William Panton, fellow of Trinity College, Dublin, well known as a teacher of mathematics and a writer on this subject, died on December 18, at the age of about sixty years.

Dr. Thaer, formerly professor of agriculture at Giessen, died on December 14, at the age of eighty-seven years.

Dr. Ochsenius, a geologist at Marburg, has died at the age of seventy-seven years.

The deaths are also announced of Dr. K. O. Harz, professor of botany and pharmacology in the Veterinary Institute at Munich, and of Dr. Antonio Mascari, assistant in the observatory at Catania.

The U. S. Civil Service Commission announces an examination on February 6–7, 1907, to fill a vacancy in the position of scientific assistant in plant pathology, $1,000 per annum, Bureau of Plant Industry; a vacancy in the position of scientific assistant in animal bacteriology, $840 per annum, Bureau of Chemistry; and vacancies as they may occur in the Department of Agriculture requiring similar qualifications.

It is expected that the National Education Association will hold its fiftieth anniversary in Philadelphia, where it was organized in 1858. The department of superintendence of the National Education Association will meet in Chicago on February 26, 27 and 28. With it will meet the National Society for the Scientific Study of Education, the Society of College Teachers and the Educational Press Association of America.

A meeting was held on January 2 at the physiological building of the Johns Hopkins Medical School for the formal presentation and acceptance of two noteworthy collections of medical works given to the library by Mr. W. A. Marburg and Mr. Francis M. Jencks. The works given by Mr. Marburg are the old Warrington Dispensary Library of Liverpool, which is particularly valuable in the history of medicine. The library consists of about 944 volumes. The collection given by Mr. Jencks consists of 936 volumes, and is the Friedrich Ahlfelb library, of Marburg, Germany. It is a specialized library, consisting of writings on 'monsters.' Dr. Ira Remsen, president of the university, presided, and addresses were made by Dr. William Osler, regius professor of medicine at Oxford, and by Dr. William H. Welch.

Mr. Andrew Carnegie has given $750,000 for the construction of a building to be used by the Bureau of American Republics. Provision for the site already has been made by the United States and the South American republics.

At a special general meeting of the Royal Society of Edinburgh, held on December 21, the council presented a report on the new accommodation to be provided for the society in consequence of its proposed removal from the Royal Institution. It appears from this report, as quoted in *Nature,* that in March last a memorial was presented to the secretary for Scotland directing attention to the needs of the society, and asking for a free grant of £600 a year. In a semi-official reply to this memorial the general secretary of the society was informed that a proposal was being entertained by the government to devote the whole of the Royal Institution to the purposes of art, and that the Royal Society must contemplate the necessity for finding accommodation elsewhere. At it appeared from correspondence and an interview with the secretary for Scotland that the government had definitely decided to allot the whole of the Royal Institution for the purposes of art, the council resolved, with great reluctance, to accept the necessity for removal, and to do its best to secure adequate reinstatement. An accommodation committee was, therefore, appointed by the society to advise the secretary for Scotland regarding sites and buildings suitable for new premises for the society with the result that the committee unanimously recommended the building at present occupied by the Edinburgh Life Insurance Office, Nos. 22 and 24 George Street. At an interview on November 22 Mr. Sinclair offered, subject to the consent of Parliament, to purchase and adapt the George Street building on certain conditions, and in addition to give a free grant for the scientific purposes of the society. The conditions proposed were approved by the representatives of the society present as being, in the circumstances, an equitable settlement of the claims of the society. The arrangements are that a sum of £25,000 will be used for the purchase of a building, and £3,000 to cover the expenses of fitting up, redecorating the new premises, and transferring the library and other effects of the society from the Royal Institution. The treasury will also give the society a grant of not more than £600 a year.

At the last monthly general meeting of the Zoological Society, of London, Mr. Howard Saunders, F.L.S., vice-president, in the chair, 103 candidates were elected fellows. The report of the council for November was read by the secretary, Dr. P. Chalmers Mitchell, F.R.S. This stated that 173 additions had been made to the society's menagerie during that month—viz., 105 acquired by presentation, 23 by purchase, 35 received on deposit, three received in exchange, and seven born in the gardens. Amongst these special attention was directed to an adult male mandrill (*Papio maimou*), the first full-sized example of this species exhibited in the gardens, deposited on November 30; to a young female hippopotamus (*Hippopotamus amphibius*) from the Niger, purchased on November 1; to a Persian stag (*Cervus maral*), presented by Mr. Carl Hagenbeck on November 13; to a Kashmir stag (*Cervus cashmiriensis*), presented by the Duke of Bedford on November 22; and to a collection of 47 birds containing, amongst other interesting specimens, a green toucan (*Aulacorhamphus sulcatus*), new to the collection, and a sun bittern (*Eurypyga helias*) from Venezuela, presented by Captain Albert Pam, F.Z.S., on November 27. The report further stated that the number of visitors to the society's gardens during the month of November had been 22,025. The total number of visitors to the gardens during the year amounted to 881,018, showing an increase of 185,055 as compared with the corresponding period of the previous year.

We learn from *The British Medical Journal* that in addition to the Johnston Laboratories, the Liverpool School of Tropical Medicine is possessed of Research Laboratories at Runcorn, established some two years ago, because the Johnston Laboratories, although perfect for the purposes for which they were built, did not, being situated in a city, lend themselves well to researches demanding the keeping of a large number of animals, both great and small. Crofton Lodge at Runcorn, some sixteen miles distant, was therefore taken, some of its rooms converted into research rooms and ample stabling and pasture for all sorts of animals secured on a small adjoining farm. For the first year the chief energies of the newly-founded laboratories were devoted to the study of various forms of trypanosomiasis, but since September, 1905, the work has been mainly concerned with the spirochætes of relapsing fever. Research work proper is, however, only part of the function of these laboratories, another being the important task of keeping the Johnston Laboratories supplied with living parasites, for the instruction of the students of the School of Tropical Medicine. To this end, therefore, the trypanosomes of dourine, mal de Caderas, nagana, Gambian horse sickness and 'sleeping sickness' are kept constantly going in animals. Similarly, the spirochætes of 'relapsing fever,' and of mice (*Spirochæta laverani*), of 'African tick-fever,' and of fowls, are kept for the same purposes. The ticks, *Ornithodoros moubara* and *Argas miniatus,* which transmit the two last-named diseases, are likewise bred in the laboratories, and stocks of the *Irodes reduvius* and *Rhipicephalus annulatus,* those transmitting the piroplasms of 'red water' in cattle, kept. The equipment of the laboratories is not elaborate or showy, but everything is available which is required to permit full advantage to be taken of the material at disposal. The present director is Dr. J. L. Todd, who during the past twelve months has had some dozen research students working with him.

UNIVERSITY AND EDUCATIONAL NEWS

Mr. John D. Rockefeller has given the University of Chicago $2,700,000 for its permanent endowment, and $217,000 for current expenses and special purposes. Among the special provisions of this latter gift are: To provide permanent increases in the salaries of instructors, $40,000; for additional cost of drinking water systems, $21,610; for the improvement of the campus, $15,000; for the Alice Freeman Palmer chimes, $5,000; for special equipment in various departments, $5,000; for greenhouses for the department of botany, $2,500. Mr. Rockefeller's gifts to the University of Chicago are said to amount to more than $20,000,000.

At the opening exercises of the new year President Warfield, of Lafayette College, announced that $325,000 had been subscribed toward the $500,000 endowment which is being raised to mark the seventy-fifth anniversary of Lafayette College. Of this sum Mr. Andrew Carnegie has given $50,000 for a mechanical engineering course. He will give an additional $50,000, provided the half million is secured.

A gift of $50,000 from Mr. Andrew Carnegie is announced by President George C. Chase, of Bates College. Mr. Carnegie's offer of this amount stipulated that friends of the institution should subscribe $100,000, and this amount has been secured.

The Chemical Library of Harvard University has recently received from the class of 1881 an addition of $1,500 to the $3,000 previously given, thus making a fund of $4,500, the income of which is to be used for the purchase of books.

We learn from the *Experiment Station Record* that plans are being perfected for a jubilee week next spring to celebrate the fiftieth anniversary of the opening of the Michigan College, the first agricultural college in the United States. It is now proposed to hold the jubilee on May 28–31. A program is being arranged so that the delegates to the Association of American Agricultural Colleges and Experiment Stations, which it is hoped will meet in Lansing at that time, may attend all the more important sessions of the jubilee. President Roosevelt will speak on May 31, and the college is planning to entertain from 10,000 to 15,000 people on that day.

The *Harvard Graduates Magazine* gives a classification of the students according to the divisions of the faculty of arts and sciences under which their studies chiefly lie. The predominance of the languages and humanities continues marked; only chemistry among the sciences shows a great growth in recent years. Semitic, none; ancient languages, 28; modern languages, 99; history and political science, 85; philosophy, 31; education, 21; fine arts, 5; music, 6; mathematics, 24; engineering, 5; forestry, 1; physics, 10; chemistry, 33; biology, 15 (botany, 2; zoology, 13); geology, 7; mining and metallurgy, 1; anthropology, 5.

Mr. R. C. Bryant, in charge of the cooperative work of the office of Forest Extension, of the Bureau of Forestry, has resigned to assist in organizing the work in connection with the chair of practical forestry and lumbering at the Yale Forest School.

There is a vacant instructorship in chemistry at Lehigh University, paying $1,000 annually. Applications should be sent to Professor W. B. Schober, South Bethlehem, Pa.

Professor A. Moller has been appointed director of the Forestry School at Eberswald.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE PRESENT AND FUTURE OF THE AMERICAN CHEMICAL SOCIETY[1]

IN view of the far-reaching changes in our society that are to inaugurate the new year now at hand, it seems to me that no more fitting subject for an address on this occasion could be chosen than one which should present to our members a statement of its present condition, the reasons for the changes undertaken, the importance of these changes to all the chemists of North America, and the need of the loyal support of our present membership at the start in securing the structure of the greater edifice upon a firm and enduring foundation, so that a powerful organization may develop for building up the profession—an organization that shall be fully worthy of what may become, as has been predicted, the future stronghold of chemistry. This presentation seems specially called for at this moment, since the existing situation and what it is hoped to accomplish by the proposed changes are not fully known to a large proportion of our members, particularly that portion that is remote from the larger industrial and educational centers and unattached to our local sections. And

[1] Address of the president of the American Chemical Society, New York meeting, December, 1906.

since the future develops from the present, and it is the future, though inseparable from the present, that holds most of interest for us now, I will call my theme 'The Present and Future of the American Chemical Society.'

The desirability of assembling all chemists in a single strong organization would seem so manifest as to be beyond the need of argument for its support, and the whole drift of the present address will conform to this postulate. In the words of a correspondent, "If the profession of chemistry is to reach and maintain the distinguished position which it ought to have in this country, it must do so by presenting a united front and combining in its membership practically all who are engaged in any branch of chemical work." And again, "A person who joins the American Chemical Society should not have in view solely his personal interests. He should first have in view the interests of the profession. His membership is a patriotic duty and not a personal perquisite."

But without specialization few advances would be made. Specialized societies are, therefore, excellent and to a certain degree necessary—and to this point I will revert later—but it would seem as if they would be most effective as adjuncts of a major organization which should include all chemists. It is a mistake to think, as some apparently do, that the chemist in one line of work has little to learn from those laboring in different fields. All owe their success to the application of the same fundamental facts and theories, and discovery in one field may have most important bearing at a point seemingly remote. Deep specialization is vital and inevitable; but he who while performing his own particular task at the same time endeavors to keep in touch with the general trend and progress of the important work in his profession will be in advance of the man who reads none but his own special journals and waits for the text-books and manuals to bring him the belated knowledge which might have been his much sooner.

The foregoing lines were written long before the opening of the new Harvard Medical School on September 26 of this year, and it was, therefore, pleasing to read in the address of Dr. Wm. H. Welch, delivered on that occasion, the following expression of his opinion: "Specialization * * * is demanded by the necessities of the case and has been the great instrument of progress, but the further division is carried, the more necessary does it become to emphasize essential unity of purpose and to secure coordination and cordial cooperation of allied sciences." This was said with particular reference to the many specialized branches of medical science.

Again, from the same address: "How disastrous may be to medicine the loss of the sense of unity in all its branches has been clearly and admirably shown by Professor Allbutt in depicting the effects which for centuries followed the casting off from medicine of surgery as a subject unworthy the attention of the medical faculty. Thereby internal medicine lost touch with reality and the inductive method, and remained sterile and fantastic until the day of Harvey, Sydenham and Boerhaave."

That the application of these statements to our own profession will be self-evident to most of its followers is my hopeful belief.

Such a general organization as that alluded to, broad in its scope and progressive in its administration, not only maintains and adds to the dignity of the profession at large, but also helps the individual in more ways than one. The opportunities which it affords by its general meetings for seeing, hearing and becoming acquainted with the foremost men in all lines of work appertaining to the profession, are not to be had elsewhere. The chances for young men to become known of their fellows are equally good. The young member may, it is true, at his first or second attendance, feel somewhat lost and out of place, but if his countenance is seen frequently, and especially if he soon presents a paper of merit, he becomes known, makes acquaintances wherever he wishes, and derives from scientific conversation and social intercourse with his fellows the full and stimulating benefit these meetings are intended to afford. I know of few greater satisfactions than the talks over problems and difficulties with a worker along like or kindred lines, all the more, perhaps, if he chance to be a new acquaintance. Narrow-mindedness and intolerance are evils which are mitigated by rubbing up against one's co-workers and finding out their different points of view. The suggestions and helps to be given and received constitute benefits to be derived from these meetings whose value is properly appraised by those who know how to profit by them, but is by too many, unfortunately, much underestimated. The older men can contribute greatly to the success of such gatherings if they come prepared to meet more than half way the perhaps diffident approaches of the younger generation and ready to part freely with information and hints drawn from their long experience. I believe this is the spirit in which most, if not all, of the older men do come to these meetings, but the younger ones must be reminded that although their seniors may have the best will in the world to help, they can not possibly do so unless they know who need help. Therefore, the younger members must not be backward about making themselves and their wants known.

In all of these ways the reorganization of the American Chemical Society in 1890–3 did much to forward the cause of chemistry in our country, aside from providing an organization representative of the profession and a medium worthy of respect at home and abroad. The low dues which it seemed necessary to levy at the start and the lack of an endowment fund, however, made it impossible to provide the needed detailed information in all branches, even in the form of abstracts relating to the advances of chemistry in the world at large. It was possible to cover only the home field, and that not fully until 1902. Notwithstanding that this field has been well covered in the main since that date, the inability to offer more has been a most serious obstacle to the full development of the society, one which, if allowed to operate too long, must inevitably lead to its deterioration and eventual disintegration. For our members all need to know what is being done abroad. Through our own inability to afford this knowledge, they must seek it in foreign publications, which, besides covering the foreign field, give accounts of much of the work originating here. If our members have not access to certain public libraries or to those at educational centers, their only recourse is to subscribe at considerable expense to other publications than that of their own society. To the young worker, this additional sum, while absolutely inconsiderable, is sufficient oftentimes to turn the scale of his indecision in favor of the foreign publication or society as against the local one which patriotism would naturally lead him to prefer. The result has been that while our society has continually prospered and increased numerically, until its membership now exceeds 3,000, making it perhaps the third among existing chemical societies, it has failed to attract to itself anything like the number that properly should be attached to it. The total number of chemists in this country is probably fully double that enrolled in our membership. It has even been placed at 8,000. Of these some, but by no means the larger number, belong to other organizations. Very many are unattached. For one reason and another, we have failed to attract these, most of whom are unquestionably connected with the technical industries. And it is among these technical members of our society that a spirit of discontent has been most pronounced. Numerically they constitute probably a majority of its membership. That this discontent should exist particularly among technical men is not surprising when we consider the conditions under which they work as compared with those of the educators and their students. The latter either possess themselves, or have access to, all or much of the world literature that they need; the former are far less favorably situated in this respect, and it is to them of the utmost importance that their society should provide through its publications the power to meet all ordinary emergencies and to rise above provincialism.

The spirit of discontent to which I have referred has been manifest for some years. It has not been due altogether to the want above specified, but is in part chargeable to certain other conditions to which I shall refer later. It has manifested itself thus far in the formation of new and independent societies of specialists and in the growth of others already existing. The fact that these organizations have come into being and find continued support and that new ones are contemplated is evidence in a general way that they are needed, or that our own society does not offer all that it should. For the best interests of the profession at large, however, it seems very unfortunate that its power for good should be diminished by degrees in this way, for it is undeniable that for chemistry as a whole much more can be accomplished through union of all chemists than through many independent organizations, some weak, few strong, the weaker ones fated to a hard struggle and destined to accomplish unaided far less than they might as members of a strong general organization.

As a first step toward improving the situation, it was early recognized that we should endeavor to provide for our members complete abstracts covering the whole range of chemical literature.

In the matter of chemical abstracts taken as a whole throughout the world, it has long seemed to many that there was an excessive waste of valuable time, labor and money, in their preparation. The same work is repeated in several languages and sometimes more than duplicated in a single tongue. Cooperation on an international scale, each country preparing its own abstracts and exchanging with other countries through a sort of clearing-house bureau, would obviously be the ideal solution of the problem, but quite as obviously an ideal that would meet with difficulties too great to be overcome at one stroke. It would be simpler to begin by securing cooperation between English-speaking countries, and earnest efforts on our part have been put forth with this end in view during the past few years. It is unnecessary to follow here the steps taken, or even to outline the tentative propositions formulated and urged, chiefly by our editor. Suffice it to say that success was not achieved, and it became clear that we should have to rely on our own unaided efforts and add to the existing duplication of abstracts if we wished to approach the ideal in our publications and make them of value to all chemists.

This end is attainable with sufficient money at command and with the right minds to plan and organize. The last condition is more readily met than the first, without which, however, success is beyond reach. Recognizing this clearly, the council a few years ago directed the appointment of a committee on endowment fund, which should canvass the situation and endeavor to devise means for the creation of a fund to aid the abstract project without rendering necessary any, or, at any rate, a great increase in the society's dues. The committee's efforts have thus far been without effect.

Less than a year ago one of our technical chemists wrote me with regard to the feeling and attitude of industrial chemists towards our society. The letter was decidedly pessimistic in tone, and the writer claimed to voice a feeling which was only partially justified by the facts as to some of the causes of dissatisfaction enumerated, and one or two of its statements were founded on misapprehension. It contained, however, much food for thought, coming as it did from one claiming to know the sentiment in his section of the country among men in the technical industries, who constitute, as before said, probably the larger part of our membership. Inquiry in various directions, chiefly in the large industrial centers, confirmed the charges in their main features and showed the existence of a wide-spread feeling that demanded immediate recognition.

The council directed the appointment of a committee of six technical chemists, with the president as an additional member and chairman, which should reduce to specific terms the wishes of this large body of men, so far as they could be ascertained, and present them for consideration to the council at the Ithaca meeting. This was done. The committee was selected with a view to securing the advice of men prominent in the technical branches, men of acknowledged reputation, some of whom had been active in expressing dissatisfaction with existing conditions, yet supposedly open-minded men of ideas, able and willing to look at the question fairly from all sides and hence free from narrow prejudice. The committee consisted of the following members in addition to the chairman: G. E. Barton, Geo. D. Chamberlin, A. D. Little, J. D. Pennock, T. J. Parker and W. D. Richardson. It may be said that the bringing together for this purpose, from widely separated parts of the country, chiefly from the great industrial centers, of men representing varied industries, has been and will be productive of good results in more ways than one. By interchange of views among themselves, and by conversation with others, they were enabled to ascertain the feeling in different sections of the country, and by contact with the governing body in one of its sessions, any possibly preconceived opinions regarding the hostility of that body or of individuals in it toward the technical side of the profession surely became dissipated. They were able to return to their homes and to disseminate a truer view of the situation than had before existed.

Before taking up the recommendation of the committee it will be well to review in detail some of the causes of discontent that were found to exist, as well as portions of the discussion arising from their presentation.

These were mainly set forth in the letter from the correspondent above referred to and related almost entirely to the character of the *Journal*. It may be said at the start that in the search for complaints, almost no objection was raised to the contents of this publication. It was freely admitted that what it contained was of good quality and there was little, if any, complaint that worthy matter had ever been rejected. The opinion was held in some circles that much of the matter emanating from government laboratories and experiment stations, which finds duplication in national and state publications, might be omitted in favor of matter less favored in this respect. The chief criticism related to omissions. It was held that probably the majority of the society membership followed the technical industries and, therefore, should receive an even share of consideration in the make-up of the *Journal,* whereas by far the larger space was devoted to contributions from, or of chief interest to, those outside the technical branches.

As a corollary to this criticism, it was maintained that the technical side of the profession was inadequately represented in the governing body of the society, and quite as much, if not more so, in the control of the *Journal.*

It would seem that the remedy for this alleged lack of due representation in the council lies largely in the hands of the complainants. It is well known that a sufficient number of votes in nomination of councilors from local sections or of councilors at large to entitle the candidates to recognition in the final ballot, can be procured only by prior agreement among a considerable number of members to unite upon certain suitable persons. As this agreement is most readily reached where many members live in close proximity, it follows that the larger sections can and do exercise a controlling influence in the choice of councilors at large. Most of the larger sections are located at centers of great industrial activity, and industrial chemists doubtless constitute a majority of their membership. The remedy is, therefore, apparent, if unison in choice is attainable. It must be borne in mind, however, that in order that good ultimate results should follow concerted action of this kind, no spirit of class feeling should be allowed to dominate in selections made either on the part of technical members or of those following science in the fields of education or pure research. The spirit of broad fraternity should have free play and both sides should be willing to recognize the rights of the other and to select from the ranks of either the most deserving, irrespective of their particular fields of work and without seeking merely numerical preponderance. It goes without saying that the present dominating influence of one side in the council, so far as numbers go, has not been the result of intention, but has come about mainly from natural conditions. Several of the local sections are located at centers of educational activity where relatively few members are industrially engaged, the result being that educators naturally represent those sections in the council. In this connection, the policy should in general be consistently followed of electing as councilors at large those who through their works have become widely and favorably known to the profession, thus reserving these positions as rewards for repeated achievements of a high order.

The written discussion brought about by the above reference to the paucity of original contributions from technical chemists deserves some consideration by itself. The original critic asserts that the controlling class, composed of professors and government and experiment station chemists, finds its needs fully met by a journal of the "type of that of the London Chemical Society and feels that the admission of most other matter would be lowering the grade of the journal, while the other class needs a journal like that of the Society of Chemical Industry," and that 'the only way to retain the two classes of members is to give the publication a dual character' with separate editorial boards. Coupled with this latter suggestion is the question whether a somewhat different standard should not be applied in the consideration of papers submitted for publication, in order to meet the wants of a class of chemists doing routine work, to whom chemistry, in the words of several high in technical circles, is a 'trade, not a profession,' whose chief demand is for 'methods and many of them' relating to their restricted work only, and who will not avail themselves of general literature, though it may be freely offered for their use. The problem as to this class is difficult, and one writer sees no solution 'except through a selected membership and increased cost of the *Journal*,' for 'printing more high-class technical papers would not help the matter much.'

A further quotation from the letter already mentioned may be given to show the feeling of the writer at the time, and, as he claims, of others, and as bearing on the question of a fair standard of excellence for papers submitted for publication. "If a man attains a chief professorship in a small college or a junior professorship in a large university, I doubt very much whether he has exhibited any higher ability than the technical chemist who maintains himself in a position where he has approximately the same number of assistants," a belief which is perhaps justified. He goes on to say: "Either a just scale of relative attainment must be recognized or two societies must naturally result, and I am convinced that the narrowness of a large portion of both the industrial and teaching chemists of the country will finally compel a separation, whatever is done. However, if an attempt is to be made to keep what is two societies in other countries in one here, I think that an independent committee composed of works chemists alone should be appointed to investigate the whole subject of inducements to works chemists to contribute to the *Journal* and to formulate general principles as to what should and what should not be considered high-class technical matter. * * * If the colleges would take the initiative and develop some plan of cooperation similar to that proposed by Mardick on pages 133–138 of the *Chemical Engineer* for January, 1906, there would be more hope for the American Chemical Society getting along without a split."

It is doubtless true that the feeling of narrowness referred to in the foregoing extract exists to some slight degree among both educational and industrial chemists. There is among a small section of those connected with educational institutions a preference for a society based on a high-grade membership and this feeling finds its counter expression among some technologists who are so shortsighted as to think they have no need for the worker in non-technical lines. The feeling is, however, far less strong than it was a few years ago, and is destined, I think, to complete eradication. To this end, the best efforts of the conservative members of both classes should be persistently and strenuously directed. It should become the fixed policy of the society to foster cordial relations among its members and to manifest a spirit of the broadest liberality in the enactments of its governing body. At the same time, those engaged in industrial pursuits must always remember that although they equal in number the educators and their students, or may even form a majority of the society, it is none the less true that the former are and no doubt will continue to be the greater producers of new and original matter, and hence be deserving of greater consideration in proportion to their numbers than those who are less productive. It is the new in chemical science that makes possible industrial advance, therefore the fullest consideration should be shown those who by their discoveries in pure science may thus be laying the foundations for future industrial enterprises. It should be immaterial whether this comes more largely from one class of workers than from another.

On one point referred to in the discussion—that relating to the unwillingness of some works chemists to avail themselves of opportunities offered them for acquiring a broader knowledge of their profession through its current literature—little need now be said. It is a situation that will ever exist so long as human nature remains what it is and educational institutions graduate low-grade men without other ambition than to earn a bare livelihood and unwilling to endure the arduous labor that must accompany earnest efforts to climb the ladder of success.

The question of applying a different standard for different classes of papers may also be left for the present. It is a point on which the committee on relations of the society to technical chemists made no recommendation. It is also a question of much delicacy, the solution of which will work itself out in the next few years, as the development and differentiation of the society's publications progresses.

The criticism that a majority of the articles appearing in the *Journal* of the society are of a non-technical character and that the educators and government chemists, through their control of the *Journal*, crowd out articles of a technical character, is as to the second point quite incorrect, and as to the first, open to a very simple and natural explanation. No article of merit has ever been intentionally declined by the committee on papers and publications, and those of a technical character have always been welcomed. It is possible that in a very few instances mistakes of judgment have been made, but this applies to papers relating to other lines of work as well as to those offered by industrial chemists. But even so, this is only an inevitable result of the fallibility of human judgment.

I have taken the trouble to tabulate the contents of the *Journal* for the eleven years preceding 1906, dividing the papers into three classes, namely, those relating to (1) agriculture, biology, etc.; (2) pure chemistry; (3) analytical and applied chemistry. No two persons would prepare identical lists because of the difficulty in classifying many of the papers, especially those on the border between agriculture, biology, etc., on the one side, and applied science and analytical methods on the other; therefore, I will not reproduce the table. Its main features, however, may be indicated, confirmed as they are by those of a similar table prepared by the editor, but covering only the years 1895, 1900 and 1905. It is shown that the agricultural and biological branches taken together stand about where they did eleven years ago, without appreciable increase, so they need not be further considered. It is further shown that although the number of papers in analytical and applied chemistry has increased in the last semi-decade, there has been a marked decrease as compared with the number published eleven years ago, and that the increase in papers relating to pure chemistry has been very marked in the last four years, but had experienced no increase in the seven years preceding and had undergone but slight fluctuations during that period. It may be said that some of the papers in analytical and applied chemistry listed for 1895, the year of greatest productiveness in those fields, were quite brief and unimportant, yet there has evidently been an absolute decrease as to number, though perhaps an improvement in quality. Whether the decrease is in any way attributable to the application of a higher standard of requirement can not be determined with certainty. It is most likely that the chief cause is to be sought in the inducements offered by the columns of journals devoted wholly to the applications of chemical science. On the other hand, the marked increase in papers devoted to pure science, so called, finds its chief and natural explanation in the rapid growth of our educational institutions and of the facilities for prosecuting research therein. This leads to a consideration of a further cause for the comparatively slight increase in technical papers in the last five-year period. The student and his instructor have absolute freedom in the matter of publication, subject to the one limitation of a satisfactory standard. The industrial chemist, on the other hand, is hampered and restricted by the unwillingness of most employers to make public the discoveries originating in their laboratories. This policy is apparently quite as firmly adhered to at the present day as in former times, and acts as a very real deterrent of publication of the work of chemists thus employed. Their discoveries are not their own property, and if made public often appear in the guise of patent specifications, in which are embodied the results of a vast amount of chemical work, much of which never gets into print in any other form. For this reason, such comparisons as are shown by the table above referred to may not be altogether fair to pure science. A just comparison should take account of the work embodied in the ever-increasing number of patents applied for. If this could be accurately determined, the ratio in the quoted table might not seem at all disproportionate. Nevertheless, there has been a marked disinclination on the part of many technical chemists to publish in our *Journal,* and this disinclination it is the purpose of the management of the society to remove if possible. How this is to be done will now be considered.

From the foregoing it is apparent that the chief cause acting against the fullest development of our society is the dissatisfaction of a large number of our technical members with present conditions, which chiefly relate to the *Journal* of the society.

The committee already referred to as having met at Ithaca last June for the purpose of formulating the wishes of the industrial chemists, offered a number of suggestions which have already been published in the *Proceedings,* pages 57 and 58. These received the favorable consideration of the council, at whose deliberations the technical members of the committee were present by invitation, and they are in substance as follows:

1. That publication in full of all official methods adopted by official organizations the world over be made in the *Journal.*

2. That the present reviews, covering the general advances in chemistry, be continued.

3. That for the benefit of that class of technical chemists whose environment tends to cause them to regard chemistry as a trade rather than a profession, 'an attitude which can not fail to react unfavorably upon the individual and the profession at large,' something may be accomplished by the publication of high-class articles from recognized authorities, treating of the advance and outlook in the different fields of chemistry. These will differ from the reviews heretofore published and to be continued in that they shall deal with broad conclusions and the trend of thought, and be so written as to be of general interest and calculated to keep all members in touch with the more important developments in the different fields of chemical work.

4. That the foregoing articles shall be supplemented by other general statements, similar to those that have from time to time appeared in the *Journal,* showing the progress, from a chemical standpoint, of special industries. By endeavoring to obtain these through direct application to manufacturers' associations covering special industries, it is thought that closer cooperation between manufacturers and the society, of mutual benefit to each, might be secured.

5. That the whole field of chemistry the

world over be covered by abstracts, so far as the financial condition of the society will permit, and that to meet this extension an increase of dues be made, if necessary.

6. That an earnest effort be made to secure a greater number of technical papers from technical schools, with special consideration of the propositions of Mardick in this direction already referred to.

7. That the committee on papers and publications be increased by the addition of two technical chemists.

The last of these recommendations goes into effect probably with the coming year. Numbers 1–4 and 6 will doubtless be followed, as heretofore in the case of those already in operation, and gradually adopted in part if not wholly in the others. The remaining and most important recommendation, that for abstracts to cover the foreign as well as home field, had already been practically decided on by the council after careful review of the ground by the editor, and merits extended consideration.

Few, except those who have been in close touch with the working out of such an undertaking as an abstract journal to cover all languages, can realize the magnitude of the task, the multiplicity of points to be taken into account, and the results of neglect to pay due regard to matters that at first thought may seem to be of minor moment. First there was the form of publication to be considered—that is, should it conform to the model hitherto followed, or should the journal be divided into sections, each devoted to a special branch of chemistry and followed by the abstracts pertaining to that branch, or should a special abstract journal be issued; and whatever the decision in this regard, should the publication or publications be issued at monthly or semi-monthly intervals. Coupled with these matters were those relating to size of page and cover, color of covers and inserts, matter to appear on the several pages of cover of one or both publications, advertising rates, subscription price, the drawing of specifications for the guidance of bidders and of a contract for the successful one.

While these questions were still under consideration a plan had to be carefully outlined for collecting and properly classifying the abstracts, involved in which were minor details of abbreviations to be used, both for titles of papers and of certain frequently appearing data in the text, of directions for the guidance of abstractors, etc. A list of journals, proceedings of societies and other publications must be carefully prepared and arrangements made to secure these by exchange or otherwise if the editorial office did not already have access to them. Then, most important of all, came the selection and securing of a competent staff of abstractors, with suitable men to take charge of special divisions and to be responsible for the work of those associated with them, for many fields are far too wide to be covered by a single man. This question was rendered specially difficult by the lack of eligibles who are acquainted with some of the less familiar foreign languages. Hence the need in a few instances of correspondence with distant lands in the hope of securing, if possible, some one there competent to attend to the publications of his land and able at the same time to write abstracts in English. A further element of difficulty here arose, that few, if any, are sufficiently at home in all fields of chemistry to be able to prepare satisfactory abstracts in them all, a difficulty which was also encountered in those cases where a competent man is available here at home for certain fields of work in a little known foreign tongue, but not in others, and the man for these others is not to be had. Often, too, appeared the need for finding some one in a particular city where alone, perhaps, certain publications are to be had that are likely to contain matter of interest to chemists. Particularly is this true in my own field of mineralogy and geology. Very many of the publications to be drawn upon, if this field is to be properly covered, are accessible only in the city of Washington, and most of these only in the library of the geological survey.

It will thus be seen what a task devolved on the editor of the *Journal* when the publication of an abstract journal was decided on. The time will undoubtedly come when the society must employ, at a good salary, a man whose whole time can be devoted to editing the society's publications, for they will demand all of his time. An alternative would be to continue the present plan, but to relieve the editor of all but supervisory labor by furnishing him with adequately paid and competent assistants. As it is, the editor has been obliged to engage an associate to share with him the arduous labors that have fallen upon his shoulders. And here I can do no less than testify to the patient, unwearied efforts and painstaking care bestowed by Dr. Wm. A. Noyes in planning for the new publication. This I can do the more authoritatively from having been in active cooperation at close hand with him throughout the past year. He has been heartily aided in the way of advice on many knotty points by the members of the committee on papers and publications and the present corps of abstractors, as well as by others, but the great weight of the task has devolved upon him, and I trust that our members will give him due credit for what appears to them good in the results attained, and withhold judgment if there be defects. That there will be mistakes to rectify and omissions to make good is to be expected, for such an undertaking can not spring into full fruition at once; it must be given time to develop; premature judgment must not be pronounced.

I will not present to you the divers reasons that influenced the decision of those in charge of the matter in their choice of the form the society's publications should take. The result has been made known to all. I will, however, briefly repeat that the *Proceedings* and *Journal* of the society will appear monthly as heretofore, and will include reviews of all kinds. The abstract journal, to be known as *Chemical Abstracts,* will issue semi-monthly and will be given up wholly to abstracts. These abstracts will naturally not be so full or so complete as is desirable, but the best will be done that our means allow and 'improvement' will be the watchword from year to year. It is altogether probable that we shall finish the first year or two with a deficit, hence the obvious need that our present members should stand by the society and endeavor to add largely to its numbers. A large proportion of our membership—that engaged in educational centers—has access to existing abstract journals covering all fields of work. They are hence, as a rule, in no special need of additional abstracts, and I wish my technical friends to bear this well in mind and to give due credit to these many members who, although their wants are largely met by existing conditions, have yet readily consented to an increase of dues and in many cases have taken on themselves the preparation of abstracts out of sheer loyalty and a desire that those who are less fortunately situated may reap the fullest benefit that the society can offer. I can not refrain from mentioning also the fact that in some quarters where at least indifference might have been looked for, not only was this feeling not apparent, but, on the contrary, the proposed changes excited a satisfaction, I might almost say, enthusiasm, that was in the highest degree encouraging.

I have said that while we shall aim to cover the whole field of chemistry, the work can not be done so fully or in such detail as we could wish, simply for the reason that our means are still insufficient. It will be necessary to condense in some fields, except as to articles that appear in less known languages and in the less accessible publications. Unimportant articles and those which relate to local matters of little or no general interest, or which are of a statistical nature, will receive but brief notice or be mentioned by title only. At the start, it will be impossible to cover the ground fully because of the enormous number of publications concerned and the inaccessibility of many of them. But as the undertaking becomes systematized and the society grows—and it is my firm belief that it will rapidly grow if the present membership gives it the loyal support asked for and thus supplies the means for development—the weak spots will be gradually mended. To adequately cover the ground, however, a far larger fund than that afforded by the increased dues must be available. If the present experiment is successful in accomplishing the ends aimed at, it is felt that we can go before the public that is able to give and ask for large contributions to a permanent publishing fund with far better grace and prospect of success than was possible before we had shown our willingness to help ourselves as far as circumstances permitted.

If the project for a Perkin Library in the city of New York, with its salaried staff, becomes a reality, it will help much to supplement a lack of fullness that our abstract journal may show, for many of our educational centers are weak in library equipment and very many of our industrial workers are far removed from libraries of any kind. It is proposed that the Perkin Library shall be in duplicate, so far as possible, so as to permit the loaning of books to applicants in any part of the country, and also the furnishing of more detailed information as to the contents of any particular article or series of articles, which our abstracts might fail to afford.

Having thus outlined the somewhat critical situation in our society and the steps thus far taken to counteract tendencies fatal to the society's fullest development, let no one cherish the thought that other difficulties will not arise that will demand the most careful leadership. The chief of these, perhaps, can be readily foreseen, for it has already manifested itself in the formation of independent specialized societies. It is the belief of myself, and probably of many others, that this tendency is one which in itself should not be unduly restricted, but that it should be guided along lines most conducive to the welfare of all concerned, that is to say, the differentiation of the society into special sections should be regarded as a result desirable in itself because inevitable and therefore to be considered as a part of the general policy of the society, to be kept constantly in view and acted on as circumstances demand. In the opinion of most of our members, the time is not yet ripe to inaugurate such a policy, though circumstances have arisen which may make it highly desirable to begin very soon. Premature departure in this direction on a large scale, however, would be ill-advised and unsafe. The society must first be placed on a basis so firm and secure that success will be assured from the start. It is necessary to gather into our fold a much larger membership than we now have, and no step that we can take to bring about this increase will, in my opinion, be more effective than the formal expression by the society of its adoption of this intention as a cardinal article of its fixed policy. A natural result would be that existing specialized societies would in all probability consent to become members of the greater organization.

This consolidation of independent specialized societies is wholly in agreement with modern evolution and has been accomplished already in other fields. The American Medical Association is such an organization of affiliated societies of all branches of medicine, and arrangements are in progress not only to bring together in a similar union all the medical societies in London, but further to add to the number by the formation of new sections as occasion arises. The degree of independence to be accorded the subordinate sections may well be left to future determination, but some such plan as that proposed for the London societies seems entirely feasible and proper. According to this each section shall be self-governing as far as possible, and shall have direct representation in the general council and editorial board, but its expenditures shall be subject to the control of the financial committee of the general society, and, further, the general management shall be controlled by a council consisting of the president, the presidents of the sections and the usual associates.

When such an organization is accomplished by us, it will doubtless be with the extension to the sections of the right to hold meetings when and where they please, limited only by the condition that one meeting in each year shall be in association with the whole body of affiliated societies. A preliminary step in this direction, which seems to work well, has been the adoption at our meetings of the practise of holding sectional meetings in addition to the general concourses participated in by the society as a whole. A prominent feature of these general gatherings might be the presence by invitation of one or more distinguished foreign guests. This feature would, I think, contribute not a little to the success of the meetings in more ways than one. It might, for instance, attract the attention of more of our countrymen at large to the importance and dignity of the science, and arouse for it and its aims a degree of interest and respect that sadly needs stimulation.

It is not to be understood that in the proposed differentiation of the society along these lines, our present local sections would be dispensed with. They fill, and doubtless will continue to fill, a most important field of usefulness.

Inseparably connected with this differentiation into sections is that of the eventual gradual development of the journal of the society, a project which should become no less a part of the general policy of the society than that of the formation of sections. As the society grows and chemistry develops, a single journal will become more and more cumbersome and unwieldy, and division must result. A short step toward this end has already been taken in the arrangement of the papers we now publish, all those relating to the major subjects being grouped together in each number of the journal; but this limited separation will soon fail to meet our needs and separate journals devoted to special subjects will be a further step in evolution. It is far better that these should be published by one powerful central organization than that they should be independent, for by reason of the greater economy resulting from centralized management more can be accomplished and offered than by the independent efforts of unaffiliated societies. One correspondent writes, "I bebelieve that natural evolution of the American Chemical Society will be along these lines, namely, the organization of special societies with special journals, as affiliated bodies of the American Chemical Society receiving the common journal. The specific articles and abstracts relating to each individual branch will be found in a special journal.'' It is, of course, implied that so long as the members in a special field are too few to warrant the publication of a special journal, the papers relating to those fields would continue to appear in the common journal, which would otherwise be devoted to the proceedings, reviews and papers of general interest. It may, however, be seriously questioned if the separation of abstracts in the manner indicated would be advisable. It were, in my opinion, far better that these should continue to appear in a special abstract journal which should, like the common journal, go to all members. Those desiring could then subscribe, for a moderate sum, to such of the special journals other than that of their special field as they would feel able to afford.

Less closely related to the future of the society as a society is a matter—already laid before the council at Ithaca, but tabled without action—that must, however, sooner or later engage attention. This has to do with the question of compensation paid for chemical services and the reflection that is cast on the profession at large by the utterly inadequate recompense that is commonly offered and, of necessity, accepted. This is far below what the properly educated chemist should receive, considering the time and money spent in acquiring his education and the extent and variety of the knowledge that he must master at the start and accumulate so long as he practises his profession. In so far as this question has to do with regularly salaried employees, the solution is perhaps more troublesome than in the case of those performing special services, such as analytical work, and that phase of it I shall not consider. A serious obstacle to be overcome before analytical chemists can occupy the position in public estimation that should be theirs, is that presented by shysters, posing as qualified chemists, particularly as analytical chemists, who seek and accept work at rates so low as absolutely to preclude accuracy in the results they obtain, so that the whole profession suffers for their misdoing. That these men are able to attract customers may be due to the greed of both employed and employer, but is in larger measure due to the ignorance of the employers as to the real nature of chemical work. It will be difficult, if not impossible, to educate the public in chemistry, but it would seem as if a partial solution might be reached by establishing some such standard of efficiency as that represented by membership in the Society of Public Analysts in Great Britain, which should be a guaranty, so far as such things can be guaranteed, that the member is a duly qualified analyst. Such an organization might well be established here, and it need in no sense compete with any other existing organization, nor need it call for more than nominal dues. Its membership should be most carefully guarded and any member showing himself unworthy should be summarily dropped. Such action would be *prima facie* cause for similar action by the American Chemical Society with respect to the culprit, if he possessed membership in that society. Membership in that organization would then be a reasonable guaranty to those wishing work done that it would be well done, and the rates of compensation could be maintained at figures commensurate with the quality of the work. Those patronizing non-members would then have but themselves to thank if disappointment and loss resulted from so doing, and they would soon learn, as so many others have learned, that cheap work is usually bad work.

In the foregoing, I have endeavored to outline the present situation with its existing complications and some of the suggested remedies, in so far as concerns the future of our society considered by itself. It may be permitted me, however, to look still further into the future, and to see therein the possibility of a yet wider affiliation than that already discussed, that of the several grand divisions of natural science, on a basis somewhat different from that now obtaining in the American Association for the Advancement of Science, an affiliation in which the American Chemical Society, by reason of its numbers and the importance of its field, shall occupy a most prominent position. Whether or not this vision is ever to come true, we should all work in harmony to the end of the formation of a grand organization of chemists that shall be a power for the greatest good to the profession and to mankind.

W. F. HILLEBRAND

U. S. GEOLOGICAL SURVEY

THE THIRTEENTH ANNUAL MEETING OF THE AMERICAN MATHEMATICAL SOCIETY

THE thirteenth annual meeting of the society was held in New York City on Friday and Saturday, December 28–29, 1906, forming a part of the general gathering of scientists in attendance at the meetings of the American Association for the Advancement of Science and the numerous affiliated societies. With this environment went a noticeable increase in attendance, over eighty members being present at the four sessions. Friday morning was devoted to a joint session with Section A of the association and the Astronomical and Astrophysical Society, Professor Simon Newcomb presiding. A large audience listened to seven papers from the fields represented. The remaining three sessions were somewhat crowded with the regular program, to which were added several papers from Section A. The great productive capacity of the society is rapidly making it a serious problem to provide adequate facilities for presentation and discussion of the output.

At the afternoon session on Friday Professors E. B. Van Vleck and Morley, and on Saturday President White, Vice-President P. F. Smith and Professor Morley occupied the chair. Owing to the recent illness of President Osgood, the presidential address was not delivered; it is hoped that it may be given at some future meeting.

The council announced the election of Mr. E. I. Shepard, of Harvard University, to membership in the society. Eight applications for membership were received. The organization was authorized of a new section of the society, to be known as the Southwestern Section. (A preliminary meeting of the proposed section was held at Columbia, Mo., on December 1.) It was decided to hold the next summer meeting at Cornell University. An amendment of the constitution was adopted by which the editorial committee of the *Transactions* is included in the council.

At the annual election the following officers and members of the council were chosen:

President—H. S. White.

Vice-presidents—Heinrich Maschke, P. F. Smith.

Secretary—F. N. Cole.

Treasurer—W. S. Dennett.

Committee on Publication—F. N. Cole, Alexander Ziwet, D. E. Smith.

Members of the Council to serve until December, 1909—G. A. Bliss, E. W. Brown, M. W. Haskell, A. G. Webster.

The treasurer's report shows a balance of $5,195.80 on hand December 14, 1906, being a gain of about $1,360 for the year, and including about $2,800 life-membership fund. The sales of the society's publications, exclusive of the *Bulletin* sent free to members, amounted during the year to over $1,500. The library has increased to over 2,500 volumes. The number of papers presented before the society in 1906 was

176. The total attendance of members at the ten meetings of the year was 350; 192 members attended at least one meeting. The total membership of the society is now 547, a gain of 43 during the year.

The publication of the New Haven colloquium lectures, delivered at the last summer meeting, has been generously undertaken by Yale University. The volume will probably appear next fall.

The following papers were read at the meeting:

S. E. Slocum: 'The rational basis of mathematical pedagogy.'

F. L. Griffin: 'On the law of gravitation in binary systems.'

James McMahon: 'A differential property of the lamellar vector field.'

J. I. Hutchinson: 'A method of constructing the fundamental region of a discontinuous group of linear transformations.'

James Pierpont: 'Multiple integrals.'

Oswald Veblen: 'Collineations in a finite projective geometry.'

W. R. Longley: 'Some particular solutions in the problem of n bodies.'

Max Mason: 'The expansion of an arbitrary function in terms of normal functions.'

R. D. Carmichael: 'On Euler's ϕ-function.'

Arthur Ranum: 'On the group of classes of congruent matrices.'

W. B. Carver: 'Sets of quadric spreads connected with the configuration $\Gamma_{n,r}$.

C. J. Keyser: 'Circle range transversals of circle ranges in a plane: a problem of construction.'

C. J. Keyser: 'Concerning the analytic treatment of geometric involution.'

A. B. Coble: 'A generalization of the plane Hesse configuration.'

A. B. Coble: 'Involutory Cremona transformations.'

W. E. Story: 'Denumerants of double differentiants.'

Virgil Snyder: 'Birational transformations of curves of high genus.'

T. E. McKinney: 'On the continued fractions representing properly and improperly equivalent real numbers in a system of continued fractions depending on a variable parameter.'

H. E. Hawkes: 'On elementary divisors.'

E. B. Wilson: 'Rotations in higher dimensions.'

Edward Kasner: 'Systems of extremals in the calculus of variations.'

Edward Kasner: 'The motion of a particle in a resisting medium.'

R. P. Stephans: 'Note on a system of curves of class n and order $2(n-1)$.'

D. E. Gillespie: 'On the construction of an integral of Lagrange's equation in the calculus of variations.'

F. R. Sharpe: 'The general circulation of the atmosphere.'

The following papers from the program of Section A of the American Association for the Advancement of Science were also laid before the society:

G. B. Halsted: 'The sect carrier and the set sect.'

Harris Hancock: 'On a fundamental theorem of Weierstrass by means of which the theory of elliptic functions may be established.'

G. A. Miller: 'On the minimum number of operations whose orders exceed two in any finite group.'

The informal dinner, always arranged in connection with each meeting of the society, adds much to the pleasure of these occasions. Despite the many distractions incident to the general gathering of scientists, over forty members attended the dinner on Friday evening and passed a few pleasant hours in social intercourse and renewal of old acquaintance.

The Chicago Section met on the same days as the society. The next meeting of the society, and also of the San Francisco Section, falls on Saturday, February 23.

F. N. Cole,
Secretary

THE AMERICAN PHYSIOLOGICAL SOCIETY

At the meeting of the society, held in New York City, December 27, 28 and 29, the following officers were elected:

President—William H. Howell, Johns Hopkins University.

Secretary—Lafayette B. Mendel, Sheffield Scientific School, Yale University.

Treasurer—Walter B. Cannon, Harvard Medical School.

Additional Members of the Council—Graham Lusk, University and Bellevue Hospital Medical College; John J. Abel, Johns Hopkins University.

The following were elected to membership:

Dr. Roswell Parker Angier, instructor in psychology, Yale University.

Dr. Philip A. Shaffer, instructor in pathological Chemistry, Cornell University Medical School.

Dr. Alexis Carrel, The Rockefeller Institute for Medical Research.

Dr. Eugene L. Opie, associate of the Rockefeller Institute for Medical Research.

Dr. Charles George Lewis Wolf, instructor in physiological chemistry, Cornell University Medical School.

Dr. Howard Davis Haskins, instructor in organic and physiological chemistry, Medical Department, Western Reserve University.

Dr. Donald R. Hooker, assistant in physiology, Johns Hopkins University.

Dr. John A. English Eyster, instructor in physiology, Johns Hopkins University.

Dr. John Raymond Murlin, assistant professor in physiology, University and Bellevue Hospital Medical College.

Dr. Ross Granville Harrison, associate professor of anatomy, Johns Hopkins University.

PROGRAM

Wednesday, December 26, 8:30 P.M.

Smoker, at the Murray Hill Hotel, Park Avenue and Forty-first Street.

Thursday, December 27, 10 A.M.

Physiological Laboratory, the University and Bellevue Hospital Medical College, Twenty-sixth Street and First Avenue.

General business. Reading of papers.

R. H. CHITTENDEN: 'The Minimal Proteid Requirement of Some High Proteid Animals.'

G. T. KEMP and L. D. HALL: 'The Formation of Fat in Animals Fattened for Slaughter.'

W. P. LOMBARD: 'The Rate of Loss of Weight of Normal Man.'

G. LUSK: 'The Influence of Mechanical Work in Phlorhizin Diabetes.'

J. R. MURLIN (by invitation): 'The Sparing Action of Gelatin.'

P. A. SHAFFER (by invitation): 'The Effect of Muscular Activity on Kreatinin Excretion; with Preliminary Observations on the Excretion of Kreatinin in Health and Disease.'

O. FOLIN: 'The Occurrence and Formation of Alkylamines and Alkylureas.'

W. SALANT: 'The Formation of Sugar from Amino-acids.'

G. B. WALLACE and J. S. DIAMOND: 'The Effects of Cocaine on the Liver.'

W. SALANT and G. M. MEYER: 'On the Elimination of Radium in Normal and Nephrectomized Animals.'

W. KOCH: 'The Relation of Inorganic Salts to Lecithin and Kephalin.'

C. L. ALSBERG, L. J. HENDERSON, H. B. WEBSTER and R. FITZ: 'Contributions to the Physiology of the Phosphates.'

C. L. ALSBERG and G. W. HALL: 'Concerning Glycolysis.'

A. S. LOEVENHART, G. PEIRCE and C. G. SOUDER: 'Some New Observations on the Action of Lipase.'

J. R. MANDEL and P. A. LEVENE: 'Nucleins of Codfish Roe.'

J. A. MANDEL and P. A. LEVENE: 'Glucothionic Acid in Pus.'

The secretary presented the question of proteid nomenclature for discussion.

The election of the council was held at 12 M.

Friday, December 28, 10 A.M.

Physiological laboratory, the College of Physicians and Surgeons, 437 West Fifty-ninth Street.

General business. Reading of papers.

C. F. HODGE and O. P. DELLINGER: 'Functions and Structures in *Amœba proteus*.'

J. DAWSON (by invitation): 'Physiological Reactions of *Physa*.'

R. S. LILLIE: 'Production of Artificial Parthenogenesis in *Asterias* Through Momentary Raising of Temperature.'

E. P. LYON and O. P. TERRY: 'Preliminary Report on the Enzymes of Unfertilized and Fertilized Eggs.'

G. N. STEWART: 'Experiments on Resuscitation.'

Y. HENDERSON: 'Artificial Regulation of the Heart-rate.'

J. ERLANGER and J. R. BLACKMAN: "On the So-called 'Ligature of Stannius in the Mammalian Heart.'"

A. J. CARLSON: 'On the Mechanism of the So-called Refractory Period of the Heart.'

A. J. CARLSON: 'On the Relation of the Normal Rhythm to the Sodium Chloride Rhythm of the Heart.'

W. T. PORTER: 'Vasomotor Reflexes.'

C. W. EDMUNDS: 'The Influence of the Digitalis Series up the Velocity of the Blood Stream.'

F. S. Lee: 'The Cause of the Treppe.'

F. S. Lee: 'Methods of Studying Fatigue.' (A demonstration.)

C. L. Alsberg: 'Demonstration of the Adiabatic Calorimeter of Richards, Henderson and Frevert.'

The election of new members was held between twelve and one o'clock.

The afternoon session was devoted to informal demonstrations.

Saturday, December 29, 10 A.M.

Rockefeller Institute for Medical Research, Sixty-sixth Street and Avenue A.

Joint session with the Section (K) of Physiology and Experimental Medicine of the American Association for the Advancement of Science.

R. M. Yerkes: 'The Functions of the Ear of the Dancing Mouse.'

S. J. Meltzer and J. Auer: 'The Effect of Section of one Vagus upon the Secondary Peristalsis of the Œsophagus.'

F. P. Underhill and L. B. Mendel: 'On the Alleged Adaptation of the Salivary Glands to Diet.'

C. H. Neilson (by invitation): 'Adaptation of Saliva to Diet.'

W. Koch and H. S. Reed: 'The Effect of Phosphorus Starvation on *Aspergillus niger*.'

W. J. Gies: 'New Chemical Facts about Tendon and Compound Proteins.'

W. J. Gies and W. N. Berg: 'A Further Study of Peptolysis.'

L. Loeb (by invitation): 'The Action of Blood Serum and Tissue Extracts on the Coagulation of the Blood.'

W. B. Cannon: 'Some Observations on the Œsophagus after Bilateral Vagotomy.'

L. B. Stookey and M. Morris: 'Concerning the Pharmacological Action of Salicylic Acid.'

P. A. Levene and J. E. Sweet: 'Nuclein Metabolism Experiment on a Dog with Eck's fistula.'

P. A. Levene, W. A. Beatty, D. R. MacLaurin and C. H. Ruiller: 'Protein Analysis.'

J. Auer: 'Demonstration of Normal Gastric Peristalsis in the Rabbit.'

J. Auer and S. J. Meltzer: 'Peristalsis of the Rabbit's Cæcum' (with demonstration).

A. Carrel (by invitation): 'Preservation of Blood Vessels in Cold Storage.'

S. J. Meltzer: 'Demonstration of the Failure of Regeneration of the Cervical Ganglion Twenty-six Months after its Removal.'

The members of the society were invited to luncheon on Thursday, Friday and Saturday, at one o'clock.

A scientific exhibition was held at the American Museum of Natural History during convocation week, under the auspices of the New York Academy of Sciences. The purpose of the exhibition is to collect and demonstrate objects showing the most recent advances in the different departments of science. The Committee on Physiological Exhibit consisted of William J. Gies, chairman; Joseph Erlanger, William H. Howell, Frederic S. Lee, Jacques Loeb, Warren P. Lombard, Graham Lusk, S. J. Meltzer, Lafayette B. Mendel, William T. Porter, Edward T. Reichert, G. N. Stewart.

The next meeting of the society will be held in conjunction with the Congress of American Physicians and Surgeons in Washington, May, 1907.

Lafayette B. Mendel,
Secretary

THE AMERICAN SOCIETY OF VERTEBRATE PALEONTOLOGISTS

The fifth annual meeting of the American Society of Vertebrate Paleontologists was held in the American Museum of Natural History, New York City, on Wednesday and Thursday, December 26 and 27, 1906. The following members were present:

Mr. Barnum Brown, American Museum of Natural History, New York City, N. Y.

Dr. Bashford Dean, Columbia University, New York City, N. Y.

Dr. Marcus S. Farr, Princeton University, Princeton, N. J.

Mr. J. W. Gidley, U. S. National Museum, Washington, D. C.

Mr. Walter Granger, American Museum of Natural History, New York City, N. Y.

Mr. W. K. Gregory, American Museum of Natural History, New York City, N. Y.

Dr. O. P. Hay, American Museum of Natural History, New York City, N. Y.

Dr. L. Hussakof, American Museum of Natural History, New York City, N. Y.

Professor F. B. Loomis, Amherst College, Amherst, Mass.

Dr. R. S. Lull, Yale University, New Haven, Conn.

Dr. J. H. McGregor, Columbia University, New York City, N. Y.

Dr. W. D. Matthew, American Museum of Natural History, New York City, N. Y.

Professor H. F. Osborn, American Museum of Natural History, New York City, N. Y.

Professor W. B. Scott, Princeton University, Princeton, N. J.

Dr. W. J. Sinclair, Princeton University, Princeton, N. J.

The following papers were presented at the meeting:

TITLES OF PAPERS READ AT THE MEETING

Dr. J. H. McGregor: 'Mesosaurus and Stereosternum from the Permian of Brazil.' Illustrated by drawings, photographs and specimens.

Professor H. F. Osborn: 'Faunal Succession in the American Tertiaries.' Charts and lantern.

Professor H. F. Osborn: 'Brief Notices of Tyrannosaurus, Naosaurus, Elephas columbi, Allosaurus, and other Vertebrates Recently Mounted at the American Museum of Natural History.'

Dr. W. D. Matthew: 'A Lower Miocene Fauna from South Dakota.'

Professor F. B. Loomis: 'Conditions under which the Wasatch and Wind River Beds were Deposited.'

Dr. L. Hussakof: 'The Devonic Fish-bearing Formations of Ohio and Eastern Canada.' Lantern illustrations.

Dr. Bashford Dean: 'Notes on Fossil Sharks.' Illustrated by models.

Dr. W. K. Gregory: 'Brief Notes and Observations on the Theory of Trituberculy.'

Dr. W. K. Gregory: 'Exhibition of a Specimen of Prorosmarus alleni, a Primitive Walrus from the Miocene.'

Professor W. B. Scott: 'The Litopterna.' (The President's address).

Dr. Chas. S. Mead: 'The Gait and Correct Pose of Brontosaurus.' Illustrated by model of skeleton.

Mr. A. O. Peterson: 'A Section of the Lower Miocene of Nebraska, and a List of its Vertebrate Fauna.'

Mr. Earl Douglas: 'A Restoration of Palæomeryx.'

The following officers were elected for the ensuing year:

President—Professor Bashford Dean, Columbia University, New York City, N. Y.

Secretary-Treasurer—Professor Frederick B. Loomis, Amherst College, Amherst, Mass.

Executive Committee—Professor E. H. Barbour, University of Nebraska, Lincoln, Nebraska; Mr. Lawrence M. Lambe, Geological Survey of Canada, Ottawa, Canada; Professor J. C. Merriam, University of California, Berkeley, California; Dr. W. J. Sinclair, Princeton University, Princeton, N. J.

Marcus S. Farr,
Secretary-Treasurer

Princeton, N. J.,
December 31, 1906

SCIENTIFIC BOOKS

THE BULLETIN OF THE AMERICAN MUSEUM OF NATURAL HISTORY.

In the issue of May 25, 1900, of this journal a review was given of the *Bulletin of the American Museum of Natural History.* It was intended to be of some assistance to those unacquainted with the extent of these scientific contributions. It was suggestive simply, and embraced no intention of giving details or exhaustive titles. It concluded with volume XII. Since then nine volumes have been published, and a new series of subjects

BULLETIN SERIES: ANALYSIS OF CONTENTS.
(PAPERS PUBLISHED.)

	13.	14.	15. Part 1.	16.	17. Parts 1, 2, 3, 4.	18. Parts 1, 2, 3.	19.	20.	21.
Ethnology	1	1	1	1	3	3			
Archæology	3	1						2	2
Mammalogy	9	9		14			8	11	
Ornithology	2	2		1				2	4
Ichthyology									
Herpetology									
Entomology		2		3			6	9	9
Invert. Zoology		3		1			1		
Conchology		1							
Paleontology (Vert.)	4	3		8			12	11	7
Paleontology (Invert.)	2	1		4			3	3	3
Geology		1		2			1		
Mineralogy	1								
Catalogue								1	
Pages	330	422	370	514	380	278	713	539	426
Plates	19	46	4	59	58	56	59	14	17
Figures and Cuts	74	63	172	126	67	102	137	130	82

have appeared on its pages. A very eclectic notice is again offered, in which particularization will be even more limited than was the case in 1900. A tabulation of general classes of subjects, number of papers published under each, and the number of pages, figures and cuts, in each volume, is here subjoined. It can be expected that, in the wide circulation of SCIENCE, many readers will welcome this generalized description of the *Bulletins of the American Museum.*

A comparison between this table and the table of analysis of contents of this publication, for the first twelve volumes, shows an increase in the papers on entomology and vertebrate paleontology, continued activity in mammalogy, a decrease in ornithology, and generally elsewhere, conditions similar to those preexisting.

The bulletins show a tendency to increase in size, and the appearance of new workers as Drs. Matthew, Gidley and Hay and Professor Wheeler, with others (Nelson, Duerden, Loomis, Hussakof, Brown Van Duzee, Banks, Miller, Brues, Bandelier) diversify the pages with new authors, and animate them with new treatments. Papers of very considerable length are noticeable, as Part I. of Vol. XV., in Professor Boas's 'The Eskimo of Baffin Land and Hudson Bay,' the extended essays by Kroeber on topics furnished by the Arapaho Indians, studies supported by Mrs. Morris K. Jesup; and Dixon's contributions to the ethnology of California in the Huntington Expedition.

The first article in Vol. XIII., by Dr. Allen, discussed the mountain caribou of British Columbia with especial reference to Mr. Ernest Seton-Thompson's new species (*R. montanus*) and forms another contribution to the often noted variability of the genus, and contained the statement 'doubtless when series of specimens of caribou from different parts of Alaska, including the tundra district west of the Mackenzie Delta, and from different parts of the Northwest Territory, are brought together, it will be found that the caribous of the region north of the United States are differentiated into quite a number of well-marked local forms, as yet undescribed.' Professor Whitfield describes some interesting fossils (*Receptaculites, Halysites, Heliolites*) from the arctic, brought back by Lieut. Peary. Mr. Stone contributes a narration, in part, of his adventuresome journey along the coast of northern Alaska. A shell gorget of Tarascan origin and certainly important, and an onyx jar from Mexico are described by M. H. Saville, and the same investigator adds an important and readable article on 'Cruciform Structures near Mitla.' Professor Osborn furnishes a discussion of the Phylogeny of the Rhinoceroses of Europe which illustrates "*the early separation, absolute distinctness, and great age of numerous phyla leading up to modern types.*" In importance this article easily exceeds the associated papers of this volume. The volume contains also a very illuminative analysis of variation in the meadow-lark by F. M. Chapman.

In volume XIV. two new contributors appear, W. D. Matthew and J. W. Gidley, both of whose names have since become very strongly impressed upon American vertebrate paleontology. Matthew's 'Additional Observations on the Creodonta,' and J. W. Gidley's 'Tooth Characters and Revision of the Genus *Equus*,' in point of originality and permanent results are the most valuable papers in the volume. Dr. Allen prepared a review of the question of the relationship of the musk-oxen of Arctic America and Greenland which also contained an extended historical reference. The same distinguished systematist included in this volume a study on the North American opossums. Dinosaur Contribution No. 6, by Osborn and Granger, appears in this volume, and Beutenmüller continued his painstaking papers on the lepidoptera. A useful descriptive catalogue of the Binney and Bland collection of mollusks closed the volume with six maps of *distributional intensity*, which were something of a novelty.

About this time an attempt was made to segregate articles on one class of subjects in single volumes, and volumes XV., XVII., XVIII. were in this way devoted to ethnology. These volumes are not yet completed and contain laborious papers by Boas, Dixon, Kroeber and Wissler, papers for the most part concerning the aspects, features and results of the Mrs. Morris K. Jesup and Huntington Expeditions.

In volume XVI. perhaps preeminence of interest attaches to Aleš Hrdlička's paper on 'The Crania of Trenton, New Jersey, and their Bearing upon the Antiquity of Man in that Region,' which concluded with these pregnant words: "It may be added that all the crania described in this paper differ widely from those of the Eskimo (nor can I recollect a single important somatological fact, from my investigations or those of others, which would support the theory of a prehistoric occupation of *any* of the eastern states below the St. Lawrence river by the Eskimo)." New species, as usual, are described in this volume, both of fossil and living animals. An almost entertaining article by Professor Whitfield accomplishes the desirable result of proving that three fossil genera of cephalopoda are different stages of one, a fact distinguishable in the beautiful examples of *Heteroceras* in his cabinet of Cretaceous fossils. The important papers on phylogeny, by Professor Osborn, were continued, and an admirably illustrated paper by Dr. E. O. Hovey on the eruptions in Martinique and St. Vincent seems a welcome variation from the endless process of creating and destroying species. Professor Whitfield's description of a genus of fossil alga in the Niagara shale has interest, as well as that of the new teredo-like shell from the Laramie. Mr. Beutenmüller added one of his instructive studies on larvæ of *Catocala,* and his minute discriminations in the 'Earlier Stages of some Moths.' Dr. Duerden's paper on 'Algæ as agents in the Disintegration of Corals' is in this volume.

Volume XIX. is the thickest, the most voluminous of all the volumes, though it does not contain more articles nor does it exceed in interest its precursors. The new contributors were making themselves felt and the topics were, in some instances, to a degree, synoptical and comprehensive. Dr. Hay opens the nineteenth volume with a technical and strong paper on 'North American Cretaceous Fishes,' in which the author displays his surprising anatomical skill. It involved very large corrections of previous observers. 'The Mammals of Northeast Siberia,' by Dr. Allen, was important. It emphasized the fact of the intimate relationship of the mammalian fauna of Siberia with that of Alaska. The itinerary of Mr. U. G. Buxton accompanying this paper is extremely interesting, and his notes appended to Dr. Allen's descriptions make good reading, and are most instructive. Dr. Allen concludes:

> There is thus evidence that eastern Siberia has derived some of its present mammalian life from boreal America, and doubtless within a comparatively recent period. The American origin of various early types that eventually attained circumpolar distribution, as the horse, camel, rhinoceros, phyla, etc., is now well established by paleontological evidence, but that the same is true of some forms of the existing mammalian fauna does not appear to have been heretofore recognized.

Dr. Matthew discusses the minute fauna of the Titanotherium beds of Montana. A paper by Dr. Hrdlička on the parietal bone in Men and Mammals was somewhat responsible for the bulkiness of volume XIX. It was a rather over-extended discussion, but very learned, of an osteological feature, which apparently refuses to yield to this persistent study very definite conclusions.

Professor W. K. Gregory contributed a suggestive paper on the 'Shortening of the Elephant's Skull,' concluding:

> The skull as a whole is thus highly adapted to resist the severe strains put upon it. The occiput, both in ontogeny and phylogeny, flattens out and rotates backward, spreading both vertically and laterally, until at last it forms, as it were, a great functionally solid bed-plate, receiving the thrusts of the opposite inverted arches into which the skull has been resolved.

Dr. Hay added another extended paper, on 'Cretaceous Fishes from Mt. Lebanon,' in which there were new species and new facts. Professor Osborn describes a new dinosaur, Mr. Gidley a new three-toed horse, and remarks that 'it seems probable that the genus *Hipparion* is limited in distribution entirely to the old world, and that the American species formerly referred to this genus should be placed in a group distinct from *Hipparion.*'

A valuable paper of Professor Whitfield's on 'Six New Species of Unios from the Laramie Group,' attracts attention in this volume, pointing as it does to a possible western metropolis and origination for fresh-water shells of this family in the central and Mississippi basins.

A new glyptodon from the lower Pleistocene of Texas was described by Professor Osborn, and possesses extreme interest. "It proves to represent a new genus and species, combining characters of several of the South American forms of the Pleistocene and Miocene periods."

The Stone Expedition to Alaska in 1902 collected 1,100 specimens of mammals, representing 43 species and subspecies, in which are 50 head of large game, and a series of 31 skulls of the Kadiak bear. It was a remarkable collection. Dr. Allen describes it, and among its additions to existing species is to be noted the Osborn caribou (*R. osborni* Allen). Mr. Barnum Brown describes a new genus of ground sloth from the Pleistocene of Nebraska, Mr. Beutenmüller new insects, and Professor Wheeler distinctly furnishes a new literary and scientific interest in his careful studies and speculations upon ants, amongst which prominence should be given to his views upon *gynandromorphism* in these insects.

Vol. XX. of the *Bulletin* was characterized by the signal predominance of the papers in 'Mammalogy, Vertebrate Paleontology and Entomology.' It is impossible or unnecessary to particularize. The articles were systematic and descriptive; all possess the distinctive authority of their writers in their several fields of research, but two, of especial interest, claim individual notice. The first is by Dr. Matthew, on 'Two New Oligocene Camels,' from which this conclusion may be quoted:

> In the Miocene the camels show increasing divergence and variety of type, and their relationship to the preceding and succeeding stages is far from clear. While we have reason to believe that the center of dispersion of the Camelidæ was somewhere on the North American continent, we have no reason to believe that it was in the particular regions from which our fossil species have been obtained.

The second is by Professor Osborn on the great Cretaceous fish, *Portheus molassus* Cape. It is preliminary and brief, but it announced the possession by the museum of a most remarkable and monstrous fossil fish procured by Mr. Charles H. Sternberg in 1900 from near Elkada, Logan Co., Kansas. Professor Wheeler included in this volume a very readable and attractive paper on 'Social Parasitism among Ants.'

In volume XXI. two very important papers from Dr. Hay are noticed in which that well-known osteologist locks horns with European authors, and discusses the origin and relationships of the testudines. A paper admirable in diction and beautifully illustrated is Chapman's 'Life History of the American Flamingo.' Professor Wheeler contributes a discussional paper on 'An Interpretation of the Slave-making Instincts in Ants,' which is somewhat varied in type from the ordinal group of papers, and is very suggestive. The remainder of the volume contains the valuable and learned studies of the naturalists, whose papers have now for over twenty-six years maintained the high standard of this publication.

L. P. Gratacap.

American Museum of Natural History.

The Relation of Leaf Structure to Physical Factors. By Edith S. Clements. *Transactions of the American Microscopical Society,* 1905, pp. 19 to 102. Published under a grant from the Spencer-Tolles fund.

In studying the reaction of the plant to its physical environment the leaf is a peculiarly favorable subject, because of its ready response to alterations in the environmental factors and because of the clearness with which such response is manifested in changes of structure. A considerable literature on the relations of leaf structure to environment has grown up in Europe, based upon the vegetation of northern Africa, tropical Asia, etc., as well as of Europe. But the subject has received comparatively little attention from North American botanists, notwithstanding the fact that our wide range of climate and consequent diversity of vegetation-types offer exceptional opportunities for this line of research.

Mrs. Clements's paper, which constitutes another of the valuable contributions to ecological botany that have issued from the School of Botany of the University of Nebraska, should, therefore, receive a cordial welcome, the more so as it embodies the results of a well-considered and carefully executed plan of investigation. One is impressed at first glance with the great amount of painstaking work that has been necessary to carry out this plan. Not only have the details of leaf anatomy been studied in about 300 species of plants, most of which were represented by at least two different habitat-forms, but a large number of measurements were necessary to obtain the normals of the more important physical factors for each of a score of habitats. The methods followed are those outlined in Dr. F. E. Clements's 'Research Methods in Ecology.' The investigations were carried on during the summers of 1903 and 1904 in the mountains and foothills around Pikes Peak, Colo., with headquarters at the alpine laboratory of the University of Nebraska at Minnehaha. The paper begins with a survey of the most important literature. In a table on page 29 are presented the normals of light, atmospheric humidity, temperature (of the air, the surface of the ground and the soil) and water content of the soil for each of the habitats studied. The greater part of the paper is devoted to brief descriptions of the leaf anatomy of the species examined, both as to the normal structure and as to the variations caused by change of habitat. Quantitative variations were given much attention and were carefully measured. 'Endemic' species (those occupying only one habitat) are grouped according to habitat under the three types of hydrophytic, mesophytic and xerophytic plants. 'Polydemic' species (those occurring in more than one habitat) are also grouped as hydrophytes, mesophytes and xerophytes, according to the normal habitat of the species, but their classification according to habitat is not carried further. Instead, under each species name is given a synopsis of the chief physical factors of the normal habitat, with brief descriptions of the corresponding leaf structure, followed by indications of the points of difference in the physical factors and leaf structure in the other habitats of the species.

A series of tables of species follow in which are graphically expressed the variations from the normal leaf structure that accompany departures from the normal environment, the varying factors of the latter being stated at the head of the table thus: 'light unchanged, water and humidity decreased.'

In the summary are stated the most important of the author's conclusions as to the effect upon the anatomical structure of leaves of each of the physical factors studied, attention being called to the points of agreement or of disaccord with the results of Heinricher, Dufour, Stahl and other well-known investigators. In future researches the writer believes that the phases of the subject which should receive especial attention are: "(1) the hereditary structure, which should include considerations of size, shape and position of leaf, as well as histology and modifications, such as hairs, stomata, mucilage cells and the like; (2) exact records of the physical factors of the habitat of the species for the day and for the growing season; (3) the physiological processes of the leaf; (4) the interrelation and correlation of the preceding data."

The paper is illustrated by nine excellent plates, showing the habitat variations of many of the species described.

To say that Mrs. Clements has published one of the most important papers dealing with this phase of American ecological botany is to render no more than justice to her achievement. An especially commendable feature of her work is the great volume of careful observations that serve as a basis for what generalizations are made. So high a ratio of fact to theory does not always characterize this branch of botanical literature. A good service has been rendered to American students of plant ecology by pointing out a field for future research which can not fail to be fruitful of results.

Thomas H. Kearney

Beet-Sugar Manufacture. By H. Claassen, Ph.D. Authorized translation from the second German edition by William T. Hall, S.B., and George William Rolfe, A.M. New York, John Wiley and Sons. 1906. Pp. xiv + 280.

Claassen's 'Die Zuckerfabrikation' was first published in 1901. Its sterling merits soon won for it such general recognition that the second edition in German followed within a few years, and now we have an authorized English translation of the work.

The scope and plan of the book embraces the entire process of beet-sugar manufacture from the time of the receiving of the beets to the finished product.

Individual chapters are devoted to the delivery of beets, their transportation and washing, weighing and slicing, utilization and disposal of exhausted chips, the process of defecation and of carbonatation, evaporation, the boiling of sugar, the preparation of raw sugar and the preparation of sugar crystals, the treatment of after-products and the utilization of molasses.

In addition to these themes the book discusses the boiler-house, the questions of economy of fuel, high-pressure and low-pressure boilers, heat losses, the construction and operation of lime-kilns, the factory control and determination of sugar losses, the setting up and running of a beet-sugar factory and the utilization of waste products incidentally produced in the process.

This mere enumeration of the contents of the work well indicates that the author has intended to prepare a monograph of beet-sugar manufacture which should not lack a single essential detail—and Claassen has succeeded in doing all which he has set out to do.

His thorough practical knowledge of beet-sugar manufacture—for many years he has been the director of one of the leading beet-sugar houses of Germany—joined to an exceptional ability to express his thoughts in a clear and concise manner, has resulted in the production of a book which ranks with the very best in the sugar literature of the day.

Turning from the work of the author to that of the translators, it is a pleasure to state that their work, too, is everything that could be desired.

In their preface they state that they have introduced into the English text data of factory practise in units which are employed in American houses. In many beet-sugar houses in this country the metric system is well understood and the centigrade thermometer is used; it is to be hoped that the introduction by the translators of the American equivalent weights and measures will tend to a greater familiarity with, and ultimately to the sole use of, the metric system in this important and growing industry.

Several of the tables appearing in the German edition have been omitted in the English text; also Appendix II., which treats of the construction of an evaporating-plant and the steam consumption for working 100 kg. beets per minute, and Appendix III., which deals with sugar statistics. These omissions seem well warranted, as the matter thus left out is presumably of very little importance to the general reader.

A few typographical errors and slips have crept in, but these will unquestionably be noted and corrected in a future edition, which, no doubt, will soon be warranted.

The American sugar industry is certainly to be congratulated on having so valuable and practical a book placed at its disposal.

F. G. Wiechmann

SCIENTIFIC JOURNALS AND ARTICLES

The *American Naturalist* for December contains the second, and concluding instalment of 'The Causes of Extinction of Mammalia,' by Henry F. Osborn. This discusses such matters as infectious diseases and insects, competing and hostile mammalia, internal causes of extinction, and the inadaptation of extreme size or specialization, with many references to literature on the subject and citation of examples. And yet, in summing up, Professor Osborn says: "The chief induction which can be made from this extensive survey of the causes of extinction seems to be this: following the diminution in number which may arise from a chief or original cause, various other causes conspire or are cumulative in effect." This conservatism is particularly refreshing in view of the many positive utterances as to the natural extinction of animals, the truth being that we actually *know* very little about it. T. D. A. Cockerell discusses at length 'The Alpine Flora of Colorado,' giving many tables showing the northerly range and vertical distribution of various species: The third long paper, by Thomas J. Headlee deals with the 'Blood Gills of *Simulium Pictipes*.' The number contains the title page and index for the volume.

The Museum News of the Brooklyn institute for January notes 'An Interesting Case of Retardation of Pupæ of a Texas Moth,' *Agapema galbina*, a number of cocoons obtained in 1903 having yielded perfect insects for three consecutive years with the probability that one or two more may appear in 1907. It is stated that the museum has obtained by the bequest of Mr. Henry Mumford the fine series of shells secured by the late Isaiah Greegor comprising 2,400 species and 15,000 specimens. This collection by the terms of the will is henceforth to be known as the Phebe L. Mumford Collection. A brief description is given of the exhibit of the museum, under the auspices of the New York Academy of Sciences, to illustrate progress in zoology. The leading article in the section devoted to the Children's Museum is on the skunk. It is stated that while the general attendance at the Children's Museum is less than during 1905 the attendance of teachers is much greater.

SOCIETIES AND ACADEMIES

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 420th meeting was held on November 17, 1906, with President Knowlton in the chair and an audience of forty persons.

Professor A. S. Hitchcock remarked on the code of nomenclature recently adopted by the International Congress of Zoologists, comparing its provisions with similar codes adopted in this country. Mr. A. A. Doolittle exhibited an abnormal rose, lacking a pistil and with the stem continued into the flower.

Dr. E. L. Greene spoke 'On So-called Rhus Toxicodendron.' The purpose of the paper was twofold. First, that of demonstrating fundamental distinctions between *Rhus* and *Toxicodendron* as perfectly distinct genera, according to which view no such name as *Rhus Toxicodendron* should be used. Proper *Rhus* has always a many-pinnated foliage, and its inflorescence is always one only to each branch and that strictly terminal. *Toxicodendron* as universally exhibits but three leaflets to each leaf, and as many inflorescences, almost, as there are leaves on the branch, namely, one in each axil, none ever terminal. The individual fruits are again as widely different in the two genera. Moreover, *Rhus* in all its species is innocuous. *Toxicodendron* is acridly poisonous in all its forms. A historic sketch of *Toxicodendron* was given, beginning with its first publication as a three-leaved ivy, by Cornutus, at Paris in 1635; after that, separated from the Ivy, and proposed as a genus *Toxicodendron* by Tournefort in 1694; augmented by Dillenius in 1732; suppressed by Linnæus, who made the name *Rhus Toxicodendron* in 1753; restored to generic rank as *Toxicodendron vulgare* by Philip Miller in 1768. Secondly, a long series of *Toxicodendron* specimens was exhibited, from almost all parts of North America from the Atlantic to the Pacific, and from Maine to central Mexico; these portraying as much diversity of foliage, fruit and modes of growth as, were they oaks or maples, would be accepted for two dozen species. *Rhus Toxicodendron*, so-called, is really a genus *Toxicodendron* made up of probably twenty or more valid species. Some remarks followed, chiefly on that part of the paper in which the action of *Toxicodendron* poison and its reputed remedies were touched upon.

The second paper was by Dr. Barton W. Evermann on 'Fish Culture and Fish and Game Protection in the Cornell and Yale Forest Schools.' He explained the relation of fish culture and the protection of fish and game to forestry and to the practical work of the forester, and the consequent incorporation of instruction on these subjects as a regular part of technical forestry courses. His remarks were illustrated by a number of lantern slides taken during the field courses given by him at Axton, in the Adirondacks, for the Cornell School, and at Milford, Pa., for Yale University.

The third paper, 'A Record of the Black Rat in Virginia,' was read by Mr. William Palmer. He noted the occurrence of an isolated colony of the black rat (*Mus rattus*) on the top of a Virginia mountain, Peaks of Otter, in Bedford County, at an elevation of 3,875 feet. The specimens collected are not quite typical. Probably but few individuals now exist in and about an old store at the summit.

THE 421st meeting was held on December 1, 1906, President Knowlton in the chair and about fifty persons present.

General T. E. Wilcox remarked on the unusual abundance of quail and the cottontail rabbit in New York a few miles south of Utica.

Dr. Evermann informed the society of the recent death of two naval officers to whom biological science is much indebted, Lieutenant Franklin Swift, retired, of the steamer *Fish Hawk,* and Lieutenant-Commander Leroy M. Garrett, of the *Albatross*. Lieutenant Swift died on November 10, at Charleston, S. C., of typhoid fever, and Lieutenant-Commander Garrett was washed overboard 500 miles northwest of Honolulu on November 21, while the *Albatross* was returning with the great collections of the trip to Japan. These officers have commanded these research vessels during some of their most important work and are in large part responsible for the excellent results obtained.

Dr. L. O. Howard presented the first paper, on the subject 'Polyembryony and Fixation of Sex.' This paper was published at length in SCIENCE, December 21, 1906.

The second paper consisted of an illustrated lecture by Mr. John W. Titcomb, on 'Principles and Methods in Fish Culture.' He explained the underlying principles of artificial propagation as applied chiefly to salmonoid fishes, described in detail the methods and manipulations concerned and illustrated every point by lantern-slide pictures, showing apparatus, operations and the fishes themselves in all stages from the egg upward. He commented on the relation of fish culture to various natural sciences. The long and interesting series of illustrations included pictures showing the inauguration of fish culture by the speaker in Argentina, South America.

M. C. MARSH,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

POLYEMBRYONY AND SEX-DETERMINATION

IN an extended review in the last number of SCIENCE (December 21, 1906), Dr. Howard has emphasized the astonishing and valuable results of the recent work by Marchal ('98, '04) and Silvestri ('05, '06) on the spontaneous polyembryony of certain parasitic Hymenoptera. He has quoted Bugnion's discussion of the bearing of this work on sex-determination but has not called attention to the fact that in the light of Silvestri's work this view may need revision.

As stated, Bugnion, '91, in the course of his work upon Encyrtus had noted that as a rule all of the individuals emerging from one host belong to a single sex. At the time, Bugnion thought that this "should be attributed to an occasional parthenogenesis, the caterpillars giving birth exclusively to males having been those which had been pierced by a non-fertilized *Encyrtus*."

This conclusion, which was a logical one in view of the data then at hand, Bugnion discards completely since the appearance of Marchal's work. He believes that the phenomenon must be "a natural consequence of polyembryony, and that one would expect the sexes to be separated in this way wherever the embryos come from the division of a single egg."

While the latter clause is undoubtedly true, the possibility of the facts being explained on the basis of parthenogenesis is by no means excluded. Bugnion, in his work, did not observe the oviposition. Marchal presents no evidence that parthenogenetic development does not take place. In fact, he purposely leaves the question open, as '04, p. 298, "Le cause de cette détermination reside-t-elle dans la fécondation pour le sexe femelle, et dans l'absence de fécondation pour le sexe mâle, il se peut qu'il en soit ainsi, mais le fait n'est pas démontré."

On the other hand, Silvestri very definitely determined that in the case of *Litomastix* the parthenogenetic development does take place and that, as in the bees, the fertilized eggs always give rise to females, the unfertilized to males. Until further observations have been made it would seem unsafe to discard Bugnion's earlier hypothesis that the observed facts regarding the preponderance of one sex or the other in *Encyrtus* are to be likewise explained. WM. A. RILEY

VARIATION OR MUTATION?

SYSTEMATIC zoologists are not likely to be hasty in endorsing the dogma of de Vries in respect of individual variations, or 'fluctuations' in his terminology: "* * * they may be proved to be inadequate even to make a single step along the great lines of evolution, in regard to progressive as well as retrogressive development."[1]

There are two methods of approach to the part played by mutations and individual variations in the development of specific characters: the comparative, in use by taxonomists, and the experimental, at the hands chiefly of embryologists.

The argument for individual variation from the comparative side was well presented by Dr. C. Hart Merriam, in his vice-presidential address before the American Association, and that for mutations from the experimental side, with equal clearness, by Professor Davenport, in SCIENCE of November 2, although he does not take the extreme view of de Vries.

Now, both systematist and experimenter will admit the absence of any exact means of determining what may or may not have been originally a mutation in such cases, for instance, as slight discontinuity observed under nature where there is no knowledge of the race history—for when Davenport asks: "But will it not be often impossible to say whether a new-appearing quality is truly new or old?"[2] no one can deny him. The statistical method, though it be fondly looked on as a universal solvent, can give no help here, for it points out only the end facts, not their causes, and there seems to be no resource but in the balanced judgment of competent observers. Therefore, when one so qualified as Dr. Merriam states his opinion that in more than a thousand species and subspecies of North American mammals and birds, he does not find one which appears to have arisen by mutation, he records a conclusion of great weight. Essential agreement with Merriam results from a similar examination of North American scaled reptiles.

The measure established is that a species or subspecies to be rated as a possible mutant must be separated from its nearest known congener by at least one indivisible character. This, I believe, accords with the standard set by de Vries, as well as with that of Professor Davenport. It might be claimed by extreme mutationists that monotypic genera, appearing to be related to a species of another genus occupying the same range, have arisen by mutation, but in these cases there is rarely valid evidence on either side, and as either view must be an assumption, they are not considered in this examination. If we are to reach a general rule of probability it must be through cases determined upon reasonable grounds.

I have followed Professor Cope's last descriptive list of Nearctic reptiles, not by any means from complete agreement with it, but for the reason that the analytic method favored by him left few variants unnamed.

Among lizards, Cope says of the genus *Sceloporus:* "I recommend it as an excellent *pièce de résistance* for those persons who do not believe in the doctrine of the derivation of species." This thought may be borrowed and extended to include the whole list of Nearctic lizards, and addressed to all who require evidence of the derivation of species by minute gradations, for nowhere else, perhaps, are they more general. There is no room here for mutations.

[1] 'Species and Varieties,' p. 18, 1905.

[2] SCIENCE, September 22, 1905, p. 370.

In serpents, such variations as the presence or absence of certain head plates, or of a pair of dorsal rows of scales, are fairly common in many genera, but as a rule they do not transgress the obvious limits of specific variation, and unless combined with other differences they are not regarded as deserving of a name. Nevertheless, when they do transgress they fall within our definition of a mutation, for these characters are the indivisible units of repetitive series, and between their presence or absence there can be no intergradation. Among the species and subspecies enumerated by Cope, there are thirteen such cases which might possibly be allowed as mutations. But even granting them to be such, they seem to have failed signally in giving rise to new species, for nine of them are known only from the one type specimen each, and of the tenth, two examples only were collected more than twenty years ago, at the same time, in a well-settled part of Texas. The remaining three cases, of more or less established forms, have some claim to consideration. They are these:

The genus *Storeria* consists of three species, two of which, *S. dekayi* with seventeen rows of dorsal scales, and *S. occipito-maculata* with fifteen, occupy practically the same range from Vera Cruz north over most of the Austroriparian and eastern regions. There are slight color differences, fairly constant, but the difference in scale rows seems to be entirely so, and all herpetologists admit their specific distinctness. As there can be no gradation between fifteen and seventeen scale rows, which vary always in pairs, one or the other of these species, probably *S. occipito-maculata,* seems to have arisen from the other by a process which might be called mutation. It may be allowed that the differential characters are not adaptive.

In exactly the same way *Virginia elegans,* occupying a limited western portion of the range of *V. valeriæ,* differs from it in having two more scale rows.

Finally, *Eutænia elegans atrata* (=*E. infernalis vidua* Cope) appears to be an offshoot of *E. elegans,* presenting a quite distinct color pattern and a tendency to a reduction of scale rows. According to Van Denburgh they are found only on the coast slope of the peninsula of San Francisco, and the examples I have seen were collected promiscuously with typical *E. elegans.* The mutation in this case would lie in the distribution of color, for the reduction in scale rows is not fully constant. Whether they breed true is not known, but their scarcity renders it doubtful.

But we are now close to a mere matter of names, for in two, at least, of these cases variation and mutation approach each other so nearly that they come under the same definition, for the addition or subtraction of a pair of scale rows represents the lowest possible term in a variation series, and the name given to it is largely a matter of choice; yet beyond these cases no other evidence for the origin of specific characters by mutation is yielded by the examination. The conclusion is near to that of Dr. Merriam.

The value of the experimental method is not questioned by the doubt whether theoretical interpretation of the behavior of 'unit characters' in the germ plasm has yet reached a stage of certainty sufficient to stand over against the body of evidence contributed by the comparative method, as to the minor rôle of mutations in specific development in vertebrates.

That mutants occur in feral animals is doubtless true, even much more widely than the cases of melanism and albinism cited by Professor Davenport, but it does not yet seem necessary to modify the opinion not long since expressed by me elsewhere[3]—"In so far as its occurrence under nature is concerned, every zoologist who has worked over many genera for purposes of taxonomy will probably admit that many of his most perplexing anomalies, which occur now and then as one or a few individuals which can not be exactly placed, are in the nature of mutations, but few, I imagine, will be disposed to allow that they find evidence that these are inherited. . . . there is little evidence that they have been starting points of new species."

Arthur Erwin Brown
The Zoological Gardens,
Philadelphia

[3] 'Theories of Evolution since Darwin,' 1906.

NEW MEXICO GEOLOGY.

IN SCIENCE for June 15, Dr. C. R. Keyes, formerly president of the New Mexico School of Mines at Socorro, gives a general section of the formations of New Mexico. This is a sequel to a series of papers in other scientific journals, particularly the *Journal of Geology,* the *American Geologist* and the *American Journal of Science,* in which he has discussed various aspects of the geology of the territory. These articles treat of phases of the subject of great interest to geologists as bearing on the geology of a field as yet little known, but the author can not well be congratulated on the extent of the contribution he has made to our knowledge of the geology of this region. There are many inaccuracies and the papers are manifestly designed to anticipate the results of investigations rather than as a record of actual observations. Heretofore, Dr. Keyes has maintained there was no evidence that Lower Paleozoic formations were present in New Mexico. He places them in the column now published, however, with thicknesses and lithological characteristics but fails to advise us as to any circumstances concerning their discovery.[1] He gives the Devonian as made up of limestones, whereas, so far as known, they consist entirely of shales.[2] Limestones and shales are said to constitute the Carboniferous thus neglecting entirely the great body of sandstones contained in the upper division. The distribution of formations shown in the map (plate 7) Water Supply Paper No. 123, U. S. Geological Survey, is considerably at variance with the facts, as is likewise the discussion given there and elsewhere of the faults and unconformities. But discrepancies of this kind are doubtless inevitable in observations made from car windows and through a field glass. A large number of formation names are proposed, but we look in vain for detailed sections or descriptions showing their character and distribution. In this respect the author does not seem to be in accord with leading geologists generally, who maintain that no formation name should be proposed without adequate definition. The correlation of formations in regions widely separated, where detailed maps and careful paleontological studies are wanting, is usually regarded as a hazardous undertaking, but Dr. Keyes does not appear to find it so. If Dr. Keyes has at hand the data upon which these conclusions are based it is to be regretted he has not published them. We are told that this 'correlated scheme of rock succession' is based on information obtained through the work of the "Geological and Mineral Survey of New Mexico under the direction of the School of Mines at Socorro." Unfortunately we have no knowledge of such an organization aside from the mention made of it in this connection. Geologists generally would be interested to know something of an organization carrying on so important a work. It appears to be wholly unknown even in New Mexico.

These exceptions are possibly of no consequence and if his attention were called to them the author would doubtless reply, as in a former instance when the writer of this note reminded him that a fossil he had figured was wrongly named, that it was a 'matter of no importance.'

As a whole the papers on New Mexico geology which issue from the above named writer's pen in such rapid succession abound in inaccuracies, while the absence of detailed description or evidence of careful field work deprives them of any value they might otherwise possess.

C. H. GORDON

U. S. GEOLOGICAL SURVEY,
August 25, 1906

[1] The announcement of the discovery of these formations in New Mexico was first made by L. C. Graton and the writer in SCIENCE for April 13, 1906, p. 590.

[2] *American Journal Science,* 4th Ser., Vol. 21, p. 394, 1906.

GEOLOGICAL WORK IN ARKANSAS BY PROFESSOR PURDUE

TO THE EDITOR OF SCIENCE: A paragraph in my letter to Doctor Branner, published in the issue of SCIENCE of December 7, is possibly open to misconstruction and may do injustice to Professor Purdue, of the University of Arkansas. The paragraph is as follows:

As to the invasion of fields occupied by professors of geology, there are in the files of the survey many letters to such professors urging them to work up the local geology and offering financial assistance and means of publication of their results. The case of the Fayetteville quadrangle is perhaps an apparent exception. It should be stated, however, that when the work was undertaken there Professor Purdue was practically unknown as a geologist and was, as a matter of fact, not sufficiently experienced to carry on independent work. Since his season with Adams he has been employed each summer and has submitted three folios for publication. It has been necessary, however, in connection with this work, to send more experienced men into the field with him, although he will receive the entire credit for the work.

It was not my intention to cast any reflection, even in a personal letter, upon Professor Purdue's work, the quality of which has been higher than that of most work done for the survey under similar conditions. When he was first entrusted with independent work, however, his field experience was less than is required for the regular members of the survey and his ability in this direction was not known to us. It is for this reason, and not because of any deficiency in the quality of his results that more experienced men have conferred or collaborated with him in the field. This course is, indeed, frequently necessary with regular members of the survey, as well as with per diem men. That Professor Purdue's work is regarded as good is sufficiently shown by the fact that an allotment for its continuance has been made every year since it was begun. A further point in his favor, and one highly appreciated, is that his results are submitted when promised.

Chas. D. Walcott,
Director

SPECIAL ARTICLES

EVIDENCE OF MAN IN THE LOESS OF NEBRASKA

After careful investigation the writer stands ready to announce his belief in the occurrence of human remains in the loess of this state, and for this primitive type he has proposed the name Nebraska loess man.[1]

[1] Nebraska Geological Survey, Vol. II., parts 5 and 6.

Such importance attaches to the discovery as to warrant a paper devoted to the geological facts connected therewith.

Physiographic Features.—North of Omaha for a number of miles the topographic features are bold and abrupt for a prairie country due to the proximity of the Missouri River, the relief being 150 to 200 feet.

On all sides landslides are in evidence and must be reckoned with in all field work. Early in October Mr. Robert F. Gilder, of Omaha, opened a mound on Long's hill facing the Missouri River, ten miles north of Omaha or three miles north of Florence, Douglass County, Nebraska. From Florence north to Long's hill there is a continuous section along the roadside for about three miles and from the base of Long's hill to the summit, on which Gilder's mound is situated, there is an unbroken section, hence the geology of the place is well exposed, and being simple is easily interpreted. The public highway, which is about forty feet above the river level, is just upon the top of the Carboniferous, the dark carbonaceous shales of which constitute a distinct geological feature. Upon the shales there rests an average of ten to twenty feet of glacial drift containing occasional Sioux quartzite and granitic boulders. Upon the drift comes 150 feet of bright buff loess such as is conspicuous in and around Omaha and Council Bluffs.

Long's hill stands 200 feet above the river level, and 150 feet above the valley out of which it rises. It is a hill of erosion, and no discoverable land slip has complicated its simple geology. On its summit is Gilder's mound, in the superficial layer of which were found mound-builder remains, and in the deeper layer eight skulls and many bones of a still more primitive type. The writer at once joined Mr. Gilder in a critical investigation of the place, continuing the work from time to time to December 2, 1906, with results leading to the conclusion that two of the skulls are mound builders', in all probability. These were found in the upper layer readily discernible as a mixture of black soil and light buff subsoil such as would result from digging and burying. This layer has a thickness of two and one half feet. Below it was an undisturbed layer of unmistakable loess, and in it numberless fragments of human bones and an occasional animal bone, loess shells and stray angular pebbles.

In brief, the conclusion is that in the case of the upper bone layer there was burial, in the lower, deposition. Those in the loess doubtless antedate the hill itself while those in the upper layer are subsequent to it. That archaic burial could have taken place in loess without detection is altogether improbable. Of necessity there would result a mixture of black with light soil and a breaking up of the lithologic structure. Where these bones occur the loess structure and color is perfectly preserved and it contains characteristic vertical lime-tubes, concretions and shells precisely as is customary. Out of the evidence at hand the writer concludes that bones of this layer were strictly synchronous with the loess formation in which they were found, in substantiation of which comes the fragmental nature of all of the bones, their water-worn condition, their range of distribution, and disassociation of parts.

One would scarcely think of such conditions being possible in the case of human burial; besides it is improbable that a primitive people would dig graves to a depth of twelve feet. Should a people without tools and appliances perform such an improbable feat, would they bury water-worn fragments, would they scatter them so widely as not to exceed five or six pieces to the cubic yard? How could they replace the earth in the grave in such order and regularity that there would be perfect structure and gradation of color from soil to subsoil?

Methods of Exploration.—Early in November the writer recognized that the bones in the loess were apparently fossil, and great care was exercised in all subsequent work. On extending the cross trenches which Mr. Gilder had dug, human bones scattered, water-worn, fragmentary and unrelated were found in natural undisturbed loess at all levels down to six feet. The most interesting single bit found on this occasion was the left half of a frontal bone secured at a depth of four to five feet. Later at a distance of five feet the other half was dug up, and the two parts fit together, completing an interesting low-browed frontal. A jaw, which was found in undisturbed loess at a depth of four feet, was that of a youth. The crowns of the teeth were scarcely worn, so old age can not be assigned as the reason for the absence of all teeth save molars Nos. 2 and 3 in the right ramus and No. 2 in the left. Just as the teeth of any water-soaked jaw drop out readily, so it seems to have been with this one. The inference is that they were lost in the process of deposition. A week later work was resumed, the writer being accompanied by Mr. Robert F. Gilder and Dr. George E. Condra, and the attempt was made to be severely critical and careful.

All surface material was carefully removed and three wide shafts were sunk on the northern, eastern, and southern points of the mound. Each shovelful of earth was scrutinized, all bone fragments carefully saved and recorded. In all some twenty bits were found, as follows: a fragment from the base of a skull, fragments of ribs, limb bones, scapula and sacrum; a clavicle, calcaneum, three complete vertebræ, two metapodes and a phalanx.

Some of the bits mentioned were but slivers, other bits were two or three inches long. Some were badly etched by water, others gnawed by rodents. As each fragment was unearthed a block of the matrix was kept and as far as possible each fragment was preserved in position in the block.

There were but twenty fragments in this lot, for while it is true that the shafts were sunk to a depth of eight feet, and while bone chips were found at all levels, they were widely scattered and few in number. Among the fragments may be mentioned five or six bits of skull, as many bits of rib, the angle of a jaw, metatarsal No. 3, and two phalanges, and with them bits of *Anadonta, Succinea avara,* and several angular pebbles. When work was resumed a few days later a circle thirty feet in diameter was described concentrically about the mound, which is about eighteen feet in diameter. The northeast quadrant of the circle was divided into sectors of twenty-two and a half degrees and lettered. This quadrant as a whole was excavated to an average depth of six feet its periphery to an average depth of eight to nine feet, and a shaft was sunk to a depth of twelve feet on the north edge.

The writer was accompanied and aided by Dr. George E. Condra, Edwin Davis, Paul Butler, and as time would permit by Mr. Gilder. Systematic work was continued for three consecutive days. Fragments of human bones, scattered and unrelated, were found throughout the quadrant at all levels even to the depth of eleven and one half feet.

It was plainly demonstrated that the part without the circle of the mound was quite as rich in bone fragments as that within. The relation of the two sets of bones may be viewed as purely accidental. In but a single instance were several bones found together. Three ribs, fragments of limb bones, and an astragalas were in proximity. Probably two hundred fragments were exhumed on this occasion. It should be noted that no whole bones were found excepting a few phalanges. Instead they are bone-chips and splinters, with an occasional section from a limb bone, and many of the fragments are pitted or etched. Out of this set the following fragments seem of especial interest: half of a jaw with a solitary molar, the condyle, angle, and region of the symphysis being weathered off, fragmentary rami of two other unrelated jaws, the bony palate with the two back molars in place. By far the most interesting and instructive specimen found at this time was a skull completely disarticulate, broken, and scattered over a space five by five feet.

This was taken out in blocks, and no attempt will be made to remove the bits from their original position, the intention being to keep everything in such condition as to facilitate the detection of inaccuracies and errors.

Age of the Supposed Loess Man.—The present paper concerns itself simply with the announcement of human remains found in undisturbed loess. The chief point is the evidence that human remains have been found in the loess, and whether this is the very oldest or newest loess seems a secondary consideration. The loess here is not leached of lime salts, but is actively effervescent at all levels, arguing for recency of deposition. All recognize the chronological diversity in the loess formation, and whether Long's hill is in the main loess body, as we believe it to be, or in a much more recent one does not materially affect the relation of the bones to some stage of glaciation, the precise glacial or interglacial age being as yet undetermined.

The loess in question rests on Kansan drift, and though as young as the later Wisconsin sheet or younger, it is nevertheless old.

Erwin Hinckley Barbour
The University of Nebraska,
December 14, 1906

ASTRONOMICAL NOTES

THE UNITED STATES NAVAL OBSERVATORY

The Naval Observatory is showing an activity in astronomical work and publication, which ought to go far toward creating a more favorable opinion of that institution than has sometimes prevailed in the past. Several volumes have recently appeared, containing observations of the sun, moon, planets and miscellaneous stars from 1900 to 1903, of the sun, moon, planets and comets from 1866 to 1891, and of standard stars and zodiacal stars from 1900 to 1902; also, reduction tables for transit circle observations and meteorological observations.

Several hundred pages and a large number of plates are also devoted to an elaborate study of the total solar eclipses of May 28, 1900, and May 17, 1901. The leading members of the astronomical department of the observatory are: Professors Skinner, Eichelberger and Littell and Assistant Astronomers Hill, Rice and Hammond, under the superintendency of Rear-Admiral Asa Walker, U.S.N. A large number of astronomers from other institutions assisted in the work of the eclipse expeditions.

THE SOLAR OBSERVATORY OF THE CARNEGIE INSTITUTION

The solar observatory on Mount Wilson continues to surprise the astronomical public with its developments. A five- or six-foot mirror has perhaps appeared to most astronomers to mark the limit to practical construction, and to usefulness in most lines of astronomical work. Professor Hale, however, comes forward with the announcement that he is prepared, through the generosity of Mr. John D. Hooker, of Los Angeles, to undertake the construction of a reflecting telescope having an aperture of eight feet and four inches. Such an undertaking is of necessity somewhat in the nature of an experiment, but no one is better able to overcome the technical difficulties involved than Professor Ritchey, Mr. Hale's able assistant.

Meanwhile researches of importance are being pushed with the present equipment. Recent observations of sun-spots, taken in connection with spectroscopic studies carried on in the new laboratory, show that the differences between the spectra of the photosphere and of sun-spots are due to differences in temperature. It has also been shown that certain stars, as Arcturus, have typical sun-spot spectra, and are thus at a lower temperature than our sun.

POSITIONS OF STARS IN THE GREAT CLUSTER IN HERCULES. LUDENDORFF

THE positions of 833 stars in the great cluster in Hercules were determined photographically by Dr. Scheiner, of Potsdam, in 1892. After thirteen years, Dr. Ludendorff, of the same observatory, has independently determined the positions of 1,136 stars in the same cluster. Of course, the largest part of the stars measured by Scheiner were remeasured by Ludendorff. Both observers determined the brightness as well as the positions of the stars. The accordance between the results obtained by these two observers is in general extremely good. Ludendorff measured the stars on two plates, and from a comparison of the two determinations an idea can be formed of the precision which was attained. Grouped according to distance from the center in R.A., the differences range from $-0.15''$ to $-0.22''$. As might be expected, the mean differences increase systematically toward the center of the cluster, where the measurements are most difficult. Among 1,588 differences only 22, or 1 in 72, amount to more than $1''$. The probable error of a catalogue position in R.A. is given as $0.179''$, and in declination $0.180''$. These values are somewhat smaller than the corresponding values in the work of Scheiner. Small systematic differences appear, however, between the two determinations by Ludendorff, and especially in the determinations of declination between Ludendorff and Scheiner.

The importance of precise measurements of the positions of the components of such clusters as that in Hercules can hardly be overestimated. Thirteen years may be too brief an interval for the determination of the proper motion of the cluster and the stars which undoubtedly are projected upon it, and much more so of the motions of the individual members of the group. When a suitable time shall have elapsed, however, these determinations should be of high value in the solution of the fascinating problems which are associated with the globular clusters.

Ludendorff finds only small changes in the brightness of the two variable stars discovered in this cluster by the writer, and no evidence of the variability of any other stars.

POSITIONS OF STARS IN THE CLUSTERS *h* AND χ PERSEI. YOUNG

AN investigation similar to the preceding appears also as No. 24 of the 'Contributions from the Observatory of Columbia University.' This is an elaborate and valuable determination of the positions of 145 stars in the double cluster in the sword-handle of Perseus, by Anne Sewell Young. These clusters are little condensed, but are beautiful objects when seen with a low power. The plates employed were made by Rutherfurd during the years 1870–1874; they had double exposures of about six minutes. The measured stars were of the tenth magnitude and brighter. The methods of reduction have been in general those of Jacoby. The results appear to be of the highest precision and in good accord with the best work which has been done before.

RESEARCHES IN STELLAR PHOTOMETRY. PARKHURST

A VALUABLE contribution to our knowledge of the variable stars has been made by Mr.

John A. Parkhurst, in a volume issued through the Carnegie Institution. This contains the results of observations by the author, during twelve years, of twelve variable stars of long period, and includes photometric determinations of the magnitudes of the comparison stars, measures of the light of the variables, and detailed and mean light-curves. Many of the recent measures were made with the forty-inch refractor, and are invaluable, since they furnish our only information in regard to the minima of some variables of large range.

Excellent photographic charts of the regions of the variables are given. It is unfortunate, however, that astronomers are not in agreement in regard to the scales of star charts. For terrestrial maps definite scales are generally employed. For astronomical charts a scale of one minute to the millimeter seems to be a natural one, with simple multiples and divisors of this scale, when necessary. This subject might be referred to a national or international committee.

S. I. Bailey
Harvard College Observatory

CURRENT NOTES ON METEOROLOGY

CLIMATE AND CLIMATIC CHANGES IN KASHMIR

Ellsworth Huntington, whose work on Turkestan as a member of the Pumpelly Expedition of a few years ago is already well known, and who has more recently been engaged in further exploration of Central Asia, notably of Chinese Turkestan, contributes to the *Bulletin of the American Geographical Society* for November, 1906, an account of his studies in the Vale of Kashmir in 1905. The climate is described as warm and damp from June to August, though but little rain falls; mild and delightful in April, May, September and October; and cold and snowy in winter, when 'bracing' is not infrequently less true to the actual conditions than 'rigorous.' Of late years there has been an increasing influx of English summer visitors from India, who seek relief from the heat of India in the cooler and more favorable climate of the Vale of Kashmir. A study of the physiographic features of the region, especially of the river terraces, as well as of the human history, leads to the conclusion that there has been a transition from colder or damper climatic conditions two thousand years or more ago to warmer or drier conditions to-day. This transition appears to Huntington to be part of a wide-spread climatic change extending at least from Persia and the Caspian Sea on the west to the borders of China proper three thousand miles away on the east.

MONTHLY WEATHER REVIEW

No. 9, Vol. XXXIV., of the *Monthly Weather Review,* contains the following articles of general interest: 'The Relation of the Weather to the Flow of Streams.' In this paper F. H. Brandenburg, District Forecaster at Denver, shows how many factors, meteorological and physical, control the run-off of streams. A 'Phenomenal Rainfall at Guinea, Va.,' on August 24 last, is reported by E. A. Evans, Section Director at Richmond, Va., to have yielded nine and a quarter inches in about thirty minutes. Professor Arthur Searle, of the Harvard Observatory, contributes a paper on 'The Zodiacal Light,' in commenting on which Professor Cleveland Abbe says editorially: "As this article by Professor Searle definitely settles the old question as to whether the zodiacal light and *Gegenschein* are atmospheric or celestial phenomena, we shall hereafter commend the publication of such material to the astronomical journals, and reserve the columns of the *Monthly Weather Review* for meteorology proper." 'The Direction of Local Winds as affected by Contiguous Areas of Land and Water,' by T. H. Davis. 'The West Indian Hurricanes of September, 1906,' by Professor E. B. Garriott. The development of hurricanes in this month was exceptionally active, a fact which the writer attributes, in part at least, to an unusually strong flow of air from the more northern latitudes toward the tropics. One of these storms, it will be remembered, caused serious damage at Pensacola and at Mobile.

A DISAPPEARING LAKE

Additional evidence regarding the desiccation of Lake Chad, in central Africa, is accumulating. Captain Tilho, of the recent Anglo-French Boundary Commission, points out that since the explorations of Barth and Nachtigal the form and area of Lake Chad have been profoundly changed. Navigation is only possible in certain places, and boats continually run aground. Instead of the great waves which, during strong winds, gave the lake the appearance of an ocean, there is now a tendency toward the development of a vast marsh (*Ciel et Terre,* November 16, 1906). It may be noted, in this connection, that there is nothing unreasonable in the supposition that Lake Chad is undergoing a temporary desiccation, which may again be followed, after some years, by another period of high water. R. DEC. WARD

JOHN M. BROOKE

AT his home on the outskirts of Lexington, Va., on December 14, within one week of his eightieth birthday, Colonel Brooke passed away.

John Mercer Brooke was born December 18, 1826, near Tampa, Florida. His father, General George M. Brooke, of Virginia, was a distinguished officer in the war of 1812, and his mother, Miss Thomas, was a native of Massachusetts. At the age of a little over fourteen years he became a midshipman in the navy, and three years were spent in cruising. In 1847 he was graduated from the Naval Academy at Annapolis, and soon afterward was assigned to work in the coast survey. From 1851 to 1853 he was stationed at the Naval Observatory in Washington, where began his life-long friendship with Matthew F. Maury, the distinguished hydrographer.

For several years prior to the civil war Lieutenant Brooke was engaged in making hydrographic surveys in the Pacific Ocean, particularly in the archipelago and along the coasts of China and Japan. It was in 1854 that Commodore M. C. Perry induced the Japanese to sign their first foreign treaty by which trade was opened with the United States, and good treatment was promised to shipwrecked crews. Brooke was thus allowed ready access to Japan, and while he was sojourning in Yeddo in 1859 his ship was destroyed by a typhoon. He remained a number of months at Yokohama, during which he did much to develop the confidence of the Japanese in their foreign friends. They decided to send an embassy to the United States and invited Brooke to accompany it. So highly was he esteemed that he was invited by the Japanese ambassadors to help himself from a large chest of native gold, but this he declined. On the arrival of the embassy at Washington the first request of the ambassador was that the services rendered by Brooke to Japan should be recorded in the archives of the United States.

It was during his extended hydrographic work in the Pacific that Brooke thoroughly tested his deep-sea sounding apparatus, the invention for which probably he became best known. He had previously originated it at the Naval Observatory. With but few modifications his method has continued in use to the present time. It has been one of the most important elements in extending our knowledge of ocean depths and in rendering possible the first successful ocean cables.

Soon after Brooke's return to America the country became rent by civil war. Along with Maury he cast his lot with the seceding states, and the rest of his life was spent in Virginia. As a Confederate officer he gave his attention especially to naval ordnance. While Parrott was experimenting at West Point on the improvement of cast-iron cannon by reenforcement of the breech with a wrought-iron jacket, Brooke was absorbed in similar experiments at Richmond and Norfolk, and the Brooke guns were conceded to be the best made at the south. While Ericsson was developing his *Monitor* at Greenpoint Brooke and his associates were building the first Confederate ironclad, known as the *Merrimac,* which took part in the dramatic naval engagement at Hampton Roads. He remained at the head of the ordnance department of the Confederate navy until this navy ceased to exist.

After the close of the war Maury and Brooke became associated as professors in the Virginia Military Institute at Lexington, where Maury died in 1873. Brooke continued to hold the chair of physics and astronomy until 1899, when the infirmities of old age necessitated his retirement with the rank of colonel. From that time until his recent death he lived in strict seclusion, retaining his connection with the institution as professor emeritus.

Personally Colonel Brooke was in his old age somewhat taciturn, retiring and singularly indifferent to popular recognition. The extraordinary influence which he exerted upon the Japanese shows that in his young manhood he was much more communicative, and that he was the possessor of great force, both of mind and of character. His ideals were lofty, and his fidelity to these and to his friends was unswerving. On coming to Lexington in 1866 he came into a congenial coterie that included such men as Robert E. Lee, Custis Lee, Pendleton, Letcher, Williamson and Maury, all of whom had held high office in the Confederacy. Among these intimates he was genial, full of humor and full of resources. Despite his modesty he was positive, a good hater, an intolerant foe to shiftiness and sham. In the performance of duty he was uncompromising to such an extent as to appear at times eccentric. These qualities became more pronounced with advancing years, and as death carried away one after another of his old friends he gradually became silent and exclusive. But to a willing ear he had a plenty to give, and the present writer remembers with pleasure his first interview with the solitary graybeard to whom he had just introduced himself. The old genial spirit came back as he became eloquent over his reminiscences of the Sea of Japan. During the last few years he has rarely ever been seen or heard, and the arrival of death was so gentle as to be scarcely recognized.

W. LeConte Stevens

PUBLICATIONS OF THE AMERICAN ETHNOLOGICAL SOCIETY

Announcement.—The American Ethnological Society is about to begin a series of publications which is to contain authentic material collected among native tribes of America. The volumes are to appear at irregular intervals.

Notwithstanding the large amount of work that has been done on American ethnology, comparatively little material has been collected regarding the customs, beliefs, and ideas of the natives in their own words. Most of our collections have been obtained indirectly through the assistance of interpreters, or are discussions of information collected from individuals more or less familiar with English or with the trade jargon.

Knowledge possessed by the Indians is of great importance as well to the ethnologist as to the student of the early history of the American continent. For this reason authentic records of information given by the Indians seem to be of prime importance for a thorough study of these subjects.

The American Ethnological Society, in beginning its series of publications, is desirous of collecting and preserving for future use such records, and it is hoped that this undertaking will meet with the support of the public.

The following volumes of the publications of the American Ethnological Society are in preparation:

Vol. I. William Jones, Ph.D., research assistant, Carnegie Institution, Fox Texts. In press. A collection of historical tales, myths, and accounts of personal religious experiences collected among the Fox Indians, a branch of the Algonquian stock. Recorded in original text, and published with translations.

Vol. II. Edward Sapir, 'The Upper Chinook.' In press. An account of the Chinook Indians of the Upper Columbia River, and a collection of myths and personal accounts. Original texts and translations.

Vol. III. Roland B. Dixon, Ph.D., instructor in anthropology, Harvard University, 'Myths of the Maidu Indians of California.'

Vol. IV. Franz Boas, Ph.D., professor of anthropology, Columbia University, 'Myths of the Tsimshian Indians of British Columbia.'

Vol. V. Roland B. Dixon, Ph.D., professor of anthropology, Harvard University, 'Myths of the Shasta Indians of Northern California.'

It is hoped that the following collection will also be published at an early date:

Livingston Farrand, Ph.D., professor of anthropology, Columbia University, 'The Alsea Indians of Oregon.'

H. H. St. Clair, 2d, 'Texts collected among the Coos Indians of Oregon.'

The price will be approximately $2.00 for a volume of three hundred pages, and proportionately for larger or smaller volumes. It is hoped to bring out about two volumes a year.

James Grant Wilson, *President*.
Franz Boas, *Vice-President*.
Harlan I. Smith,
Corresponding Secretary.
Marshall H. Saville,
Recording Secretary.
George H. Pepper, *Treasurer*.

SCIENTIFIC NOTES AND NEWS

Dr. E. L. Nichols, professor of physics at Cornell University, president of the American Association for the Advancement of Science, was elected president of the American Physical Society at the New York meeting.

Professor George F. Atkinson, of Cornell University, has been elected president of the Botanical Society of America.

Officers of the Association of American Geographers were elected at the New York meeting as follows:

President—Professor Angelo Heilprin, Sheffield Scientific School.

First Vice-president—Professor Ralph S. Tarr, Cornell University.

Second Vice-president—Mr. G. W. Littlehales, U. S. Hydrographic Office, Washington, D. C.

Secretary and Treasurer—Professor A. P. Brigham, Colgate University.

Councillors—Professor W. M. Davis, Harvard University; Mr. Cyrus C. Adams, American Geographical Society; Professor J. Paul Goode, University of Chicago.

The Brazilian government proposes to establish a national geological survey under the direction of Dr. O. A. Derby, who was for many years geologist of the state of S. Paulo. Dr. Derby went to Brazil in 1875 as a member of the extinct Commissão Geologica, of which Professor C. F. Hartt was the chief. He has lived in Brazil ever since, and is the leading authority on Brazilian geology.

The lords commissioners of the admiralty have appointed Sydney S. Hough, Esq., F.R.S., chief assistant to the astronomer at the observatory, Cape of Good Hope, to be astronomer at that observatory on the retirement of Sir David Gill, K.C.B.

Professor Arthur Schuster has resigned his position as Langworthy professor of physics and director of the physical laboratories at Manchester. Dr. Schuster's connection with the university dates from 1871, when he entered Owens College as a student. In 1873 he held the post of demonstrator under Professor Balfour Stewart, and in 1881 he was appointed to the newly created chair of applied mathematics, which he resigned to become professor of physics in 1888. Both the council and the senate have placed on record by formal resolutions their regret at Professor Schuster's resignation.

Professor L. C. Miall, D.Sc., F.R.S., will retire from the chair of botany in the University of Leeds in June, after thirty-one years' service.

Dr. G. R. Parkin has sailed from Liverpool to supervise the examinations for Rhodes scholarships throughout Canada and the United States. His address till the middle of February will be McGill University, Montreal.

Mr. H. J. Mackinder, M.A., director of the London School of Economics, has been appointed to represent the university at the Ninth International Congress of Geography to be held at Geneva in the summer of 1908.

Sir Alfred Jones, president of the British Cotton-growing Association, has invited a party to accompany him to the West Indies in the interests of cotton-growing in the British colonies.

On December 18 Dr. William Osler unveiled a portrait of the late Dr. J. E. Graham, formerly professor of medicine in Toronto University, a gift by the widow and son, Dr. J. S. Graham, to the Ontario Medical Library.

The Rev. Alexander Harper, M.A., Wishaw, has presented to the University of

Aberdeen with a fine collection of precious stones "in memorial recognition of the scientific eminence, personal worth, and perennial influence of James Nicol, professor of natural history, 1853–78."

The deaths are announced of Dr. Karl Garzarolli, associate professor of chemistry at Vienna, and of Professor Lorenzo Tenchini, professor of anatomy at Palma.

There will be a civil service examination on February 5, to fill the position of chief food and drug inspection chemist in the Bureau of Chemistry of the Department of Agriculture at a salary of $3,000, and of food and drug inspectors, at salaries of $2,000. On the same day there will also be an examination for assistant in sugar-beet investigations at a salary of $1,200 to $1,400.

An examination will be held in Chicago on January 22, under the direction of the Civil Service Commission, to select a psychopathologist for the Cook County Insane Hospital, Dunning. The salary is fixed at $2,400, in addition to living expenses, and an excellent opportunity for original research is furnished.

The honorary treasurer of the Imperial Cancer Research Fund has received from Mr. and Mrs. Bischoffsheim the munificent donation of £40,000 on the occasion of the celebration of their golden wedding.

The International Association of Academies will hold its third general meeting at Vienna, beginning on May 29.

The Astronomical and Astrophysical Society of America at the New York meeting during convocation week reelected its former officers and determined that its next meeting shall be held in the summer of 1908. The president is E. C. Pickering, of Cambridge, Mass.; the secretary is G. C. Comstock, of Madison, Wis.

The American Forestry Association held its annual meeting at Washington beginning on January 9. The report of the board of directors was read by Dr. Thomas E. Wills, secretary, and addresses were made by Secretary Wilson, Mr. Gifford Pinchot, Professor H. S. Graves and others.

The New York Association of Biology Teachers will hold its annual meeting at the High School of Commerce on the evening of January 25. Professor Gary N. Calkins, of Columbia University, will speak informally on 'The Life Cycle of Protozoa.'

The preliminary program of the seventh meeting of the Congress of American Physicians and Surgeons on May 7, 8 and 9, has been issued. Sixteen national societies devoted to the medical sciences will meet in affiliation at the congress. The program of the congress itself is as follows:

Tuesday, May 7, 3 P.M.—'The Historical Development and Relative Value of Laboratory and Clinical Methods in Diagnosis.'

Dr. William Osler, of Oxford, England: 'The Evolution of the Idea of Experiment in the Study of Medicine.'

Dr. Lewellys F. Barker, of Baltimore, Md.: 'On Neurological and Psychiatric Diagnosis.'

Dr. Alfred Stengel, of Philadelphia, Pa.: 'On Chemical and Biological Diagnosis.'

Dr. Richard H. Cabot, of Boston, Mass.: 'On Physical Diagnosis.'

Followed by a discussion by Prof. Fred'k Müller, of Munich, Dr. George Blumer, of New Haven, and others.

8 P.M.—Address by the President of the Congress, Reginald H. Fitz, M.D., LL.D. To be followed by a reception.

Wednesday, May 8, 3 P.M.—'The Comparative Value of the Medical and Surgical Treatment of the Immediate and Remote Results of Ulcer of the Stomach.'

Dr. John H. Musser, of Philadelphia, Pa., and Dr. Charles G. Stockton, of Buffalo, N. Y.: 'On the Indications for, the Methods of, and the Results to be expected in the Medicinal Treatment.'

Dr. William J. Mayo, of Rochester, Minn.: 'On Surgical Treatment of Acute Ulcers of the Stomach, including Perforations and Hemorrhage'; and Dr. John C. Munro, of Boston, Mass.: 'On Chronic Ulcers and the Indications for Surgical Treatment.'

Followed by a discussion by Mr. B. G. A. Moynihan, of Leeds, England; Dr. A. Jacobi, of New York; and others.

Thursday Evening, May 9, 8 P.M.—Smoker.

The permanent commission of the International Seismological Association has resolved to hold a competition for the construction of a seismometer recording the movements of the soil during earthquakes which have their origin near the place of observation. The instrument must satisfy the following conditions: (1) It must be able to register both horizontal and vertical movements. (2) It must be of simple construction. The record must give a magnification of not less than 40 to 50 times. (3) The selling price of the instrument, with the registering apparatus, must be as low as possible ($75 approximately). Four prizes are offered, having the respective values of about $250, $175, $125 and $75. The instruments must be sent at the expense and risk of the competitors to the Vice-president of the Association, Dr. J. P. van der Stok, De Bilt, Netherlands, before September 1, 1907, and are to be exhibited at the general meeting of the association which will be held at The Hague during the month of September. The efficiency of the instrument will be investigated at the Central Bureau, Strasburg, and the awards will be determined by a jury composed of five seismologists chosen by the permanent commission. The results will be announced at Easter, 1908. For further particulars apply to Professor G. Gerland, director of the Central Bureau, Strassburg i. E.

DURING the past season, Dr. W. W. Atwood began an investigation of the more important coal fields of Alaska. This work will probably occupy at least three years and will include (1) a broad study of the coal-bearing formations with the aim of correlating the coal horizons in the different portions of the territory, (2) a comparative study of the coals, including the sampling for analysis of all workable coal seams investigated, (3) estimates of the amount of coal available, and (4) a study of the methods of mining and marketing Alaskan coals. The plan for the first season was to visit the Controller Bay and Cook Inlet coal fields. The early part of the second season, 1907, will probably be spent in southeastern Alaska, but most of that summer's work will be done in the interior. It is expected that during 1908 the studies along the coast will be resumed and pushed westward so as to include all important coal fields on the Alaskan peninsula and neighboring islands.

THE secretaries of the organizing committee on the union of medical societies having their headquarters in London, have written to the *British Medical Journal* as follows: Twelve societies have decided to join the union, namely: British Electro-Therapeutic; British Gynæcological; British Laryngological, Rhinological, and Otological; Clinical of London; Dermatological of Great Britain and Ireland; Dermatological of London; Epidemiological; Neurological; Odontological of Great Britain; Obstetrical of London; Pathological of London; Royal Medical and Chirurgical. Four societies are willing to join under certain conditions, namely: British Balneological and Climatological; Laryngological of London; Otological of the United Kingdom; Therapeutical. In the case of some societies, such as the Anatomical of Great Britain and Ireland, the Physiological, the Medico-Psychological, and the Medico-Legal, there are peculiar difficulties, but the members of certain of these societies have indicated their willingness to form similar sections in the new society. Five societies have definitely refused to join the union, namely: Life Assurance Medical Officers' Association, Medical Society of London, Ophthalmological Society of the United Kingdom, Society of Anæsthetists, Society for the Study of Diseases of Children. The first meeting of the representatives of the societies which are forming the union will be held at 20 Hanover Square on Friday, January 18, at 5 p. m.

THE U. S. Geological Survey has published a report by Messrs. C.-E. A. Winslow and Earle B. Phelps of their investigations on the purification of Boston sewage. In an introductory chapter written by Professor William T. Sedgwick the explanation is made that an anonymous friend of the Massachusetts Institute of Technology had, in 1902, presented to that institution the sum of $5,000 a year for three years, for the purpose of making experiments on sewage purification and of giving the widest possible publicity to means or methods by which the present too often crude and imperfect systems may be improved.

It was decided to establish a sanitary research laboratory and sewage experiment station on the main trunk sewer of the south metropolitan system of the city of Boston. Professor Sedgwick was made director of the work, Mr. Winslow was installed as biologist in charge of the laboratory and station, and Mr. Phelps as research chemist and bacteriologist. The report now made by them is not final, for experiments are still in progress, the donor of the original gift having consented to continue the work for the fourth and fifth years.

UNIVERSITY AND EDUCATIONAL NEWS

Announcement has been made that Mr. John D. Rockefeller will endow the University of Chicago with $3,000,000 to maintain a pension fund, the institution having been excluded from the scope of the Carnegie Foundation, owing to its denominational control.

It is also reported that Mr. Rockefeller has agreed to give $2,000,000 for the endowment of a university for Louisville, provided a similar amount is raised by those interested in the new institution. A plan has been practically agreed upon whereby the University of Louisville will unite with the proposed universities of the Baptist and Methodist churches, and property now occupied by the House of Refuge will be donated by the city as the site for the university. The Methodist church has already at hand $250,000. The Baptist church has promised $260,000, and the University of Louisville has pledges for over $200,000.

The chair of chemistry at the University of Pennsylvania, held by Dr. Edgar F. Smith, vice-provost of the university, has been anonymously endowed by a gift of $100,000.

Mr. Samuel W. Bowne has given to Syracuse University, of which he is a trustee, a chemical laboratory, costing $100,000.

The packing interests of Chicago have offered to the University of Illinois the sum of $250,000 with which to establish in that city a veterinary college.

The vice-chancellor of the University of Cambridge announces recent contributions to the benefaction fund: £5,000 from the Goldsmiths' Company to the library; £2,500, part of the sum received in response to the appeal on behalf of the library; £1,600, resulting from the general appeal of the Cambridge University Association to general university purposes; £904 for the building fund for the new Museum of Archeology and Ethnology. Also subscriptions of £12,325 (including £5,000 from the Drapers' Company, and £1,000 each from six individual contributors) towards the building fund of the department of agriculture.

Glasgow University has received £6,500 from Mr. John S. Dixon, to raise the lectureship in mining to the status of a chair; and £5,000 from the Graham Young trustees towards the endowment of a lectureship in metallurgical chemistry.

The professorship of pure and applied mathematics in the University of Otago is vacant. Particulars can be obtained from the High Commissioner for New Zealand, 13 Victoria Street, London, S.W.

Professor B. E. Fernow, formerly director of the New York State College of Forestry at Cornell University, has been appointed to organize a forest department in the State College of Pennsylvania on the same lines as the Cornell institution, making it a first-class undergraduate forestry school.

Mr. J. F. Breazeale, of the Bureau of Soils, has been appointed assistant professor of experimental agronomy at the Pennsylvania State College. He will make a study of the rotation fertilizer plots which have been conducted by that station for twenty-four years. Mr. C. L. Cook and Mr. F. R. Reid have been assigned by the Bureau of Soils to assist in these and make other soil investigations. Mr. C. F. Shaw has been appointed instructor in agronomy and Mr. J. H. Barron assistant in experimental agronomy.

Professor Ernest Rutherford, Macdonald professor of physics in McGill University, has been appointed to succeed Professor Schuster as Langworthy professor and director of the physical laboratories at the University of Manchester.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, JANUARY 25, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ON SOME POINTS OF IMPORTANCE TO ANATOMISTS[1]

IT has been said that the president of a scientific association should not burden its meeting with an address in case the communications to be made to it are numerous and of value. If this be true it seems to me that we have reached a stage at which silence on my part would be appropriate, but I can not resist the temptation to exercise my right to speak, for a few minutes only, regarding some points which I consider to be of vital importance to our profession and to this association.

I think we have safely passed the pioneer stage in the development of scientific professions in America and it is unnecessary now to formulate the forces which have brought this change about, for they are known to us all. However, during the development of a science, it is well for the workers in it to meet from time to time to pass judgment upon the recent progress that has been made. Their approval, given on such an occasion to an investigator, is a most encouraging stimulus and election by his colleagues to the presidency of such a meeting is the highest honor a scientist can receive. I accept with gratitude this elevated post to which you have appointed me and regard it not as approbation of myself, but rather as a mark of appreciation of the co-workers, whom I have had the good fortune to have associated with

[1] Remarks by the president at the twenty-first meeting of the Association of American Anatomists, New York, December 27, 1906.

me in Baltimore, and of their contributions to anatomy.

More than a century ago the status of anatomy in America compared favorably with that in Europe, but the degeneration of medical education which followed rapidly and successfully pushed anatomy into an inferior position. This decay in medical instruction reached in America as low a level as the civilized world has perhaps ever seen about the time of our civil war. At this period the chair of anatomy was almost always used as a stepping-stone to that of surgery and under these conditions the quality of the teaching was rarely good. A certain type of surgical anatomy developed from this combination and but very few contributions to the science were made. Too often, however, there was a mere exploitation of the chair of anatomy, the teaching was poor, and the practical work in the dissecting room was neglected. Since then there has been a gradual improvement in medical education, due largely to the cultivation of its underlying sciences. During all those dark years, however, there was one place in which the light of anatomy shone continuously; thither Caspar Wistar carried it and there the Wistar Institute is located.

Caspar Wistar, the second professor of anatomy at the University of Pennsylvania, did much to perpetuate the good traditions of Benjamin Franklin, John Morgan and William Shippen. He was a man of great influence, took a lively interest in natural history, and made many anatomical specimens, some of which may still be seen in the Wistar Institute. He wrote an excellent text-book of anatomy, which was used by many students. He was succeeded first by Physick and then by Horner, who made a number of important discoveries in anatomy. Horner was followed by Dr. Leidy, who held the chair for nearly forty years. During all these years the chair of anatomy became notably conspicuous on account of the luster shed upon it by the eminence of its occupants. The greatest of this brilliant group was Leidy, in fact he was the greatest teacher of anatomy to medical students this country has seen. His ideals were of the highest and his scientific discoveries were numerous and accurate, contributing much to comparative anatomy and zoology. The good influence he exerted upon the various institutions in Philadelphia has been extended over the nation through this association, of which he was one of the founders. It is also fitting, and by no means accidental, that the Wistar Institute is located in Philadelphia. Few, perhaps, are aware of what has really taken place at this great foundation for scientific anatomy. Thanks largely to the far-seeing policy of its present director, Dr. Greenman, the first division of its staff has been manned by the ablest investigators in neurology, and the Institute has been made the central institution for America by the Commission for Brain Investigation appointed by the International Association of Academies. I sincerely hope that our society will give full support to the Wistar Institute, for its work will be national as well as international.

On account of the low ebb of medical education at the period mentioned a certain dividing of the ways occurred, best illustrated, perhaps, by what happened at Harvard. At this college there fortunately appeared the ablest anatomist this country has yet produced, but the Medical School saw fit to duplicate his chair for reasons that are not clear to me. There existed in Harvard College and Harvard Medical School, side by side, Jeffrys Wyman, the scientific anatomist, with but few students, and Oliver Wendell Holmes, the poet anatomist, with many of them. This unwise arrangement, it seems to me, delayed the revival of sound medical education in this country for a number of years. However, the presence of Jeffrys Wyman in the Hersey chair of anatomy was of the greatest significance in founding the American school of zoology. Wyman graduated from Harvard Medical School in 1837, and after having been Warren's demonstrator of anatomy, succeeded him in 1847. During the following quarter of a century he made numerous important discoveries in comparative anatomy and embryology and contributed also to teratology and ethnology. The loss of the influence of this great philosopher and teacher upon medical students has been one of the misfortunes that medicine in this country has sustained. But it was in zoology in America that scientific anatomy was temporarily preserved and extended rather than in the departments of anatomy in the medical schools. The American anatomists should emulate Jeffrys Wyman and our first president, Joseph Leidy.

Under the conditions which prevailed it was quite natural that the better work of Europe—the work of anatomists like Blumenbach, Ernst Heinrich Weber, Meckel, Johannes Müller, Schwann and Kölliker—barely reached this country, for the little anatomy that was cultivated subserved the surgical art. This arrangement may possibly have been beneficial as a training school for surgeons, but it was so bad for anatomy that as a science and as a profession it gradually fell into disrepute among most of the people. This conception of anatomy as a mere maid-servant of surgery is still entertained by some of our colleagues in other sciences. In nearly all of the medical schools anatomy settled down to a dead level, the so-called 'practical,' and, during the second half of the past century most of the progress made in Europe found its way to America not by way of American anatomists, but through our zoologists, pathologists and physiologists. Fortunately, many of the latter have kept their membership in this society, for while this association consists of anatomists, a perusal of the list of members shows that some are also distinguished as physicians, surgeons, physiologists, pathologists, zoologists, anthropologists or psychologists. This I consider to be a fortunate circumstance, for it will prove to be a most potent factor in the reorganization and development of anatomy in this country and in its consequent broadening influence upon medical education. However, the catholicism in our society is not properly appreciated by educated people in America, for we often hear it said that our more prominent members are not anatomists, but biologists or something else. Let me illustrate: probably the most typical anatomist of us all, a man of the widest culture and a profound scholar, a scientist known as an anatomist the world over, a member of our executive committee for eight years, a founder of the *American Journal of Anatomy* and my predecessor in office, has been wrested from our ranks and called a zoologist by a recent writer in his study of American scientists. That a single writer should do this would be of no special importance, were it not that this view of the scope of anatomy is entertained by so many Americans of prominence that it interferes very much with the development of our science as a profession. Those who hold this narrow and perverted view of anatomy can not be familiar with its history, its present status in Europe, nor its recent development in America. It is the duty of the members of this association to correct this erroneous conception of anatomy by precept and by example. I have full confidence that this can be done with ease.

That anatomy played so important a rôle in the development of our school of zoology (was absorbed in it, some will say) while it was fossilizing in medical schools can be viewed as a fortunate condition from a number of standpoints. For us it hastened the destruction of certain traditions, which can now be ignored while we are constructing a new anatomy and establishing a new *modus vivendi* with the medical disciplines. With this change we are placing ourselves in a new and a better position than ever before. While anatomy is well represented in college and university departments not connected with medical schools, we must look for the highest development to anatomy in connection with medical education. In order to be more effective in the training of scientific physicians, we are gradually making our anatomical instruction more and more inductive and this naturally reacts upon the instructor in a beneficial way.

To bring about the desired reform it is necessary to have represented in an anatomical department, even in a medical school, all which naturally belongs to this science. The study of anatomy begins with the cell, ends with the entire individual, and includes man. In fact the greatest anatomical problems almost always involve a consideration of human anatomy. The teachers and students in an anatomical department should be given a free hand; they should not be retarded by arbitrary lines; they should dissect sometimes with the scalpel, sometimes with the microtome knife; they may look through spectacles or through the microscope and they may study the arm of a human embryo or the negro brain. In other parts of the world this liberty is a self-evident necessity and has always been granted. It follows from what I have said that an anatomical department must include histology, histogenesis and embryology; in a medical school it must cover vertebrate anatomy in the fullest sense. In general, due to the influence of this society, an unrestricted anatomical department has found its way into nearly a dozen important universities during recent years. Among the universities in large centers those in this city are the only ones in which the scope of the anatomical departments is still limited, since here histology and embryology are not included. Our wandering society, meeting as it does, in different portions of the country, will be, I believe, a great force in helping to perfect and to extend anatomical departments.

This is not the time to enumerate the really good anatomical departments, nor those that have been markedly improved in recent years, but I must not fail to note the great advancement which has been made in our state universities, due to the enlightened policy of their presidents, who are of the opinion that a professor of anatomy should be a specialist ranking high in his profession. It is safe to say that those departments in which the staff is actively engaged in scientific research are contributing most to medical progress and are exerting the best influence upon medical students. Yet, American anatomical departments taken as a whole are rendering an unsatisfactory account of themselves, and it is eminently desirable that this should change. In our wanderings as an association during the last dozen years we have had good opportunities to witness the improvements and growth which have taken place from year to year in the better universities. During this period we have met at Columbia three times, and it is a pleasure to me to acknowledge to Professor Huntington the obligations of this society for the splendid example he sets before us.

It is stated in our constitution that "the purpose of this association shall be the advancement of anatomical science." I firmly believe that such advancement can be made through scientific investigation only, but we must provide a suitable atmosphere for our investigators. It seems to me that certain conditions, which are necessary to make a good atmosphere, are, as yet, lacking in many institutions. Probably the most serious defect in our anatomical departments is due to the appointment of men in active medical practise to the chairs of anatomy. Unlike professional anatomists, they rarely have the time to devote to teaching students, nor the requisite training to enable them to develop the department properly, and anatomy necessarily suffers. However, I regard it as fortunate that circumstances have placed us in a position from which there is no retreat. To carry on the campaign, now so well started, we must have many more productive anatomists. In order to obtain them and to make the efforts of our present investigators more effective, we must use all our influence to bring the greatest opportunities and the best men together. A highly cultured community naturally desires the ablest man. My earnest hope is that those in authority in various communities will recognize that our idea of the scope of anatomy is correct, and that they will seek productive anatomists, when vacancies occur, so that our grand science may be raised to the level it has always held in Europe.

FRANKLIN P. MALL

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SOME PHASES OF PREHISTORIC ARCHEOLOGY [1]

THE American field for anthropological research is so wide and so fertile that it not only monopolizes the attention of specialists at home, but also attracts to our shores numerous foreign investigators. For attestation of this fact, one has but to cite the fourteenth International Congress of Americanists held in the city of Quebec last September. The same congress had convened in the new world twice before, once in New York City and once in the City of Mexico, the remaining sessions having been held in various European cities.

When foreign savants take such a deep interest in our own problems it is fitting that we should reciprocate by at least an occasional survey of the foreign field. In looking over the list of vice-presidential addresses read before this section, I find that two such surveys have already been made.[2] The address of Professor E. S. Morse, entitled 'Man in the Tertiaries,' was a powerful argument in favor of the existence of man's ancestors in Tertiary times. Fifteen years later Dr. Thomas Wilson chose for his subject, 'The Beginnings of Prehistoric Anthropology.'[3] He not only had something to say about Tertiary man, but also covered the paleolithic and neolithic periods. In the more than seven years that have elapsed since Dr. Wilson's address was read, much progress has been made in the prehistoric archeology of Europe. This is especially true concerning our knowledge of the *eolithic question* and of *paleolithic art* in so far as it has to do with engravings and frescoes on certain cavern walls. In fact, eoliths and paleolithic mural decorations were not even mentioned by Dr. Wilson. He did refer, however, to Harrison's discoveries of 'paleoliths' on the Chalk Plateau of Kent, but confused these with the well-known river-drift implements.

THE EOLITHIC PERIOD

When Thomsen published his relative chronology for prehistoric times in 1836,

[1] Address of the vice-president and chairman of Section H—Anthropology—at the New York meeting of the American Association for the Advancement of Science.

[2] Vice-presidential address, *Proc. A. A. A. S.*, 1884, XXXIII., 579.

[3] Vice-presidential address, *Proc. A. A. A. S.*, 1899, XLVIII., 309.

the only stone age known was that which is now called the neolithic period. Boucher de Perthes's first discovery of paleoliths in the river drift of the valley of the Somme came just two years later. These river-drift implements, however, were not accepted until after (Sir) Joseph Prestwich's visit to Abbeville in 1859.

Is there a stone industry antedating the paleolithic? The answer depends in a measure upon the definition of the term. The Chellean *coup de poing* is quite generally looked upon as representing the oldest paleolithic industry. As to its position in the geological scale opinions differ. Piette and de Mortillet placed it in the Lower Quaternary. According to the more recent classifications of Rutot and Boule, the Chellean belongs to the Middle Quaternary. If the latter view is correct, then a pre-Chellean industry need not necessarily be of Tertiary age. The presence of artifacts in the Lower Quaternary should not be surprising even to the most sceptical. This is particularly true in view of the fact that the well-known almond-shaped implement represents an advanced stage in the art of chipping flint. While Professor M. Boule does not doubt that industrial remains may exist in the Lower Quaternary and even in the Tertiary, he denies that they have as yet been discovered. In his own words as a paleontologist he has a firm faith in the existence of Tertiary man, traces of whom he believes will some day be found. On the other hand, Dr. A. Rutot accepts as man's handiwork the rudely chipped specimens not only from the Lower Quaternary, but also from the Pliocene and Upper Miocene.

The first serious claim for the existence of a Tertiary industry was made by the Abbé Bourgeois in 1867. The subject at once attracted considerable attention; but after a lively discussion that lasted for five or six years it was relegated to the background. The specimens that Bourgeois found in the Upper Oligocene at Thenay are not at present accepted as artifacts.

Carlos Ribeiro's discovery of chipped flints in the Upper Miocene and Lower Pliocene at Otta and other localities near Lisbon was announced in 1871. An account of Delgado's researches at Otta was published in 1889. Professor Verworn,[4] who recently visited this locality, is of the opinion that the deposits there have been so disturbed as to make the age of the artifacts doubtful. They may be paleolithic and even neolithic.

The problem is simpler at the classic stations near Aurillac (Cantal). The best known of these are at Puy-Courny and Puy-Boudieu. Here the deposit in question is of Upper Miocene age, fossiliferous and undisturbed. It is covered by a bed of andesitic tufa that attains in places a thickness of from sixty to one hundred meters. There is no doubt as to the geological age of the chipped flints. As to the nature of the chipping, however, opinions differ.

When attention was called to the first specimens discovered by Rames in 1877, such well-known authorities as de Mortillet, Cartailhac, Chantre, de Quatrefages and Capellini declared that if these flints had been found in Quaternary deposits, no one would hesitate to regard them as artifacts. The Cantal industry has been carefully studied in more recent years by Capitan, Rutot, Courty, Klaatsch and Verworn, all of whom have decided in favor of its genuineness.

The revival of interest in a pre-paleolithic industry in England began when Mr. Benjamin Harrison, of Ightham, Kent, who had been collecting paleoliths from the

[4] Max Verworn, 'Archäolithische und paläolithische Reisestudien in Frankreich und Portugal,' *Zeit. für Ethnol.*, 1906, S. 611.

river drift of the neighborhood for years, extended his field of search in 1885 to include the summit of that portion of the chalk plateau which lies between the valley of the Darent on the west and that of the Medway on the east. Here at heights of from four hundred to seven hundred feet above the sea, he discovered flints supposed to have been chipped by the hand of man.

As Harrison's collection grew it was submitted to Sir Joseph Prestwich, whose country-seat was at Shoreham in the Darent Valley near by. Thirty years earlier Prestwich had confirmed the accuracy of Boucher de Perthes's discoveries in the valley of the Somme. Who could be better fitted than he to answer the questions as to the age of the specimens and of the southern drift in which they occur, as well as to the character of the chipping. According to Prestwich the rudely chipped flints are artifacts and are as old as the southern drift. They are both older than the northern drift or boulder clay, and hence preglacial. Rutot places them in the Middle Pliocene. The southern drift (with implements) was transported across the chalk escarpment and the chalk plain into the Thames Valley along lines independent of the present drainage; the patches that are now left on the highest points (eight hundred and sixty-four feet at Titsey hill west of the Darent Valley), marking what were then the valleys.

Mr. J. Allen Brown,[5] in discussing the specimens found on the North Downs by Harrison, was the first to propose the term 'eolithic,' now so much in evidence. Two years later, G. de Mortillet made use of the term in his 'Classification palethnologique,'[6] applying it to the Tertiary only. Dr. Rutot[7] does not limit the eolithic period to the Tertiary. In his classification, as previously stated, the early phases of the Quaternary are also eolithic, the well-known hache type (Chellean) not appearing until the second advance of the ice.

Eoliths are by no means confined to Kent. They have been found by Shrubsole in Berkshire; by Blackmore, Bullen and others near Salisbury, Wilts; at Dewlish in Dorset; also in Surrey, Hampshire, the southern part of Essex and Norfolk. Mr. Percival A. B. Martin has found eoliths at a number of places on the South Downs in the neighborhood of Eastbourne and Beachy Head.

Are eoliths artifacts? This is the fateful question. Their geological age is of no consequence if they are only natural forms and have never been used by man or his precursor. The first flakes to be utilized were in all probability natural forms. It is not likely that eolithic man knew how to obtain the raw material from the chalk. He depended on picking up from the drift flakes of approximately the shape and size needed. A sharp edge was utilized once, twice, or until it became dulled, and was then cast aside. If an angular piece did not admit of being comfortably grasped in the hand, the troublesome corners were removed. Such conclusions as these are forced upon one after careful examination of a series of the specimens in question. Would the same conclusions be so irresistible if these objects were merely nature's playthings? Many may even be grouped according to more or less definite patterns. Two of these deserve special mention, viz., the small crescent-shaped scrapers comparable to the spoke-shave, and the double scrapers with an intervening point between the two scraping edges. Sometimes two margins are worked, but on opposite sides. That is to say, after chipping one of the margins, instead of rotating the specimen

[5] *Jour. Anthr. Inst.*, March 8, 1892, XII., 93–94.

[7] *Bull. Soc. belge de géol., de paleon. et d'hydrol.*, Bruxelles, 1903, XVII., 425.

[6] *Bull. Soc. d'Anthr. de Paris*, 1894, p. 616.

until the adjacent margin comes into play, it was reversed.

Belgian archeologists were among the first contributors to our knowledge of a pre-Chellean industry. The discoveries by Neyrinckx in the railway cut at Mesvin, between Mons and Harmignies, date from 1868. M. Emile Delvaux later took up the work at Mesvin, where he succeeded in determining the presence of a rude industry antedating the paleolithic, to which he gave the name Mesvinian. During the past twenty years, Belgium's most indefatigable worker in the prehistoric field has been Dr. Rutot, his studies being confined chiefly to the Quaternary deposits.

The river valleys of Belgium are often marked by three terraces: the upper terrace, of Pliocene age, about ninety meters above the present water-level; the middle terrace, at an elevation of from twenty-five to sixty-five meters, and the lower terrace, a little above high-water-level, both of Quaternary age. The Quaternary may be divided into five series of deposits. Beginning with the oldest, these are: (1) Mosean, (2) Campinian, (3) Hesbayan, (4) Brabantian, (5) Flandrian. These deposits have been carefully examined by Rutot in quest of industrial remains.

With the exception of the Brabantian, which is above the eolithic zone, all five divisions of the Quaternary are represented in section in the exploitation Helin at Spiennes, near Mons, phosphate works now owned by the Société de Saint-Gobain. All of the Quaternary eolithic epochs are likewise represented here with the exception of the oldest, the Reutelian. Rutot found that the three separated industry-bearing Campinian layers each furnished one of the several elements composing the industry previously found elsewhere in disturbed Campinian deposits. In the lowest of the three, there were not only eoliths of Mesvinian age, but also rude implements roughly amygdaloid in shape, selected flint nodules only slightly chipped to a semblance of the hache type, or poniard. All the requirements of a transition industry between the Mesvinian (eolithic) and the Chellean (paleolithic) are therefore satisfied. The middle layer furnished examples of the classic *coup de poing;* and in the uppermost layer there were specimens of the hache type, carefully chipped on both sides until the margins presented almost a straight line as opposed to the zigzag margin of the Chellean implement—in other words, the so-called Acheulian industry of M. d'Ault du Mesnil. Rutot has proposed the name Strépyan for the industry of transition from the eolithic to the paleolithic because of the character and abundance of the specimens found at Strépy, on the right bank of the Haine, between Estinnes and Cronfestu.

Following Rutot's lead, many German investigators have taken up the search for a pre-paleolithic industry in northern Germany, particularly in the valleys of the Elbe and Spree and on the Island of Rügen. The chief contributors have been Professors H. Klaatsch, Eugene Bracht and Max Verworn and Drs. Hans Hahne, G. Schweinfurth,[8] Eduard Krause, *et al.*

For some years past, the spread of the eolithic propaganda has been so rapid as to cause dismay in the camp of its opponents. I spent the summer of 1903 in England and Belgium for the express purpose of studying the question at closer range. That summer's work formed the basis for a preliminary report[9] read at the St. Louis meeting of the American Association at the close of the same year, as well as for a more extended paper[10] pub-

[8] Schweinfurth's studies have been confined chiefly to Egypt.

[9] SCIENCE, 1904, p. 449.

[10] 'The Eolithic Problem—Evidences of a Rude Industry Antedating the Paleolithic,' *Amer. Anthropol.*, N. S., VII., 425–479.

lished in 1905. Before the latter was out of press there appeared an article by Professor Boule,[11] intended as a severe blow to the genuineness of eoliths. He had been trying for twenty years to stem the rising tide in favor of a pre-paleolithic industry and was beginning to think of instituting experiments in the hope of throwing light on the origin of eoliths, when M. A. Laville, preparator at the École des Mines, Paris, found an experiment station already in working order and turning out 'eoliths' daily by the hundreds. It was a cement factory on the left bank of the Seine, two kilometers southeast of Mantes, near Paris.

In extracting the chalk from the quarry most of the flint nodules are cast aside. Some, however, pass unnoticed by the workmen and are carried with the chalk to the factory. This, together with a certain amount of clay, is emptied into circular basins (délayeurs) or diluters. These vats have a diameter of about five meters and a depth of 1.4 meters. The water is supplied by means of conduits and finally escapes through lateral sieves, carrying with it the mixture of chalk and clay, both highly pulverized.

Each circular vat is provided with a horizontal wheel, the spokes of which are armed with cast-iron teeth that reach to within two tenths meter of the bottom, the wheel itself being just above the surface of the mixture. This wheel, with a diameter of five meters and making sixteen revolutions a minute, attains a velocity at the circumference of about four meters a second.

In this whirlpool of moving water, chalk, clay and iron teeth, are also the flint nodules that escaped the notice of the quarrymen. These nodules, therefore, receive thousands of knocks, some mutual, some from the iron teeth, until at the end of a period of twenty-nine hours the machinery is stopped and they are removed. They are then washed and piled up to await their ultimate use as a by-product. It was in one of these piles that M. Laville's[12] discovery was made. Later he visited the place in company with MM. Boule, E. Cartailhac and H. Obermaier.

According to Boule, the flints that have passed through the machine have all the characters of the ancient river gravels. Most of them have become rounded pebbles. Many, however, are chipped in a manner to resemble a true artifact. He and his companions were able in a few minutes to make a 'superb collection, including the most characteristic forms of eoliths, hammer-stones, scrapers, spoke-shaves,' etc. His article is illustrated by half-tone figures, which, however, are of very little use to the reader. Photographs of eoliths are practically useless; faithfully executed line drawings are little better; a view of the objects themselves is absolutely necessary before passing judgment on their origin.

Professor Boule does not pretend that all eoliths have a natural origin more or less analogous to those made by machinery. He does claim 'that it is often impossible to distinguish between intentional rudimentary chipping and that due to natural causes.' In his opinion, the artificial dynamics of the cement factory are comparable in every respect to the dynamic action of a natural torrent.

Nothing is really gained even by proving the impossibility of distinguishing between man's work and chipping due to natural causes. If the argument is worth anything it will admit of a still wider application because of the fact that it is admittedly impossible to distinguish between certain true eoliths and some artifacts of the paleolithic and neolithic periods. Things that

[11] "L'Origine des éolithes,' *L'Anthropologie*, t. XVI.

[12] 'Feuille des jeunes naturalistes,' 1905, p. 119.

are equal to the same thing are equal to each other. By substituting, therefore, one arrives at the identity between stream-made and machine-made eoliths, on the one hand, and recognized artifacts, on the other. This does not prove the non-existence of true eoliths any more than it does that of the paleolithic or neolithic artifacts.

If streams at flood ever produced eoliths it is more than probable that they may still be doing so. While keeping one eye on the chalk-mill at Mantes might it not be well to keep the other on the Seine that flows near by? A few Seine-made eoliths would certainly be more convincing than those turned out at the factory.

It has not been my good fortune to see one of those cement factories at work. It is evident from the available literature on the subject that considerable prejudice has entered into the controversy. MM. Laville, Boule, *et al.*, were evidently seeking for what they claim to have found at Mantes. On the other hand, it was extremely unfortunate that certain believers in an eolithic industry were refused admittance into the Mantes establishment. A selected series, however, from Mantes, which was sent in 1905 to the Salzburg meeting of the German Anthropological Association by Herr Obermaier, was later placed at the disposal of Professor Verworn and Dr. Hahne. Verworn compared them with his collection of eoliths from Cantal, while Hahne compared the Mantes specimens with a similar series from a chalk-mill on the Island of Rügen, and the eoliths from Belgium sent to him by Rutot.

Dr. Rutot has sought to match his superb collection of eoliths in the Royal Museum of Natural History, Brussels, with specimens from the chalk-mills of Belgium, but in vain. Professor Verworn and Dr. Hahne have been no more successful in their comparative studies. All three agree in their general conclusions as to the radical differences between the true and the false eoliths; also that the action of the mill is hardly comparable with that of the natural streams of the regions in question except in one particular, viz., both tend in time to make pebbles of the flints that are offered to them.

According to Professor Verworn,[13] a fundamental difference exists between the eoliths he found at Puy-Boudieu and the pseudo-eoliths from Mantes. The corners and edges of the latter are worn, while those of the Cantal eoliths are not. It has been suggested that the chipping on the specimens from Puy-Boudieu may be due to pressure of the overlying beds. Such a result might be possible where unstable beds contained a sufficient quantity of flint nodules and chips pressing against each other. At Puy-Boudieu, however, the chipped flints are not resting against each other. They are separated by masses of tufa, loam and sand.

After a careful comparison of machine-made eoliths from both Mantes and Sassnitz with the Mafflean and Mesvinian industry from Belgium, Dr. Hahne's[14] conclusions are as follows: (1) The chalk-mill flints are all scratched and otherwise marked by the iron teeth of the mill. (2) The sides of all the larger pieces are bedecked with scars from blows that were not properly placed to remove a flake. (3) Almost every piece shows more or less of the original chalky crust of the nodule. (4) Anything like a systematic chipping of an edge or margin is never found except for very short stretches where one would expect it to be carried along the entire margin. This is quite different from the long retouched margins of most eoliths. (5) The same edge is often rechipped first on one side and then on the other absolutely without

[13] *Op. cit.*, p. 620.

[14] 'Über die Beziehungen der Kreidemühlen zur Eolithenfrage,' *Zeit, für Ethnol.*, 1905, S. 1024.

meaning or purpose. The 'reverse working' of true eoliths is quite another thing. (6) In the mill product coarse chipping alternates with fine retouches along the same margin, while on the eolith there is a regularity and orderly sequence of chipping. (7) The repeated rechipping of the same edge, while others are left untouched, does not occur in machine-made eoliths. (8) The chief difference is between the haphazard and meaningless, on the one hand, and the purposeful, on the other.

The most prominent and easily breakable parts suffer most in passing through the mill. They are often retained intact, or only slightly altered, on the eolith to serve as a hand-hold, and there is a logical relationship between the worked and unworked portions.

The eolithic problem in northern Germany is even more difficult of solution than that of chalk-mill 'eoliths.' Dr. Klaatsch, who had previously made a study of eoliths in France and Belgium, was among the first to find so-called eoliths in fluvio-glacial deposits in the valley of the Spree. His discoveries were supplemented by Dr. Hahne's in the valley of the Elbe. At this latitude, the deposits of the first glacial and first interglacial period, containing what appears to be an eolithic as well as a transition (Strépyan) industry, were very much disturbed by the second advance of the ice.

The result is that the pieces in question are so badly damaged as to obscure the evidence bearing on their genuineness. The German geologists and anthropologists are divided as to the proper interpretation to place upon these specimens. After going over the material with Dr. Hahne, Rutot is of the opinion that after all doubtful pieces are rejected there will remain enough to establish the existence of an eolithic industry in those regions.

Another subject studied in common by Rutot[15] and Hahne is that of shore-made eoliths, the locality being a favored stretch of coast on the island of Rügen in the Baltic Sea. The chalk cliff is surmounted by a moraine with large erratic blocks. When the seas run high, the large blocks and glacial till are thrown forward over the cliff. Masses of the fissured chalk are also loosened and fall to the foot of the cliff. With recurring high seas, broken nodules of flint come in contact with the erratic blocks and the production of pseudo-eoliths begins. If left, however, to their own fate they are finally reduced to sand. When rescued at the proper time, they resemble more or less the true eolith. They certainly form a more convincing argument in favor of the natural origin of all eoliths than do those from the chalk-mills. But they resemble the latter more than they do the genuine eolith, which, according to Rutot and Hahne, is still unaccounted for unless it represents the handiwork of man or his precursor.

The differences are not great enough to be detected by the untrained eye. They may be compared to the differences between hand-made and machine-made music. The untrained ear might not detect them without seeing the operator at work, but no such substitution could deceive an expert. It would be rather wide of the mark to conclude that, because pianos may be played by a pianola, they were never played by hand. Or if ever played by hand the result must necessarily be identical with that produced by the pianola.

The wide differences of opinion in the opposing camps can hardly be due to prejudice alone. Faulty or insufficient observation and incorrect interpretation doubtless play their part. Luckily, there is no disposition to drop the matter until the truth appears. At the International Congress of Anthropology and Prehistoric Archeol-

[15] 'Eolithes et pseudo-eolithes,' *Mém. de la Soc. d'anthrop. de Bruxelles*, 1906, t. XXV.

ogy held at Monaco, April 15 to 22, 1906, the chief subject of the second session was the pedigree of the eolith. According to *Nature*,[16] "a series of mill-modeled flint nodules was exhibited, among which there were certainly a number closely resembling many Prestwichian types, but conspicuous by their absence were the decidedly purposeful and rationally usable Kentian forms." On the other hand, Professor E. Ray Lankester "submitted that he had recently placed on exhibition in the British Museum a considerable series[17] of specimens selected from Prestwich's collection, all borer-like in form, too identical in shape and so rationally of obvious utility for any possibility of their being the result of fortuitous natural collisions."

As a further indication of the importance attaching to a correct solution of the problem and indirectly in recognition of the value of Rutot's contribution toward such a solution, the meeting of the German Anthropological Association for 1907 will be held in Cologne[18] in order that the members may visit the eolithic stations of Belgium and see the collections of the Brussels Museum.

THE ART OF THE CAVE-DWELLER

Passing now from Tertiary and Lower Quaternary eoliths and leaving out of account for the time being the important industry of the Chellean and Mousterian epochs, we come upon a most interesting chapter in the history of paleolithic man—a chapter to which many attractive pages have been added during the past ten years. It concerns the art of the cave-dweller.

One of the earliest enlightened searchers for prehistoric man in caverns was the Rev. J. MacEnery, a Roman Catholic priest, stationed at Torquay on the southern coast of England. As early as 1825 he found, in Kent's Cavern, flint implements definitely associated with the remains of the mammoth, rhinoceros and other extinct animals.

Then came, in 1833, the discoveries of Schmerling in the caverns about Liége, Belgium; but these also did not receive the attention they deserved, owing to the then all-powerful influence of Cuvier.

Following the appearance of Darwin's 'Origin of Species' and the acceptance of the river-drift implements as artifacts, both of which events occurred in 1859, cavern explorations received a new impetus. In Europe alone hundreds of paleolithic caverns have already been explored. About one third of these are situated in France. Some of the well-known localities outside of France are the regions about Namur and Liége, Belgium; Liguria in Italy; Moravia in Austria; and Schaafhausen in Switzerland.

To the student of the cave-dweller period, southern France is perhaps the most fruitful field in all Europe. Of this area Les Eyzies and its environs in the valley of the Vézère, department of Dordogne, is one of the chief centers. It was in 1862 that M. J. Charnet found in a shallow cave at Les Eyzies the first flint implements and breccia, with bones of the reindeer and other animals. He communicated the facts to Professor Eduard Lartet, of Paris, and Mr. Henry Christy, of London, who visited the place in August, 1863, making explorations simultaneously at Les Eyzies, Gorge d'Enfer and Laugerie-Haute.

This led, in 1865, to the plans for a great publication to be called 'Reliquiæ Aquitanicæ.' In importance, as well as in authorship, this is an international volume. The specimens described belong to the Christy collection of the British Museum

[16] June 28, 1906, p. 211.

[17] *Amer. Anthropol.* (N. S.), 1905, VII., 432, 433.

[18] It is proposed to make the Cologne Congress international. American anthropologists have been invited to take part.

and to the Museum of National Antiquities at Saint-Germain. The Lartet and Christy explorations were practically confined to the rock shelters and shallow caves. If these men had lived longer, they might have discovered the extensive subterranean caverns of the neighborhood, the walls of which are decorated with a remarkable series of frescoes and engravings.

The valley of the Vézère has been an attractive field for archeological excursions ever since the appearance of Lartet and Christy's classic work; and has come even more into favor since 1895, when the first discovery was made of engravings and frescoes on the walls of one of the caverns. Several other caverns have been explored during the past six years with similar results. In company with a small party of Frenchmen from Paris, members of the Société des Excursions Scientifiques, I visited the region during the summer of 1903.

Vézère collections have found their way practically into all the important museums of the world, but the British Museum (Bloomsbury), the Natural History Museum, Paris, and the Museum of National Antiquities at Saint-Germain-en-Laye contain the major part. These should all be visited before, as well as after, a trip to the Dordogne. The train can be taken direct from Paris (Gare d'Orléans) to Perigueux, the capital of the department of Dordogne, the site of ancient Vesuna of the Petrocorii and later a flourishing Roman town. Here one may stop with profit to see the ruins of a Roman amphitheater and tower, also the Musée de Périgord, rich in prehistoric relics of Dordogne, including the Vézère region.

From Perigueux it is less than two hours by train to Les Eyzies, the heart of the cave-dweller country, where one stops at the Auberge Berthoumeyrou, well and favorably known to a long line of pilgrims to this enchanted land of limpid streams, green valleys and lofty, picturesque escarpments.

The calcareous formation, cleft by the Vézère and its tributaries, is composed of Cretaceous beds approximately horizontal and of varying degrees of hardness; so that overhanging rocks often shelter horizontal galleries and niches. Again subterranean streams have left meandering caverns, some of them several hundred meters in length. These as well as the rock-shelters and open, shallow caves, formed through atmospheric agencies, were inhabited by early man. Some were enlarged or modified and occupied during the middle ages. At a safe height in the *roc de Tayac,* one such that withstood successive sieges in the fourteenth and fifteenth centuries is at present used as a restaurant and appropriately named 'au Paradis.'

The earlier explorations at Les Eyzies, Cro-Magnon, Gorge-d'Enfer, Laugerie-Basse, Laugerie-Haute, La Madeleine and Le Moustier are so well known that they are mentioned only in passing. After so long a series of important discoveries, it might well be supposed that the archeological possibilities of the region had been exhausted, yet some of the most important treasures still remained locked in the recesses of the less easily accessible and little known subterranean caverns which penetrate the hills to great depths. The entrances to these caverns are small and invisible from the valley below. Some, indeed, were completely stopped by hillside débris, leaving no outer trace of their existence. It is not strange that they escaped immediate notice. They were neglected until the early nineties, when Rivière removed some of the floor deposits in the cavern of Combarelles that yielded many flint implements, and especially fine bone needles. In 1895 he began work in similar deposits in the cavern of La Mouthe. One day, after penetrating to a considerable depth, he and his companion, the son of Berthoumeyrou, the innkeeper, sat down to rest. In lighting a cigar, the extra light of the match added to the feeble candle light and placed at the proper angle revealed to one of them what had not been observed before—an engraving on the wall. The discovery was duly announced and marked the beginning of a new epoch in cavern explorations. Not that Rivière's discovery was the first of its kind; but that the two previous finds of a similar nature had not been accepted as authentic. These were in the cavern of Altamira, Province of Santander, Spain, explored in 1879 by Sautuola,[19] and of Chabot (Gard), explored prior to 1889 by Léopold Chiron. Then followed Rivière's discovery at La Mouthe and that of Daleau in 1896 in the cavern of Pair-non-Pair (Gironde).

The mural decorations at La Mouthe occur in four groups or panels. The first panel is about ninety-three meters from the entrance. The second, four meters farther on, is called the 'Hall of the Bison.' Seven animals are represented on an area 5.02 m. by 2.6 m. The third and fourth panels are one hundred and thirteen and one hundred and thirty meters, respectively, from the entrance.

In 1899, Rivière was so fortunate as to find a stone lamp in the floor deposits of this cavern at a point about seventeen meters from the entrance. The pick of the workman broke the lamp into four pieces, of which three were immediately recovered. Rivière and two of his men searched for the missing fragment an entire day, but without success. The shallow bowl contained some carbonized matter, an analysis of which led M. Berthelot, the chemist, to conclude that lard was used for lighting purposes. On the base, there is an engraving of a wild goat's head and horns. A figure exactly like this was found on the third mural panel already mentioned. This was the fourth lamp to be found in French caverns. The first and second were from the cavern of Monthier (Charente), and the third from the cavern of Coual (Lot). The necessities of men dwelling in dark caverns would be likely to lead to the invention of artificial light, which light made it possible for them to depict the frescoes and engravings on the walls of their abodes.

The past six years have witnessed a succession of remarkable discoveries by MM. Capitan, Breuil, Bourrinet and Peyrony, in the caverns of Combarelles, Font-de-Gaume, Bernifal and others.

The Combarelles cavern has a total length of two hundred and thirty-four meters, is from one to two meters wide, and high enough to admit of walking upright for most of the way. The engravings begin at a point about one hundred and eighteen meters from the entrance, and occupy both walls for a distance of one hundred meters. Some of the figures are deeply incised; others are mere scratches. In some, the effect is heightened by the application of a dark coloring matter (oxide of manganese). Portions of the walls are covered by a coating of stalactite thick enough in places completely to hide engravings; while in others the more deeply incised figures are still visible. On areas devoid of incrustations, the figures are fresh and distinct. The artist sometimes had recourse to champlevé; sometimes natural prominences were utilized to add relief to the figures. Of the one hundred and nine engravings of various animals on the walls at Combarelles there are some forty equine figures, occurring either singly or in groups, and fourteen of the mammoth. The mural engravings belong precisely to the same school of art as the relief and incised figures

[19] Don M. S. de Sautuola, 'Breves apuntes sobre algunos objetos prehistoricos de la provincia de Santander,' 1880, Libreria Murillo, Madrid.

from the floor deposits of the shallow caves and rock shelters, so well known through the works of the earlier investigators. This statement applies equally to all the caverns thus far explored.

The cavern of Bernifal was first explored in 1903. It was discovered by accident. The original entrance near the base of an escarpment is completely obstructed by earth and stones. The present artificial entrance is at a point where the ceiling of the cavern comes close to the surface of the wooded, sloping upland. The descent into the cavern is almost vertical, and made by means of an iron ladder about three meters long. There is a joint in the ladder, the upper portion of which may be inclined and locked so as to secure the interior against vandalism.[20] Within are three large chambers united by rather narrow corridors. The first is twenty-two meters long, with high ceiling and a maximum breadth of eight meters. The others are not quite so large. The beautiful stalactites overhead have been left undisturbed. Most of the engravings are to be found in the second chamber. They are cut rather deeply into the calcareous walls, and generally coated over with a thin, hard layer of stalactite. Twelve groups, numbering in all twenty-six figures, have been recognized. These include geometric, triangular signs, in addition to various animal figures—reindeer, mammoth, horse, bison and antelope. Some are simply engraved, others are painted with red ochre and manganese. Many are probably wholly hidden beneath thick mural incrustations. Tectiform signs, the significance of which is unknown, were also met with at Combarelles and Font-de-Gaume.

The Font-de-Gaume frescoes and engravings were discovered in 1901 by Capitan and Breuil with the assistance of M. Peyrony, the school principal of Lez Eyzies. The entrance is some twenty meters above the valley and near the top of the escarpment.

A passage about sixty-five meters long, and much restricted in places, leads to an ample gallery forty meters in length, two to three in breadth and five to six in height. A majority of the paintings, and Font-de-Gaume is especially rich in paintings, occur on the walls of this gallery and in a little side chamber farther on. The latter contains thirteen remarkable figures, in color, of the bison and a group of reindeer. The coloring matter was red ochre and manganese, either mixed so as to give various intermediate shades or used separately. Both these materials are found on top of the neighboring plateaus. The dimensions of the figures vary from 2.70 m. down to .20 m. Some are on regular surfaces, while others include natural prominences in such a way as to give the effect of relief. They are veritable frescoes, the whole figure often being covered with paint. Engraving and fresco are usually associated in the same figure. The coloring matter was, in some cases, applied after the engraving; while in others the process was reversed. Again some figures are a piece-work of engraving and fresco. Some are engraved only. In certain cases the outlines of the animal are simply traced by a single stroke of the brush or pencil, usually in black. Where the contours are filled in, various tints from black to red are usually employed. The outlines are seldom marred by blotches or evidences of an uncertain stroke.

Of the more than eighty figures described already from Font-de-Gaume, forty-nine represent the bison, four the reindeer, four the horse, three the antelope, two the mammoth, one the stag, one *Felis leo*, one *Rhi-*

[20] Most of the prehistoric monuments of France are now the property of the government and are protected by the enactment and enforcement of wise laws.

noceros tichorhinus, six various signs. A number have not yet been determined.

In their various explorations MM. Capitan, Breuil and Peyrony have collected about a hundred drawings of the mammoth. Those of the bison, horse and reindeer are also numerous. On the other hand, representations of *Ursus, Felis* and *Rhinoceros* are rare. The engraving of *Ursus spelœus* on a piece of schist found in the floor deposits of the cavern of Massat (Ariège) has been known since 1867. A similar figure is to be seen on the cavern walls of Combarelles. An engraving of *Felis* on a pebble from the cavern of Gourden (Haute-Garonne) was recently published by Piette. Two mural engravings of *Felis* are known; one at Combarelles and the other at Font-de-Gaume. In the latter the entire animal is represented, being characterized by the form of the head, the general aspect of the body, the long, lifted tail and short paws. The animal is probably *Felis leo,* var. *spelœa,* since it is figured somewhat larger than are the four horses forming part of the same group or picture.

One of the most interesting animal representations on the cavern walls of Dordogne is a color drawing of *Rhinoceros tichorhinus,* found at Font-de-Gaume near the group that included an engraving of the cave lion. The figure is not only complete, but also exact. The two horns are faithfully indicated, the anterior notably longer and larger than the posterior. The only other representation of the woolly rhinoceros is an indifferent engraving on a piece of stone found in the cavern of Gourdan and recently published by Piette.

The cavern of Font-de-Gaume opens on a narrow valley tributary to that of the Beune and near their junction. The well-known rock shelter of Les Eyzies lies across the valley of the Beune. It is visible from Font-de-Gaume, appearing like a black spot on the face of the great escarpment, and only eight hundred meters distant. M. Peyrony[21] suggests that the two prehistoric communities may have been closely united. His recent researches at Les Eyzies tend to confirm this view.

The shallow cave of Les Eyzies, overlooking the Beune near its junction with the Vézère, opens on a sort of natural platform about thirty-five meters above the bed of the stream. The opening of the cave is wide and high enough to admit the light to its greatest depth, which is twelve meters. The geatest width is sixteen meters. It has a southern exposure; is dry and habitable. Font-de-Gaume was never a place of residence, as is indicated by the absence of floor deposits. About the only objects found there are a few broken gravers with edges dulled in executing the wall engravings, a few pieces of ochre and manganese and one handsome ochre pencil. Why should the artists make residence of a dark subterranean cavern, when by going a short distance they could have an ample shallow cave or rock shelter facing the south and warmed and lighted by the sun? Such a shelter is Les Eyzies, and the enormous quantities of refuse taken from its floor at various periods testify to its use as a place of habitation by generation after generation.

The rock shelter of Les Eyzies has furnished unusually large quantities of ochre of various tints. Most of the pieces have been scraped to produce a colored powder which was mixed with grease or some liquid, thus forming a paint. In order to pulverize and thoroughly mix the coloring matter, mortars were used. An interesting series of these mortars from Les Eyzies

[21] Le Dr. Capitan, l'Abbé Breuil et Peyrony, 'Nouvelles observations sur la grotte des Eyzies et ses relations avec celles de Font-de-Gaume,' *Compte rendu, Congrès préh. de France,* 1905, p. 137.

forms a part of the famous Christy collection in the British Museum. Very few mortars have been found in neighboring stations. Besides, ochre pencils exactly like the one from Font-de-Gaume have been found in the rock shelter of Les Eyzies. Sometimes a flat piece of ochre is cut in the form of a triangle, each angle serving in turn as a pencil point. Some of these pencils are perforated to be suspended, and might well be supposed to form a part of the outfit of the artists who drew in color figures such as that of the two-horned rhinoceros previously mentioned.

It may be that the artists who made their home at Les Eyzies decorated its walls also. Exposure would have obliterated these decorations long ago. Lucky it was for present-day lovers of art and archeology that their troglodyte forebears had the good sense to seek at Font-de-Gaume a more permanent gallery for their masterpieces.

In addition to the four caverns with wall engravings and paintings in the Vézère valley group, one other is now being explored in the Dordogne, viz., the cavern of La Mairie et Teyjat. This large cavern is only two hundred meters distant from the rock shelter of Mège, discovered in 1903 by M. Bourrinet. In the cavern of La Mairie the floor deposits may be separated into two industry-bearing layers. The upper one of these contains the same industry as the single layer in the adjacent rock shelter of Mège, except that the latter has furnished archeological material in greater quantities than were found in the deposits of the cavern. The relative positions of the engravings on the cavern walls and the upper layer of floor deposits prove that both belong to the same epoch (Magdalenian). It is also interesting to note that while bones of the reindeer abound in the rock shelter of Mège, representations of this animal predominate among the mural engravings in La Mairie cavern.

Besides the cavern of Chabot, at Aíguèze and of Pair-non-Pair, already mentioned, other decorated French caverns explored to date are: Le Figuier (Ardèche) across the river from Chabot, La Grèze and Marsoulas (Haute-Garonne).

Of caverns with paleolithic mural decorations outside of France, thus far reported, one is in Italy and four are in Spain. The most important cavern in the Spanish group is that of Altamira in the north coast province of Santander, previously mentioned as being the one in which the discovery of mural figures first took place. The genuineness of these figures would have continued to remain in doubt had it not been for similar subsequent discoveries elsewhere.

M. Émile Cartailhac and the Abbé H. Breuil have recently studied with great care the wall paintings and engravings at Altamira. The cavern is a series of large chambers connected by passageways. There is no evidence of its having been occupied by either man or beast since the close of the Quaternary, at which time the entrance was completely closed by a fall of earth and stones.

A second, recent fall has afforded a new opening to the cavern, reached by clambering over the débris that closed the original entrance. The first chamber is divided by means of a mass of fallen stones. The one on the left is forty meters long by twenty meters wide. The one on the right is a sort of corridor connecting with other chambers. Industrial remains of the floor deposits are confined to the entry and the chamber on the left. There is evidence that the cave bear had occupied the cavern before man took possession. Figures, engraved or painted, are found on the walls of every part of the cavern, especially on the ceiling of the chamber on the left near the entrance, where the frescoes are remarkable for their beauty, size and good preservation—a sort of Sistine chapel representing the *chef d'œuvre* of perhaps more than one Michael Angelo of that far-off time.

These works of art represent a variety of technique. Some are simple line engravings. Others are more deeply incised. But the engravings are not so numerous as the figures represented in color. Many are done in a single color, either red or black. The most remarkable are the polychrome frescoes similar to those of Font-de-Gaume, already described.

The figures are not all animal representations. Many are signs, the significance of which is not known. They do not belong to a single epoch. The superposition of figures, each in a different technique, studied in connection with the relative state of preservation of the various figures has furnished a key to the order of succession. The same succession is traceable in the caverns of France, so that the Abbé Breuil and his colleagues: MM. Cartailhac, Capitan, Peyrony and Bourrinet, have been able to distinguish four distinct phases[22] in the evolution of mural painting and engraving, all of them being represented in the cavern of Altamira.

The *first phase* includes deeply incised figures representing the animal in absolute profile, *i. e.*, with a single forefoot and a single hind foot, the outlines being rude and not well proportioned, and details, such as hoofs and hair, not indicated. The figure of a bison in the corridor on the right is a specimen of this oldest class of wall decoration. Other examples are found at La Grèze, Chabot and Pair-non-Pair.

The paintings of this stage are also in outline, the color being black or red and drawn with a crayon, there being absolutely no effort at modeling. The horse drawn in black on the ceiling of the left chamber is an illustration. Others may be seen at Marsoulas, Font-de-Gaume, La Mouthe, Combarelles and Bernifal.

The incised figures of the *second phase* remain deep and broad; but the outlines are more lifelike although often ill-proportioned. All four legs are often represented, the distal ones being almost completely hidden by those nearest the beholder. The hoofs are sometimes represented with great care. As the incisions become less deep they also gain in neatness. In places the effect of bas-relief is given by means of champlevé. The more hairy portions are indicated by incised lines. Engravings of this stage are not numerous at Altamira. They are seen to better advantage at La Mouthe, Font-de-Gaume, Bernifal and above all at Combarelles.

The paintings of this phase evince the first attempts at modeling by shading at various points. Engraving is often combined with the painting. The use of color continues to develop until one arrives at a monochrome silhouette usually in black. The contours are often heightened by engraving. The second phase is represented not only at Altamira, but also at Marsoulas, Combarelles, Font-de-Gaume and La Mouthe.

The engravings of the *third phase* are generally of small dimensions. Many of these are admirable in their execution, as, for example, the bison in the terminal corridor. The entire mural decorations in the cavern of La Mairie at Teyjat are in this style, as are a number from Font-de-Gaume and Marsoulas.

In the domain of painting, the third phase is represented by an excessive use of color, producing a flat effect, thus destroy-

[22] A fifth and closing phase is discernible at Marsoulas, resembling somewhat the work on the painted pebbles of Mas d'Azil.

ing the modeling that was such an attractive feature of the preceding stage. At Altamira the color employed is red and the drawing is deplorable. As a rule these examples are not well preserved. Those from Marsoulas, in either black or red, are not much better. The best work of this phase is to be seen at Font-de-Gaume and is executed in black or brown. It is often combined with engraving of a high order, done before the color was applied.

In the *fourth phase* the engravings lose their importance. The lines are broken and difficult to follow. The small figures of the mammoth at Font-de-Gaume and of the bison at Marsoulas show this tendency to emphasize detail at the expense of the ensemble.

Paleolithic painting reached its zenith in the fourth phase. The outlines are drawn in black, as are the eyes, horns, mane and hoofs. The modeling is done with various shades produced by the mixing of yellow, red and black. Engraving always accompanies the fresco, serving to emphasize the details. These polychrome figures are seen at their best on the ceiling of the left chamber near the entrance; also at Marsoulas and Font-de-Gaume.

Shortly before his death, M. P. Jamin, a well-known Parisian artist, exhibited in the Paris Salon of 1903 a large oil painting inspired by the discovery of these polychrome frescoes. This canvas also formed part of the French art exhibit at the Louisiana Purchase Exposition, St. Louis, in 1904. It represents the cave-dweller artist in the little side chamber of Font-de-Gaume at work on one of the thirteen figures of the bison while members of his family look on and applaud. In a panel above his head is the unfinished group of reindeer. It has long been the custom for artists to copy the old masters. M. Jamin has rendered a valuable service to both art and archeology by introducing the modern French school of painters to the earliest school of art developed on what is now French soil. The ages of Phidias and of the Italian Renaissance, viewed in the light of their antecedents, are wonderful manifestations; but not more wonderful than that of the Vézère troglodyte, a contemporary of the mammoth and rhinoceros, the bison and the reindeer.

GEORGE GRANT MACCURDY
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AMERICAN SOCIETY OF BIOLOGICAL CHEMISTS

FOR several years the biological chemists of this country have been considering the advisability of organizing a national biochemical society. The growth of the Society of Physiological Chemists (New York City), which was founded in 1899, the development of the biochemical section of the American Chemical Society, which was organized in 1905, the increasing number of chemical papers on the programs of the American Physiological Society, and the great success of the recently established *Journal of Biological Chemistry*, were among the influences that stimulated thoughts of a national organization of biochemical workers.

At the suggestion of Professor John J. Abel, a meeting for the purpose of effecting the establishment of such a society was held in New York City, at the headquarters of the American Association for the Advancement of Science (Hotel Belmont), on the afternoon of December 26, 1906. About seventy-five American biological chemists had been invited by Professor Abel to attend the meeting, but many were unable to go to New York at the time stated. There were few, however, who did not heartily favor the project. Of those who had been invited to attend the meeting the following were present:

John J. Abel, Carl L. Alsberg, Samuel Amberg, Silas P. Beebe, Russell H. Chittenden, Otto Folin, Nellis B. Foster, C. Stuart Gager, William J. Gies, Robert A. Hatcher, Reid Hunt, Holmes C. Jackson, Walter Jones, Waldemar Koch, Phoebus A. Levene, Arthur S. Loevenhart, John A. Mandel, John Marshall, Gustave M. Meyer, Thomas B. Osborne, Raymond H. Pond, Alfred N. Richards, Herbert M. Richards, William Salant, Philip A. Shaffer, Herbert E. Smith, Frank P. Underhill, George B. Wallace, Charles G. L. Wolf.

The meeting was formally addressed by Professor Abel, who, in urging the desirability and timeliness of immediate organization of biological chemists, made the following remarks:

"I take the liberty of rehearsing briefly the reasons for which this meeting has been called. * * * We have become convinced that there is need in this country for an organization which shall further the interests and foster the growth of biological chemistry. Biological chemists at present are affiliated with widely differing societies and come little in contact with the great body of men who are interested in biochemical work. Whether we as chemists have as our field of work the physiological chemistry of our medical schools or deal with the chemical problems of botany, zoology, pathology, pharmacology or medicine, we all have one common meeting-ground, and that is chemistry as applied to animal or vegetable structures, living or dead. As distinguished from the work of pure chemists, organic or inorganic, our efforts are directed towards throwing light on the life processes and functions of living structures, with the help of chemical and physico-chemical methods.

"Now, it will be granted, I think, that scattered and divided forces cannot develop that coordination of effort that is desirable when many workers have one great interest in common. In such a case, organization is beneficial. It encourages research, it furnishes the mechanism for competent criticism and helpful discussion; and lastly, the very fact that we have felt impelled to organize will make it evident to faculties of science and medicine and to scientific and medical societies that a great and growing department of research demands its fitting place in the general scheme of higher education.

"I come now to the question of an academic career in biological chemistry. You have probably all, at one time or another, been asked to recommend some young man for a teaching position in physiological chemistry. The authorities in question want a man who has had a first class training in organic, inorganic and physical chemistry and biology, has had some experience in teaching physiological chemistry, has an agreeable personality, is a fascinating lecturer, and a promising if not already fruitful investigator. For such a rare combination of natural endowment and acquired culture, there is offered a salary ranging from $800 to $1,500, the title of assistant or instructor, with guarded hints as to promotion at some uncertain date and still more non-committal statements as to a possible rise in salary.

"Biochemical research is quite the thing to-day. Every species of laboratory, clinical, bacteriological, hygienic, pathological, pharmacological, wants a chemist. All these laboratories no doubt afford fine opportunity to the young chemist for training in the broad field of biological chemistry. But what of his future? Is it as promising as it should be?

"This state of affairs is largely our own fault. We attend only the meetings of societies of other specialists for fear we shall lose something that lies on the border line between their territory and ours. These other specialists have their house in order, organization has done its invaluable service for them, and the result is that every worker knows his fellows, each knows where to turn for advice and sympathy; each member, no matter how remotely placed or how depressing his immediate environment, has the courage and enthusiasm in his work which comes from being connected with those who have the profound conviction that their branch is one of prime importance and dignity.

"I believe in special societies for specialists and I have no fear of the so-called narrowing influence of specialization. I feel rather that any possible danger in that direction is more than offset by the stimulus to go deeply into our subject which comes from association with those of like interests. Chemistry, the fundamental science that must always guide our work, offers unlimited opportunity for broadening the mind.

"It is my firm conviction that a national society of biological chemists should be organized at once. There are in this country, as near as I can ascertain, about one hundred active workers in this field, using the term in its widest sense. A very small minority of those with whom I have corresponded are undecided as to the wisdom of forming such a society, but are willing to accept the action of the majority. Some of these, again, have raised the question as to the advisability of asking the Physiological Society to give us a separate chemical section.

"Many of us have given careful thought to this proposition, but have decided that it will be best to have an independent organization. I have already outlined some of the advantages that would follow on organization, and I can only repeat that I believe these advantages would be greater if in name and fact the organization is independent. I believe that we can have a society on broader lines than is possible to a mere section. We wish to draw into our society the biological chemists of all departments of biology including those organic and physical chemists who take a lively interest in our subject, but who would perhaps not care to join a physiological society. In fact, since a large number of our proposed membership are primarily chemists rather than physiologists, we should be marching under a wrong banner, no matter how great the freedom granted by the parent society.

"This desire for, or prejudice, if you will, in favor of, entire independence in name and action, would equally forbid our organization as a section under the American Chemical Society. While recognizing that the various branches of science are mutually dependent and constantly receiving help from each other we still contend that special devotion in each individual branch alone insures success. In other words, we should stand for *independence* with *interdependence.*"

At the conclusion of Professor Abel's remarks, which were warmly applauded, general discussion ensued. Hearty approval of his proposal was evidenced, and it was agreed to organize at once the society that Professor Abel suggested. The undersigned thereupon proposed a few written articles of agreement on which a permanent organization could be based. These articles of agreement were adopted and will govern the society until the formal ratification of constitution.

On motion by Professor Abel, Professor Chittenden was elected president of the society. In accepting the presidency, Professor Chittenden expressed appreciation of the reasons which led to the establishment of the society, and declared his hearty interest in its future growth and success.

The following council and officers were elected: John J. Abel (*vice-president*), R. H. Chittenden (*president*), Otto Folin, William J. Gies (*secretary*), Walter Jones, Waldemar Koch, John Marshall, Lafayette B. Mendel (*treasurer*) and Thomas B. Osborne.

No plans have been made for a meeting before next December, although the council was authorized to use its discretion in this and all other matters affecting the welfare of the society.

WILLIAM J. GIES,
Secretary

SCIENTIFIC BOOKS

The Bird: Its Form and Function. By C. WILLIAM BEEBE, Curator of Birds, New York Zoological Park. New York, Henry Holt & Co. 1906. 8vo. Pp. 496, with over 370 illustrations.

This book is 'intended as an untechnical study of the bird in the abstract' and tells of the structure and characteristics of birds, dwelling especially upon the adaptations of the various organs to their uses, and their bearing on the relationships and past history of birds. It thus covers ground that has been but little worked, for while there are books a many on the anatomy of birds, these, with the exception of Headley's 'Structure and Life of Birds,' are purely descriptive and fail to show the relations of a bird's structure to its surroundings and mode of life. Here we are told why a beak, a foot, a wing, is of a given shape, what rôle it plays in a bird's daily life, or, if its present use is not obvious, what hint it gives of a bird's past history when the part now useless was all-important.

The first chapter, devoted to the ancestors of birds, of necessity recapitulates what is already known—what we do *not* know will fill volumes still to be written. We would, in passing, dissent from the statement that Archæopteryx 'frequently walked or ran on all fours,' and if Mr. Beebe will make a figure of the animal in such a position he will doubtless appreciate the difficulties in the way. Next is a long chapter on 'Feathers,' including their origin, structure, development, arrangement and moult, and this is followed by a discussion of 'The Framework of a Bird,' the skull being given a chapter by itself. Much information as to color and color changes will be found under the caption 'The Body of a Bird,' where some good illustrations are given of the effects of food, light and moisture, one of the most striking being the very dark form of the white-throated sparrow, produced by exposure to moisture-laden air through two moults. So, part by part, the bird is considered in detail, the final chapter treating of 'The Bird in the Egg.' Under 'The Eggs of Birds' we learn of the eggs themselves and of the information that may be gathered from them when studied in connection with the habits of the bird that laid them. For "That which adds the greatest interest to anything is the '*why*' of it, and a vast collection of eggs, beautiful though they are, yet if ignorantly looked at is worse than useless. Why one bird lays twenty eggs and another but two; why one bird's eggs are white, another's of varied colors, we will never learn from blown museum specimens." It has been denied that oology is a science, but whether it is or is not depends on the individual and it is to be hoped that this chapter may afford fruitful suggestions for future work on the part of our younger ornithologists. The chapters on Wings and Beaks and Bills are among the best because Mr. Beebe, who is a keen observer, has here combined the results of his experience in the field, and of the opportunities offered by having many species of birds continually under his observation in the New York Zoological Park. In regard to beaks we are told that "The finding and securing of food being the most important problem birds have to solve for themselves, it is for these purposes, and especially the last mentioned, that we find bills most adapted. This is so universally the case that we may often judge accurately of the kind of food of a certain bird from a glance at its beak."

It is impossible that a book of this nature, where much information is crowded into a small space, should be entirely free from errors, and here and there slips occur. Thus we are told that *Amphioxus* has biconcave vertebræ, after having been correctly informed that the backbone is represented by a mere thread of gristle; that the moa was found in

Madagascar; and that the femur of a bird is short *because* the knee is concealed. There is also an occasional tendency to overestimate the size of birds; for example, it is stated that the South American condor sometimes has a spread of wing of fifteen feet. As a matter of fact this bird is slightly exceeded by the California vulture, whose greatest recorded spread is ten feet six inches, and it is an unusually large condor that measures even nine feet from tip to tip. We *believe* that the albatross sometimes exceeds twelve feet across the wings and *may* reach fourteen, but never measured one greater than ten. However, the above are but minor blemishes, the book abounds in information and represents a large amount of original work.

The illustrations, mostly from photographs taken by the author, are numerous, and, with rare exceptions, good. One of the exceptions is that on page 85, showing the shoulder girdle of a pigeon, and is not only taken from a diseased specimen, but fails to show the parts described. Some particularly good pictures are to be found in 'Heads and Necks' and 'Wings,' the young green heron and his *vis-à-vis,* the great white heron, being most excellent.

All in all, this volume of The American Nature Series is admirable.

F. A. L.

Einleitung in die Chemische Krystallographie. By P. GROTH. Pp. v + 80, 6 figures, 8vo, cloth, 4 marks. Leipzig, Wilhelm Engelmann. 1904. English translation by HUGH MARSHALL, 12mo, cloth, $1.25. New York, John Wiley & Son. 1906.

Chemische Krystallographie. By P. GROTH. In four volumes. Vol. I., pp. viii + 634. 389 figures, 8vo, cloth, 20 marks. Leipzig, Wilhelm Engelmann. 1906.

For nearly a score of years Professor Paul von Groth, of the University of Munich, has had in preparation this 'Chemische Krystallographie' which aims to include in systematic order trustworthy data of all crystallized chemical substances.

The introduction to this monumental work appeared as a separate publication under the title 'Einleitung in die Chemische Krystallographie' in 1904, and has since been translated into English. In the 'Einleitung' the recognized relations existing between the properties of crystals and their chemical constitution are explained in the light of modern ideas of crystal structure. In so doing Professor von Groth assumes a knowledge of physical crystallography and chemistry. In order, such topics as crystal structure and its varieties, polymorphism, morphotropy, isomorphism, and molecular compounds are discussed. In the chapter on morphotropy much attention is given to a full discussion of the 'topical parameters,' first proposed by Muthmann and Becke, by means of which it is possible to compare to better advantage than was hitherto possible the crystal structure of different substances as well as to note the variation caused in their crystallization by changes in chemical composition.

The English translation by Hugh Marshall, of the University of Edinburgh, is in every respect admirable. A copy of this introduction ought to be in the hands of every chemist.

The 'Chemische Krystallographie' proper is to comprise four volumes as follows: Volume I., Elements, inorganic binary compounds, simple and complex haloids, cyanides, nitrites and their alkyl compounds of the metals; Volume II., inorganic oxy- and sulfo-salts and their alkyl compounds; Volumes III. and IV. will contain the organic compounds. Of these only the first volume has been published. The remaining volumes are to appear at intervals of one year.

The arrangement of Volume I., which is also to be followed in the others, is such that substances, which are similar chemically are treated together in separate groups or sections by first discussing our present knowledge of the same. These discussions present a very clear and concise survey of the literature, point out the conclusions to be drawn concerning the crystal structure of the substances under consideration, and in many instances indicate important lines of needed research. The second portion of each section is devoted to a systematic description of the members of that group for which crystallographic data were obtainable. These data are uniformly complete and usually accompanied by numerous figures. For example, 42 pages and 40 figures are devoted to the elements. In this chapter on elements there are no less than 237 references to literature given. The monohaloids are described in 40 pages containing 50 figures and 147 references, of which over two pages and six figures are given to ammonium chloride alone.

Chemists, crystallographers and mineralogists have long felt the need of a good reference work of this character and are greatly indebted to Professor von Groth for placing at their command in a clear and concise form such a vast amount of information concerning crystallized bodies. The appearance of the remaining three volumes will be awaited with much interest. EDWARD H. KRAUS

MINERALOGICAL LABORATORY,
UNIVERSITY OF MICHIGAN,
December 14, 1906

Principles of Botany. By JOSEPH Y. BERGEN, A.M., and BRADLEY M. DAVIS, Ph.D. Boston, Ginn & Company. 12mo. Pp. x + 555.

Ten years ago Mr. Bergen, then instructor in biology in the English High School of Boston, brought out an admirable little book entitled 'The Elements of Botany' designed to be a text-book for use in the high schools. It soon became deservedly popular and was very widely used. Five years later there appeared 'The Foundations of Botany,' a much larger book, in which the author, after revising the chapters of his earlier book, had injected a good deal of the new branch of botany—ecology—accompanied with a considerable number of half-tone and other illustrations of leaf-patterns and landscapes, in accordance with the ecological fashion of that day. In the book before us, we have a further modification of the author's idea of the kind of matter to be presented to the young beginner in botany in the high school, and perhaps the first course in college. In its preparation the author associated with him Dr. Davis, until recently of the University of Chicago, so that it appears under their joint authorship.

After a brief introduction, mainly devoted to a definition of botany and its subdivisions (morphology, physiology, plant geography, paleobotany, taxonomy, ecology and economic botany), we have the remainder of the book divided into three parts, viz., I., 'The Structure and Physiology of Seed Plants' (146 pages), II., 'The Morphology, Evolution and Classification of Plants' (257 pages), and III., 'Ecology and Economic Botany' (129 pages). Parts I. and III. are the work of the senior author, while Part II. is from the hand of Dr. Davis.

Part I. is a still further revision of the first dozen or so chapters of the 'Foundations.' The treatment is much briefer, and all 'experiments' are left out, so that instead of 227 pages in the 'Foundations' only 146 pages are given to this portion of the subject in the 'Principles.' Part II. is entirely new matter, and is an admirable presentation of the elements of systematic botany. Dr. Davis has shown his ability to present an outline of this vast subject in such manner as to give the student a clear picture of the whole. The only criticism of this part of the book is that it will probably be found to be quite too full, and perhaps too difficult for pupils in secondary schools, and better adapted to the capacity of college students. Part III. is based upon the second part ('Ecology') of the 'Foundations,' containing, however, much new ecological matter, which is well and clearly presented, and several chapters on economic botany which do not appear to be necessary in a book of this kind. One may seriously question the usefulness to beginning students of chapters including such topics as plant breeding, the production of hybrids, selection among corn, selection among wheat, results of hybridizing citrous fruits, and wheat, food products for human use, and for domestic animals, plant-fibers, timber, forestry and fuel. These subjects can not be adequately treated in an elementary text-book intended for children. The little that is said under each topic is not enough to serve as a beginning of the subject, and there is certainly neither space nor time for more. It has often been said that the most difficult task in the preparation of an elementary text-book is to make a judicious selection of the things to be included from the vast multitude of things which present themselves. To know what he may safely exclude, and yet make a connected story, which shall be brief enough to be mastered in the time at the student's disposal, is, we admit, not easy to accomplish. To 'touch the high points' and yet to keep up the connection between them is the difficult task of the writer of an elementary text-book. In some portions of the book before us this has been accomplished, while in others a good deal of matter has been admitted which might well have been left out.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Comparative Neurology and Psychology for January includes a paper 'On the Place of Origin and Method of Distribution of Taste Buds in *Ameiurus melas*,' by F. L. Landacre, a study of the embryology of the taste buds of the catfish. He shows that taste buds appear simultaneously in the entoderm of the gill arches and in the ectoderm of the lips. From both of these centers the buds spread backward, from the first into pharynx and œsophagus and from the second into the mucous membrane of the mouth and also into the outer skin, finally reaching the extreme dimensions of the outer surface of the body. No buds migrate from entoderm to the skin. The series of papers on the nervous mechanisms of touch and taste in fishes by C. J. Herrick is continued by 'A Study of the Vagal Lobes and Funicular Nuclei of the Brain of the Codfish.' Instructive comparisons are drawn between the central mechanism of this fish and Ameiurus and an attempt made to explain their difference on the basis of the mode of life of the fishes. There is also given a translation of the recent researches by Minkiewicz on 'Chromotropism and Phototropism.'

SOCIETIES AND ACADEMIES

THE AMERICAN PHILOSOPHICAL SOCIETY.

A STATED meeting was held on January 4, at 8 o'clock. Professor J. C. Branner communicated a paper on 'The Geology of the San Francisco Peninsula,' by Roderic Crandall.

DISCUSSION AND CORRESPONDENCE

THE 'FIRST SPECIES RULE' VS. THE 'LAW OF PRIORITY' IN DETERMINING TYPES OF GENERA

IN connection with the discussion on 'elimination' vs. 'first species,' in determining type species, may I be permitted to bring forward certain points which seem to me to be worthy of consideration?

That some authors are decidedly opposed to 'elimination,' while others are equally opposed to 'first species,' indicates rather strongly that there are valid objections to both methods, or at least that neither method is perfect. Whatever our views in the case may be, it is a matter of record that some authors have adopted the one method, while other authors have adopted the other.

If a given rule of nomenclature is to command the general respect of biologists and not to be subject to change from generation to generation, it should be sufficiently just, objectively, to appeal to all persons who are called upon to apply it and who may be temporarily inconvenienced by its application. The question, therefore, arises whether the 'first species' rule is so inherently just in principle that it will appeal to systematists to sufficiently convince them of the justice of overturning hundreds or possibly thousands of cases of type determination which have been made since 1758, and especially since 1842.

Personally, I view the first species rule as one of enormous convenience, and as one which can be applied, in the vast majority of cases, uniformly by all workers.

That it is necessary (however desirable it may be) to have a rule which will apply uniformly to all genera, is a point which I very seriously doubt. On the contrary, it seems to me that there is a certain amount of advantage in allowing a margin for the exercise of some discretion in certain cases. That two authors may arrive at different conclusions on the basis of elimination does not, therefore, seem to me to condemn it.

My friend Dr. Allen has given *Vultur* Linnæus, 1758, as an example of elimination. All authors may not agree with him in his method of elimination, but I believe I am justified in pointing out that both he and those authors who differ with him as to his method of elimination (for two methods are possible in this case) have overlooked the very important fact that they have ignored the rule which Linnæus himself laid down for the determination of types of his own genera. Now, since Linnæus did give a rule to be applied to his genera, it seems to me that it is obligatory to apply that rule to Linnæan genera, regardless of our views of first species or of elimination. The example upon which the discussion is based is, therefore, an invalid one from either Dr. Allen's or Mr. Stone's point of view.

A point to which attention may be directed is that, not only the elimination rule, but the first species rule also is interpreted differently by different men. Take the genus *Dispharagus* Dujardin, 1845, for instance. If an author who does not adopt the rule of type by inclusion were to determine a type for this genus, he might select either *D. decorus* or *D. laticeps* or *D. tenuis* as type, and yet he would be following the first species rule, as interpreted by three different sets of workers. It should, however, be mentioned that if he followed the rule as interpreted by the American Ornithological Union, this difference of opinion would not arise; but not all first-species men agree with the American Ornithological Union interpretation.

Still more important than the foregoing, is the fact that the first species rule would in many cases virtually misrepresent an original author's intention, in that if the type is determined on basis of his generic diagnosis and with a number of species from which to select, the nature of his genus becomes more or less clear and we have a more or less definite systematic unit upon basis of which we can confidently proceed with further work. If, however, the first species rule is made ironclad, then not infrequently will the genus be based upon a very imperfectly described species and will thus be more or less obscure, hence further systematic work based upon such determination unnecessarily runs the risk of being only ephemeral in character. We would thus make ourselves slaves to a rule of convenience (the convenience being judged from only one standpoint), rather than make the rules our servants.

Again, in many groups there is an exceedingly great advantage in taking a figured species as type. In view of the possible necessity of a future restudy of the anatomy of the type species, in order to determine some point which was not foreseen, it is often of great importance to select as type a species which is common and, therefore, easily obtained. Nor should we forget DeCandolle's excellent advice as to the value of selecting as type a species belonging to a group which contains as large a number of species as possible, in order to change as few names as possible. These are other points of view in reference to 'convenience,' points of view which are important not alone to the systematist, but also to the morphologist, while the first species rule ignores the morphologists entirely.

In return for ignoring these points, the first species rule presents to us one one-sided advantage. It is a rule of convenience—not of principle, and as such it relieves an author from knowing the literature when he attempts systematic work. My very esteemed colleague, Mr. Stone, expresses it, at least inferentially, in another way, namely, he compares the first species rule to arithmetic, hence he argues that any one can use it, while the more complicated methods he compares to higher mathematics. The comparison is an excellent one, and I accept it. But it may be remarked that the question of type determination is one which involves so many points that it should not be entered into by any person whose education has not extended beyond the arithmetic stage; like astronomy, it calls for higher mathematics; and to my mind the application of the first species rule to type determination will be found, when extended to all zoological groups, to be about as satisfactory to the systematists at large, as would arithmetic be as a final mathematical process in the case of astronomy.

I can not, therefore, see in the first species rule any inherently just principle, nor can I see in the processes which it is designed to supplant any corresponding inherently unjust principle, which indicates that future generations of zoologists would abide by the rule if adopted. Accordingly, I am unable to view this proposed legislation as advisable.

All, or practically all, systematic zoologists recognize that the principle of priority is inherently just. It commands respect, even though it irritates us occasionally. We apply it to generic names, without a murmur, or at least without murmuring very loud. If this principle is just when applied to the generic names, why is it not equally just when applied to the generic types? In the one case as in the other, the author who applies it must know the literature. As a matter of fact, the status of no generic name is satisfactorily established, from the modern point of view, until the type is designated. But when this type is once designated, by any method whatsoever, so long as the species selected was an original species, valid from the original author's point of view, and unreservedly classified in his genus, why reopen the question? At that date the generic name first complied with all of the formal conditions which can reasonably be demanded of it. Why now reverse the decision of the author who took this step, even if you or I would have done it in a somewhat different manner? If he selected the type on the first species rule, or if he did so on some other rule, or on no rule at all, the point can still be objectively demonstrated that the type was actually designated. This point being established, the question should be settled once for all. A genus can not have two separate type species; if, therefore, any author has definitely designated a type species for any given genus (regardless of his method), how can we establish another type species for it? To do so, by legislation or otherwise, is to weaken the very foundation of nomenclature —namely, the principle of the law of priority.

The discussion on this very live subject in nomenclature has convinced me more than ever of the justice of a rule to the effect that no new generic name published after a given date, say January 1, 1908, shall be entitled to consideration unless its author definitely designates a type at the time of its publication. If American zoologists approve of this proposition (several systematists have already signified their approval), I am willing to do what I can to have it inserted in the International Code. I believe it would be wiser to make such a rule retroactive (namely, to date all genera from the time their types were designated) than to adopt the first species rule at this late day.

CH. WARDELL STILES

WASHINGTON, D. C.

THE FIRST SPECIES RULE VERSUS ELIMINATION

DISCUSSIONS concerning the adoption of the first species rule for fixing the types of genera have been so generally accompanied by extravagant statements of the probable revolution that would be thus occasioned in our nomenclature that there seemed to be a need for some statement of the matter based on fact and not on theory, and my recent article in SCIENCE was intended largely to supply this need. I had no thought of starting a lengthy controversy, nor do I desire to do so now. As my friend Dr. Allen in his recent comments upon my paper relies mainly upon general statements and does not prove any of my facts or figures to be inaccurate, he does not impair the strength of my argument and there would be no call for a reply were it not that he claims that I have been (doubtless unconsciously) led into a few misleading statements. These so far as I gather from his article are:

1. "That elimination has never been practised in Europe and does not seem to be understood by foreign writers." I was perfectly well aware that the 'first reviser' principle was incorporated in the B A Code of 1842, and in most others, *i. e.*, "that when no type is indicated the author who first subdivides a composite genus may restrict the original name to such part of it as he may deem advisable." But I claim that so far as birds are concerned the first revisers in the vast majority of cases have restricted the original name to the first species and its allies and

that when they failed to do so, subsequent authors have frequently ignored them and have selected the first species as the type. Furthermore, European authors have not practised the kind of elimination that shifts *Passerina* on to the snowflake and *Sarcorhamphus* on to the eared vulture, and this sort of name shifting is what I claimed to be not understood[1] abroad.

Moreover, when we find that out of 277[2] complex genera of birds the currently accepted types of only 38 would be changed by the operation of the first species rule I am forced to believe that the first species was very generally regarded as the type by the first revisers and that the result is not a mere 'coincidence.'

2. Dr. Allen states that the rules and recommendations of Dr. Stiles referred to by me 'relate only in small part to the method of elimination' and cover the whole field of the determination of generic types, including the 'four conditions' of (1) monotypic genera, (2) type designation by the author, (3) tautonomy and (4) selection of type by subsequent author.

This is perfectly true as applied to Dr. Stiles's rules as a whole, but he has twenty-four rules and recommendations and Dr. Allen will find that I referred to only nineteen, omitting those covering the first three conditions stated above. It is true that I did include the 'first reviser prerogative' which Dr. Allen in this connection implies is not elimination. It seems to me, however, to be so intimately associated with the operation of elimination as to be inseparable from it, and Dr. Allen himself says on p. 773 that with the adoption of the 'first reviser' rule 'the elimination principle follows as a necessary corollary.' The thirteen secondary suggestions to which I referred all relate to elimination in its strictest sense.

3. On p. 775 Dr. Allen makes a statement that I fail to understand, *i. e.*, "that the first species method is 'not always so simple and direct' as I have stated and that the case of *Vultur* will show that more than one reference must be consulted even under the first species rule." I have searched in vain for any demonstration of this claim in the subsequent pages of Dr. Allen's paper. Surely to ascertain the first species mentioned by an author in describing a new genus we have only to look at his original description! Dr. Allen must certainly have misunderstood the first species method here and also at the bottom of p. 776, where he says it would conflict with the 'rule that a monotypic genus takes its sole species as its type.' If *barbatus* had been the first species in *Vultur*, as he suggests, it would of course be the type, but this would in no way affect the type of the monotypic genus *Gypaëtus* which would remain *barbatus*. *Gypaëtus* being of later date would of course be a synonym of *Vultur* just as it would have been if *barbatus* had been the only species in *Vultur* or if it had been designated by Linnæus as the type of *Vultur*. This argument simply shows that genera with the same types are synonyms and has no further bearing.

4. Dr. Allen at p. 778 calls attention to the fact that "by the first species rule, where the first species happens to be the same in two or more genera * * * all the later genera become pure synonyms of the earliest genus" and then goes on to say: "It is thus evident that Mr. Stone's statistics greatly underestimate the number of changes in names that would result from the adoption of the first species rule." This deduction is entirely unwarranted. It assumes that I overlooked the synonymizing of genera with the same first species. This I did not do and all changes due to this cause are included in my statistics.

[1] I regret that this word has proven misleading. I had no intention whatever to question the ability of our friends across the water to practise elimination as Dr. Bather supposed, but simply that they did not *interpret* the method in the way Americans have done.

[2] Since my paper was published I have continued my card list of bird genera to 1830. Up to that date I have 1,119 genera, of which 842 are either (1) monotypic, (2) have their types designated by their authors, (3) indicated by tautonomy or (4) are substitutes, leaving 277 with no indication of type, and in 86 per cent. of these the first species is the currently accepted type according to the British Museum catalogue.

As an argument against the first species rule this has no weight, as it applies with equal force to any method of fixing types. I might say, for instance, "if the types of two or more genera happen to be the same by elimination the later genera become pure synonyms of the earliest." *Otogyps* is suppressed as a synonym of *Sarcorhamphus* by this very method in Dr. Allen's paper.

So much for my 'misleading statements.' Turning now to Dr. Allen's elaborate discussion of the types of the Vulturine genera, which he gives as an example of how elimination should be practised and which we should be very glad to see, as it gives us an actual case or series of cases worked out by one who is a recognized expert in this method of fixing types.

My chief objection to the method (*i. e.*, elimination) is that it will give different results in the hands of different workers owing to the almost infinite variety of ways in which it may be applied. Dr. Allen, far from refuting this claim, actually shows that two different methods of elimination may (no doubt unconsciouly) be used by the same author in the same paper, thus emphasizing the elasticity of the method and the impossibility of formulating rules that will meet all its varied requirements.

Any one who has practised elimination knows that there are two methods in use in successively removing the species of a genus which have been made the basis of subsequent genera.

(*a*) Some remove only the species which has been made the type of a subsequent genus at the date at which the genus was established.

(*b*) Others remove along with the type any other strictly congeneric species, and here again there are two practises according as we interpret congeneric to mean congeneric from the standpoint of the author of the genus, or congeneric from the standpoint of the eliminator.

Taking Dr. Allen's elimination of *Sarcorhamphus* at the top of p. 776, he says:

Sarcorhamphus, 1806; species *gryphus*, *papa*, *auricularis*. The species *papa* was removed to *Cathartes* in 1811, *gryphus* to *Gypagus* in 1816, leaving *auricularis* as the type of *Sarcorhamphus*.

The species thus removed are not, according to Dr. Allen's conclusions, the types of the genera *Cathartes* and *Gypagus*, but they were included in these genera by their authors in 1811 and 1816, respectively. It will thus be seen that Dr. Allen adopts method '*b*' (above) in his elimination and interprets 'congeneric' to mean congeneric from the standpoint of the original author, not from that of the eliminator (or the usage of the present day). Having fixed the types of the four involved genera in this way, he next proceeds to eliminate *Vultur* by removing the species *at the dates at which they became the types* of subsequent genera—*i. e.*, according to method '*a*.'

If *Vultur* were eliminated in the same way as *Sarcorhamphus* the result would be as follows:

Vultur, 1758; species *gryphus*, *harpyja*, *papa*, *aura*, *barbatus*, *percnopterus*. The species *barbatus* was removed to *Gypaëtus* in 1784, *gryphus* and *papa* to *Sarcorhamphus* in 1806, *percnopterus* to *Neophron* in 1808, *aura* to *Cathartes* in 1811, leaving *harpyja* as the type of Vultur.

If we do not trouble ourselves to ascertain the types of *Cathartes* and *Gypagus* when we eliminate *Sarcorhamphus*, I fail to see why we have to ascertain the types of the involved genera when we eliminate *Vultur*.

As a further example of the various ways in which elimination may be practised, it will be noticed that Dr. Allen pays no attention to what may have been done to species prior to the date of the genus that he is eliminating. Under *Gypagus*, 1816, he says: "*gryphus* was removed to the genus *Gryphus* in 1854," but as a matter of fact it had already figured in the establishment of the genus *Sarcorhamphus*, 1806, and proves, according to Dr. Allen's demonstration, to be the type of *Vultur*, 1758. Here again very different results may be obtained according as we consider or ignore the work of authors prior to the date of the genus we are eliminating.

Dr. Allen truly says that elimination requires 'a thorough knowledge of the literature of the cases involved' and 'is therefore not a task a novice should meddle with.' This is another great objection to the method, since we never know when we have exhausted the literature and so never know when we have our types definitely fixed, while the worker who has not an enormous library at his command is unable to attempt to settle the application of his genera.

In the *Vultur* case, Dr. Allen, whose knowledge of ornithological literature is equaled by few, has overlooked two genera, *Rhinogryphus*, 1874, and *Torgos*, 1828, which, respectively, antedate *Œnops* and *Otogyps*. Fortunately for his eliminations these are both monotypic and their dates are such that they do not alter the results. If they had been proposed some years earlier, however, they would not only have replaced the above genera, which they do in any case, but by removing their species from other genera at earlier dates they would have altered the results of several of Dr. Allen's eliminations.

If *Torgos*, for instance, had been 1815 it would have left *gryphus* as the type of *Sarcorhamphus* instead of *auricularis*, while *Rhinogyphus* at 1815 would have left *papa* as the type of *Cathartes* instead of *aura*, and by Dr. Allen's method the type of *Vultur* would then have been *harpyja*. In other words, the discovery of two overlooked genera would not only replace two current genera by reason of priority, but would *by elimination* alter the types of three other genera. With the types fixed by the first species rule the only effect of the resurrection of the old names would be their substitution for the two current names having the same types.[3]

The *Vultur* text invites one more comment. Dr. Allen states that by ignoring 'the fixing of a type by a later author' I have 'needlessly increased the number of open cases by from probably 50 to 75 per cent.' Now as a matter of fact the fixing of a type by a later author has no status whatever in the eyes of those who practise elimination *unless it agrees with the action of revisers* up to the time that the type was so fixed. Therefore the cases are more open under the operation of elimination than if we settled them once for all by taking the first species of the original publication as the type. For example, the types of *Cathartes*, *Sarcorhamphus* and *Gypagus*, the three genera most involved in this Vulturine muddle, were definitely fixed by Mr. Ridgway in 1874, and independently by Dr. Bowdler Sharpe in the same year, each selecting the same species, as follows:

Sarcorhamphus, type *gryphus*.
Cathartes, type *papa*.
Gypagus, type *papa*.[4]

We might infer from Dr. Allen's statements that this settled the cases of these genera for all time, for he says: "There are four conditions, any one of which when present determines the type of a genus *beyond appeal* [italics mine] under current usage" and as the fourth condition he gives "4. When some subsequent author has selected one of its [*i. e.*, the original genus] species as its type."

Nevertheless, he ignores absolutely the action of these two eminent type-fixers and opens all these genera to elimination with the following results:

Sarcorhamphus, type *auricularis*.
Cathartes, type *aura*.
Gypagus, type *papa*.

It seems, therefore, that the action of a later author in fixing the type of a genus is *not* 'beyond appeal' and 'condition 4' needs an important amendment. Further examples of the unsatisfactory nature of elimination might be drawn from this case of *Vultur*, but I fear I shall be charged with rivaling the combined vision of Romulus and Remus on

[3] In spite of what Dr. Allen says on p. 777, the first species rule will give the same relief in cases where the type of one genus depends on whether or not two other groups are regarded as congeneric or not. *Cf.* Jordan, SCIENCE, 1901, Vol. XIII., p. 500, where the first species rule as advocated in my paper is formally proposed.

[4] It is interesting to note that both Mr. Ridgway and Dr. Sharp have in each instance selected the *first species* as the type and one would be inclined to suspect that they were following, consciously or unconsciously, the first species rule, though it may have been merely a 'coincidence' as Dr. Allen suggests in another connection.

the hills of ancient Rome in the number and variety of Vultures that I have been able to discern.

With Dr. Allen's closing statement that the first species rule 'has only here and there a disciple' or that it has ever been generally abandoned *in practise* so far as ornithology is concerned, I beg to differ.

The interviews and correspondence that I have had since my paper was published show that the adoption of the first species rule as there outlined meets with very general approval among vertebrate zoologists as well as entomologists, while botanists, as is well known, have long practised it.

One prominent entomologist in a recent publication hopes that it may be incorporated in the International Code at an early date, while one of the foremost zoologists of America writes me that "elimination is absolutely dead and ought not to be revived in any code or thought of in any connection."

A thorough discussion of this subject is desirable, but really, my friend Dr. Allen and I are of nearly the same mind on the question. He says at the beginning of his article: "I have always conceded that this [*i. e.*, the first species principle] would be the ideal method if we were at the threshold of our work * * * and my opposition to it has been * * * that to adopt it now would introduce serious confusion into nomenclature." This was exactly my view, and when upon investigation I found that serious confusion (so far as birds are concerned) would not ensue, I thought that there were no further grounds for objection. The other objections that have occurred to Dr. Allen in the later pages of his paper I have tried to dispel.

At the present time I feel more sure than ever that the zoological code that adopts the first species rule (excepting in relation to Linnæus) will be setting an example which will in a few years be followed by vertebrate zoologists in general and, with a possible further limitation, by invertebrate zoologists as well.

WITMER STONE

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SPECIAL ARTICLES

ON A CASE OF REVERSION INDUCED BY CROSS-BREEDING AND ITS FIXATION[1]

PERHAPS the most important extension which has been made of the law of heredity originally discovered by Gregor Mendel consists in the demonstration (chiefly by Cuènot and Bateson) that certain characters are produced only when two or more separately heritable *factors* are present together. Such a character does not conform with the simple Mendelian laws of inheritance, but its *factors* do. Herein lies the key to the explanation of so-called *heterozygous* characters and to the practical process of their fixation. This same principle serves to explain also atavism or *reversion*, and the process by which reversionary characters may be fixed.

When pure-bred black guinea-pigs are mated with red ones, only black offspring are, as a rule, obtained. The hairs of the offspring do indeed contain some red pigment, but the black pigment is so much darker that it largely obscures the red. In other words, black behaves as an ordinary Mendelian dominant. In the next generation black and red segregate in ordinary Mendelian fashion, and the young produced are in the usual proportions, three black to one red. All black races behave alike in crosses with the same red individual, but among the reds individual differences exist. Some, instead of behaving like Mendelian recessives, produce in crosses with a black race a third apparently new condition, but in reality a very old one, the agouti type of coat found in all wild guinea-pigs, as well as in wild rats, mice, squirrels and other rodents. In this type of coat red pigment alone is found in a conspicuous band near the tip of each hair, while the rest of the hair bears black pigment. The result is a brownish or grayish ticked or grizzled coat, doubtless inconspicuous and so protective in many natural situations. Some red individuals produce the reversion in half of their young by black mates, some in all, and others, as we have seen, in none, this last condition being the commonest of the three. It is evident that the

[1] Published by permission of the Carnegie Institution of Washington.

reversion is due to the introduction of a third factor, additional to simple red and simple black. It is evident further that this new third factor, which we will call A (agouti), has been introduced through the red parent, and that as regards this factor, A, some individuals are homozygous (AA) in character, others are heterozygous (transmit it in half their gametes only), while others lack it altogether. Further observations show that it is *independent* in its inheritance of both black and red; it is, in fact, an independent Mendelian character, which can *become visible* only in the presence of both black and red, because it is a mosaic of those two pigments.

The reversionary individuals (agoutis) do not breed true. They produce offspring of three sorts, agouti, black and red. When mated *inter se,* they produce young in the proportions, 9 agouti: 3 black: 4 red; when mated with reds (recessives), they produce young in the proportions 1 agouti: 1 black: 2 red.

It has been found by experiment that the F_2 agoutis (produced by mating the original agoutis *inter se*) are of three different sorts. It is probable that they are in reality of four different sorts. The three sorts thus far recognized are (1) agoutis like their parents, which produced the three sorts of young, agouti, black, and red in the proportions already stated; (2) agoutis which in matings with recessives (reds) produced only agouti young and red young in equal proportions; (3) agoutis which in matings with red animals produced *no red offspring,* but only agouti ones and black ones in equal numbers. The fourth (expected) class should when mated with reds (or any other color, for that matter) produce only agouti colored young; *i. e.,* should not only *resemble* wild guinea-pigs in the character of its coat, but should transmit this character to all its young, as wild ones do. This is on the hypothesis already stated that the factor A is a distinct Mendelian character. The gametic constitution of the four classes of agoutis would on this hypothesis be:

1. $RA \cdot B$, forming gametes R, RA, B, and BA with equal frequency.

2. $RA \cdot BA$, forming gametes RA and BA with equal frequency.

3. $BA \cdot B$, forming gametes BA and B with equal frequency; all these classes have been proved to exist.

4. The fourth (expected) class, the fully fixed agouti, known to exist but not yet produced in this experiment, should have the formula $BA \cdot BA$ like class 2 in that A is transmitted in all its gametes, but differing from it in that black also is transmitted in *all* the gametes, instead of in half of them only.

But, some one may inquire, how is it that an individual which forms *no* red gametes (as $B \cdot BA$ or $BA \cdot BA$) can nevertheless produce agouti young, which character is by hypothesis a mosaic of red with black. This is a matter which gave me considerable trouble and made the at first wholly unexpected class 3 ($B \cdot BA$) for a long time perplexing. The explanation is this: Ordinary black individuals, while homozygous, are not pure in the sense that they form no pigment but black. They probably always form a certain amount of red and of brown pigment, which is overlooked only because it is interspersed with the more opaque black pigment. If the red pigment is segregated, as is the case when the factor A is present, then it becomes visible as the agouti marking of the hair. If this factor, A, is present in both halves of the zygote together with black (and enough red to form the agouti hair tip) a homozygous agouti animal is the result (class 4); if B is present in both halves of the zygote, but A in one half only, agouti and black young will be produced.

The existence of a third factor, A, in cases of reversion in coat-character among rodents has been heretofore overlooked merely because it does not represent a distinct pigment or set of pigments, but consists in a particular kind of pigment distribution on the individual hairs. The agouti hair is due to a definite cycle of activity of the hair follicle in forming its pigments, first black, then red, then black; the wholly black hair is due to a continuous process of pigment formation without alternation in the character of the pigments produced. The relation between agouti and black is precisely the same as that between short hair and long hair due likewise to differences in follicle activity, as I have elsewhere shown, but inherited quite independently of hair pigmentation. Short hair is the result of a determinate growth cycle; the hair grows so long and then stops growing; long or angora hair is the result of indeterminate activity on the part of the hair follicle; the hair keeps growing so long as its follicle is alive.

We are now able to give a rational explanation of the *origin* of the various color varieties of rodents. The wild cavy transmits *in all its gametes* the three factors A, B and R. By accident (mutation) a gamete has been formed which lacked A. When two such gametes came together the result is a black individual, *and this individual will breed true*. Here is the explanation of our occasional black squirrels, porcupines and the like. If by a further mutation B is lost, leaving R alone, a red race is produced which will breed true and *will not give reversion on crossing with blacks*. Such are ordinary red guinea-pigs.

But if mutation is directly from the wild or agouti condition, ABR, by loss of B, leaving AR, then there is produced a red not different from ordinary reds in appearance, but which will give reversion in crosses with black.

The albino mutation, which is frequently found in wild as well as in tame rodents, is not due, as might be supposed, to simultaneous loss of the three factors A, B and R, for albinos can be shown to possess, some one, some two and some all three of these factors. They have, according to Cuènot, lost a certain other factor necessary for the production of pigment of any kind, an activating or ferment-like factor.

It has been observed that one mutation is often followed by another. De Vries in his *Mutationstheorie* speaks repeatedly of *periods of mutation*. We can begin to see the significance of this; given one mutation, we can produce others.

Suppose, for example, that we possess agouti and ordinary red varieties only and desire black, we are not compelled to await a mutation to produce it; we can cross red with agouti and obtain black in the second generation. This is not hypothesis merely; its correctness has already been in part demonstrated. Thus, in one experiment, there was employed an agouti of the formula $AB \cdot AR$, which gave only reds and agoutis in crosses with red, but the agoutis so produced when mated in the same way as the parent gave blacks as well as reds and agoutis, for they were of the formula $AB \cdot R$. From such animals homozygous blacks ($B \cdot B$) are readily obtained.

To produce a red variety from agoutis and blacks alone would not be so easy; it would be necessary either to await a mutation or to work by the slow process of selection from continuous variations in the intensity of blacks under cross-breeding with agoutis. In mice and rabbits as well as in guinea-pigs red (or yellow) varieties are well known, but in rats yellow has never been obtained separate from black, though black and agouti varieties are common, both wild and in captivity.

We now know what the 'fixation' of a heterozygous character implies. When A and B are crossed, we obtain C. C is due either simply to co-existence of A with B, or to the co-existence with them of a third factor introduced with one or the other. In either case fixation will consist in getting *into the gamete* all the factors which produce C. In the first case, the zygote is $A \cdot B$, and the resultant is equivalent to C. Fixation will consist in getting a zygote of the formula $AB \cdot AB$; every gamete produced will then bear the equivalent of C, viz., AB. In the second case, the zygote is either $AC \cdot B$ or $A \cdot CB$; fixation will consist in obtaining a zygote, $ACB \cdot ACB$; every gamete formed will then contain the three factors, A, C and B.

W. E. CASTLE
ZOOLOGICAL LABORATORY,
HARVARD UNIVERSITY,
December 26, 1906

BOTANICAL NOTES

THE RUSTS OF AUSTRALIA

UNDER this title D. McAlpine, the government pathologist of Victoria, Australia, prepared a book of 350 pages, which has been issued by the Department of Agriculture of that province. In it the author "aims at recording all rusts, as far as known in Australia at present," so as "to prepare the way for a consideration of the best methods of preventing their appearance, or limiting their spread in the numerous commercial crops subject to their ravages." Part I., consisting of 75 pages, is devoted to the general characters and mode of life of the rusts (*Uredineae*). This portion would be a very helpful text-book for college students anywhere, since the matter is presented in a clear and comprehensive manner. It appears that in Australia *Puccinia graminis* does not infect the barberry, even where the attempt is made to bring about such infection by artificial means. This is much like the condition which prevails on our own great plains, where wheat rust is sometimes very abundant, although there may be no barberry plants in the neighborhood, or if these are present they may have no aecidiospores upon their leaves. Part II., which includes 260 pages, is devoted to classification and technical descriptions. The arrangement here is principally by hosts, the rusts of the *Gramineae* being taken up first, and then those of *Cyperaceae, Juncaceae, Liliaceae, Haemodoraceae, Amaryllidaceae,* etc. Fifty-five plates (eleven of them beautifully and accurately colored) help to render the descriptions more easily understood. A glossary of technical terms, a bibliography, an alphabetical host index, a fungus index (alphabetical by genera) and a general index complete this very satisfactory volume.

POPULAR CANADIAN BOTANY

This appears to be the day of popular botany of a type somewhat different from that which used to be prepared for the perusal of the non-scientific reader. All the books on the ecological phase of botany owe much of their readableness to the fact that they are popular in a certain sense. They tell a good deal about plants and vegetation in language that may be understood by people who are not experts in systematic botany. The scientific lists of plants which used to be published were very illuminating to those botanists who knew plants by their latest Latin names, but they conveyed only the haziest ideas to other people, even though they were botanists, in some other field. So when we found books in which there were consecutive pages of 'reading matter' telling something about the kinds of vegetation in a field, a county or a state, no wonder that a good many of us rejoiced that at last we had the means of finding out about the plants of a region without the labor of building up a picture of its vegetation from the bare lists. All this time also there were popular books which aimed to please the non-scientific reader by presenting the beauties, the oddities, the curiosities in nature. Such books usually dwelt upon the wonders which the reader might see if only he was willing to open his eyes in the right way—the way of the author, of course. That such books are not yet extinct is shown by the republication, after revision, of Mrs. Traill's 'Studies of Plant Life in Canada' (W. Briggs, Toronto), a book of somewhat more than two hundred pages of descriptive text, accompanied by twenty plates, a part of them done in colors. The text is likely to appeal to many young people who have not yet waked to an appreciation of the scientific aspects of botany, and it will appeal, also, to many older persons who look at all vegetation as something to be admired and enjoyed without too much hard study. It has been recommended by several of the Canadian botanists as a book for use in nature study work in the public schools, and no doubt it might be helpful in such case if used as a reading text for the purpose of suggesting the notice of the many pretty and attractive plants to be found everywhere. Since poetry and Scripture are freely quoted the moral effect of the book is likely to be elevating, at least the author has distinctly intended it to be so.

THE PHILIPPINE JOURNAL OF SCIENCE

The announcement is made that beginning with the second year of its publication *The Philippine Journal of Science* will be issued in three divisions or series, viz., (*a*) general science, (*b*) medical science, (*c*) botany. This will enable botanists to subscribe for the botanical parts alone, a considerable convenience, since the subscription price is only two dollars (U. S. currency) for the botanical series, instead of five dollars, as heretofore for the whole journal. As this publication is of constantly increasing importance to American botanists, it is hoped that it will be generously supported. Subscriptions are to be sent to the Director of Printing, Manila, P. I.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

FELLOWS ELECTED AT THE NEW YORK MEETING OF THE AMERICAN ASSOCIATION

SECTION A, MATHEMATICS AND ASTRONOMY

Baker, Robert Horace, Amherst, Mass.

Bliss, Gilbert Ames, Princeton, N. J.

Bowie, William, Coast and Geodetic Survey, Washington, D. C.

Brown, George Lincoln, Brookings, S. D.

Dugan, Raymond S., Princeton, N. J.

Faught, John Brookie, 1312 Presque Isle Ave., Marquette, Mich.

Glenn, Oliver Edmunds, 1227 Clay Avenue, Springfield, Mo.

Graham, William Joseph, 1 Madison Avenue, New York, N. Y.

Granville, William Anthony, Yale University, New Haven, Conn.

Hadley, Stephen M., Oskaloosa, Iowa.

Leavitt, Henrietta Levan, Harvard College, Cambridge, Mass.

Lowell, Percival, 53 State Street, Boston, Mass.

Maclay, James, Columbia University, New York, N. Y.

Manning, Henry Parker, Brown University, Providence, R. I.

Olds, George Daniel, Amherst, Mass.

Plimpton, George Arthur, 70 Fifth Avenue, New York, N. Y.

Poor, Charles Lane, 4 East 48th Street, New York, N. Y.

Riggs, Norman C., Armour Institute, Chicago, Ill.

Smith, Franklin Hans, P. O. Box 762, Denton, Texas.

Snyder, Z. X., Greeley, Colo.

Washburne, Alva Courting, c/o Hartford Mutual Life, Hartford, Conn.

Wilson, Norman Richard, Winnipeg, Canada.

Young, Anna Sewell, South Hadley, Mass.

SECTION B, PHYSICS

Blaker, Ernest, 402 Oak Avenue, Ithaca, N. Y.

Burgess, Geo. K., Bureau of Standards, Washington, D. C.

Chamberlain, C. W., Denison University, Granville, Ohio.

Earhart, R. F., Ohio State University, Columbus, Ohio.

Fox, Wm., College of the City of New York, New York, N. Y.

Gates, Fannie Cook, Woman's College, Baltimore, Md.

Hower, Harry S., Carnegie Technical Schools, Pittsburg, Pa.

Lyman, Theodore, Harvard University, Cambridge, Mass.

Ramsey, Rolla R., Indiana University, Bloomington, Ind.

Schultz, Lewis G., Mount Weather, Bluemont, Va.

Wolcott, E. R., Colorado School of Mines, Golden, Colo.

SECTION C, CHEMISTRY

Walker, W. H., Mass. Institute of Technology, Boston, Mass.

SECTION D, MECHANICAL SCIENCE AND ENGINEERING

Humphreys, Alex. C., Stevens Institute of Technology, Hoboken, N. J.

SECTION E, GEOLOGY AND GEOGRAPHY

Brock, Reginald W., Ottawa, Canada.

Cleland, Herdman Fitzgerald, Williamstown, Mass.

Gould, Charles Newton, Norman, Oklahoma.

McCaskey, Hiram Deyer, Manila, P. I.

Miller, Benjamin L., Bryn Mawr, Pa.

Montgomery, Henry, Toronto, Canada.

O'Harra, Cleophas C., Rapid City, S. D.

Peck, Frederick B., Easton, Pa.

Richardson, G. B., U. S. Geological Survey, Washington, D. C.

Veatch, A. C., U. S. Geological Survey, Washington, D. C.

SECTION F, ZOOLOGY

Baird, Robert Logan, Denmark Academy, Denmark, Iowa.

Bancroft, F. W., University of California, Berkeley, Cal.

Barbour, Thomas, Museum of Comparative Zoology, Harvard University, Cambridge, Mass.

Batchelder, Charles F., 7 Kirkland Street, Cambridge, Mass.

Forbes, Stephen Alfred, Urbana, Ill.

Harris, J. Campbell, 119 S. 16th Street, Philadelphia, Pa.

Hay, W. P., Howard University, Washington, D. C.

Lee, Thomas G., University of Minnesota, Minneapolis, Minn.

SECTION G, BOTANY

Harris, J. Arthur, Mo. Botanical Garden, St. Louis, Mo.

Millspaugh, Charles Frederick, Field Museum, Chicago, Ill.

Penhallow, D. P., McGill University, Montreal, Canada.

SECTION I, SOCIAL AND ECONOMIC SCIENCE

Austin, Oscar P., Dept. Commerce and Labor, Washington, D. C.

Blackshear, E. L., Prairie View, Texas.

Chancellor, W. E., Supt. of Schools, Washington, D. C.

Crunden, Frederick M., St. Louis, Mo.

Emory, Frederic, Queenstown, Md.

MacCracken, John H., Syndic of New York University, Washington Square, New York, N. Y.

McKelvay, A. J., Charlotte, N. C.

Norton, J. P., Yale University, New Haven, Conn.

O'Shea, M. V., University of Wisconsin, Madison, Wis.

Payne, Bruce R., Virginia University, Charlottesville, Va.

Wallace, Henry, Des Moines, Iowa.

SECTION K, PHYSIOLOGY AND EXPERIMENTAL MEDICINE

Bancroft, Frank Watts, University of California, Berkeley, Cal.

Brinckerhoff, Walter R., Public Health and Marine-Hospital Service, Honolulu, Hawaii.

Buxton, B. H., Cornell Medical School, New York, N. Y.

Crampton, C. Ward, Public Schools of New York, New York, N. Y.

Hall, Winfield S., Northwestern University, Chicago, Ill.

Hooker, Donald R., Johns Hopkins University, Baltimore, Md.

Langmann, Gustav, 121 West 57th Street, New York, N. Y.

Macleod, J. J. R., Western Reserve Medical College, Cleveland, O.

Mendel, L. B., Yale University, New Haven, Conn.

Meylan, Geo. L., Columbia University, New York, N. Y.

Soper, George A., 29 Broadway, New York, N. Y.

Stahley, Geo. D., Penn. College, Gettysburg, Pa.

Stewart, C. C., Hanover, N. H.

Sullivan, M. X., Brown University, Providence, R. I.

Terry, B. T., Rockefeller Institute, New York, N. Y.

SECTION L, EDUCATION

Brown, Elmer E., U. S. Commissioner of Education, Washington, D. C.

THE SHEFFIELD LECTURE COURSE

Professor Chittenden, director of the Sheffield Scientific School, has just announced the Sheffield Lecture Course for 1907, Friday evenings, commencing on January 18, to be held at College Street Hall:

January 18—'The Bird Life of California,' by Mr. Frank M. Chapman.

January 25—'The Making of Empire: Nome and Dawson,' by Professor Angelo Heilprin.

February 1—'Progress in Food Production, as illustrated by Milk Supply,' by Professor Lafayette B. Mendel.

February 8—'Through the Great Lakes to the Yellowstone,' by Mr. Howard W. DuBois, M. E.

February 15—'The Life and Legends of the Blackfeet Indians,' by Mr. Walter McClintock.

February 22—'Commercial Waterways and their Economics,' by Professor Lewis M. Haupt.

March 1—'Experiences Among the Chinese,' by Mr. Bailey Willis, M.E., C.E.

March 8—'Meteorites,' by Dr. George P. Merrill.

March 15—'Whales and Whaling,' by Mr. Frederic A. Lucas.

March 22—'Earthquakes, Cause and Effect,' by Professor Herbert E. Gregory.

All the lectures are to be illustrated, and the composition of the course seems unusually attractive.

SCIENTIFIC NOTES AND NEWS

The officers of the American Society of Naturalists, elected at the New York meeting, are as follows: *President,* Professor J. Playfair McMurrich, of the University of Michigan; *Vice-President,* Professor D. P. Penhallow, of McGill University; *Treasurer,* Dr. Hermann von Schrenk, Missouri Botanical

Garden; *Secretary,* Professor E. L. Thorndike, Columbia University; *additional members of the Executive Council,* Professor W. E. Castle, Harvard University, and Dr. Charles B. Davenport, Cold Spring Harbor, N. Y. The society will meet next year at Chicago in convocation week.

DR. FRANZ BOAS, of Columbia University, has been elected president of the American Anthropological Association.

PROFESSOR J. H. COMSTOCK, of Cornell University, has been elected president of the American Entomological Society, which was organized in New York City during convocation week.

MARSTON TAYLOR BOGERT, professor of organic chemistry at Columbia University, has been elected president of the American Chemical Society for the year 1907.

DR. WILLIAM BATESON, fellow of St. John's College, Cambridge, well known for his work on variation and heredity, will give the Silliman memorial lectures at Yale University next year. The preceding lecturers on this foundation have been Professor J. J. Thomson, of Cambridge; Professor C. S. Sherrington, of Liverpool; Professor Ernest Rutherford, of McGill, and Professor Walther Nernst, of Berlin.

DR. OTTO LUMMER, professor of experimental physics at Breslau, will begin his course of ten lectures at Columbia University on February 15. Dr. Joseph Larmor, of St. Johns College, Cambridge, will begin a course of six lectures on March 27.

PROFESSOR ERNEST W. BROWN, who goes at the end of the present academic year from Haverford College to Yale University, has been awarded the gold medal for 1907 by the Royal Astronomical Society for his work on the movements of the moon.

THE council of the Geological Society of London has made the following awards: The Wollaston medal to W. J. Sollas, F.R.S., professor of geology at Oxford; the Murchison medal to A. Harker, F.R.S. of Cambridge; the Lyell medal to J. F. Whiteaves, paleontologist to the geological survey of Canada; the Bigsby medal to A. W. Rogers, director of the geological survey of Cape Colony; the Wollaston fund to A. Vaughan, for his work on zoning the Carboniferous Limestone of England; the Murchison fund, to F. Oswald, for his book on the geology of Armenia; the Lyell fund, divided between T. Sheppard, of Hull, Yorkshire, and T. C. Cantrill, of the Geological Survey of England.

AN oil portrait of Dr. J. C. Branner, vice-president of Leland Stanford Jr. University, and lately state geologist of Arkansas, has been presented him by the members of the former survey as an expression of their high regard and of their appreciation of his example and inspiration as a geologist and as a man. The portrait was painted by Mrs. Richardson, of San Francisco.

IT appears from the daily papers that a vote has been taken in Germany on the twelve greatest Germans now living: Dr. Robert Koch, Professor Ernst Haeckel, Professor Konrad Röntgen and Professor Ernst von Behring occupy, respectively, the third, fourth, fifth and eleventh positions in this list.

THE Carnegie Institution of Washington has made a grant of $3,000 a year for a period of four years to Dean W. F. M. Goss, of Purdue University, for the purpose of determining the value of superheated steam in locomotive service; first, in connection with single expansion engines; and second, in connection with compound engines. This is the second grant which the institution has made to Dean Goss. While given to him personally, its effect will be to stimulate and to make more effective the work of the Purdue Locomotive Laboratory. Funds thus received will be employed in supplementing the resources of the laboratory as derived from all other sources. The results of Dr. Goss's previous research under the auspices of the Carnegie Institution, which was for the purpose of determining the value of different steam pressures in locomotive service, are now in press.

KING EDWARD has granted to Professor Sir Rubert William Boyce, Professor Major Ronald Ross, C.B., and Mr. John Lancelot Todd license and authority to accept the cross of commander of the Order of Leopold II., which decoration has been conferred upon them by the King of the Belgians, in recognition of their valuable services to the cause of medical science.

Mr. E. H. Wilson has been sent by the Arnold Arboretum of Harvard University to make collections in central and western China.

Major C. F. Close and Captain G. R. Frith, Royal Engineers, have come to this country on behalf of the British government to study the methods of the U. S. Coast and Geodetic Survey.

Dr. Hugo Münsterberg, professor of psychology in Harvard University, has returned to Cambridge after a visit to Germany.

Mr. Ernest Chubb, employed in the collection of recent mammals at the British Museum (Natural History), has been appointed assistant curator of the Buluwayo Museum, Rhodesia.

Dr. M. X. Sullivan, instructor in physiology in Brown University, has been appointed an expert in fertility investigations in the Bureau of Soils, U. S. Department of Agriculture.

Professor A. Lawrence Rotch delivered a lecture on the 'Exploration of the atmosphere over land and sea' before the Canadian Institute at Toronto, on January 12. Mr. Stupart, the director of the Canadian Meteorological Office, expects to establish a sub-station at Toronto, and may be able to carry out the wish of the International Committee for Scientific Aeronautics by creating stations for aerial soundings in Newfoundland and Bermuda.

The seventh lecture in the Harvey Society Course will be given by Professor Edmund B. Wilson, of Columbia University, at the New York Academy of Medicine, on Saturday evening, January 26, at 8:30 p.m. Subject: 'Recent Studies of Heredity.' All interested are invited to be present.

On February 1, Dr. A. G. Webster, head of the Department of Physics in Clark University, will give the address on Founder's Day commemorating the anniversary of the birth of Jonas B. Clark.

Dr. George H. Shull, of the Carnegie Station for Experimental Evolution of the Carnegie Institution, spoke before the Columbia chapter of the Sigma Xi Society on January 17 on 'Recent Aspects of Plant Breeding and Variation.'

January 11 was the hundredth anniversary of the birth of Ezra Cornell. The founder's day exercises at Cornell University, however, have been postponed until April 26. It is hoped that Governor Hughes, ex-officio trustee, and formerly of the faculty, will be present and speak.

On the evening of December 31, Dr. John C. Hemmeter presented to the Medical and Chirurgical Faculty of Maryland a life size marble bust of Rudolph Virchow, and made an address on 'Virchow as an Anthropologist.'

The Rev. Dr. James Woodrow, formerly professor of natural sciences in the University of South Carolina, has died at the age of seventy-nine years.

Dr. Wilhelm Königs, professor of chemistry at Munich, has died at the age of fifty-five years.

Advance proofs of the report of the director of the Missouri Botanical Garden show the customary activity and growth of that institution for the past year. Visitors numbered 117,553, an increase of 16 per cent. over the number for 1905, and 36 per cent. more than the average for earlier years—except the World's Fair year. The number of species and varieties of plants cultivated was increased about 7 per cent., to a total of 17,072. Over 300 kinds of chrysanthemums, in 4,000 specimens, were tented in the fall, and viewed by about 17,000 persons. The herbarium was increased about 6 per cent., to a total of 559,267 specimens, valued at $83,890.05. The number of books and pamphlets in the library was increased about 7 per cent., to 54,895; and the total valuation of the library was increased about 5½ per cent., to $89,023.26.

A LETTER to the Harvard College Observatory from the Rev. Joel H. Metcalf, of Taunton, Mass., announces that he photographed the planet Ocllo (475) on 1907, Jan. 11^d, $15^h\,30^m.5$ G. M. T. in

R.A. $7^h\ 32^m\ 17^s.8$ (1855).
Dec. $+48°\ 22'\ 58''$. (1855).

Note: This object is of interest since its orbit has a greater eccentricity than that of any other known asteroid. A year or two ago it was in danger of being lost. It is now very faint as its computed magnitude at opposition on January 12, 1907, was 14.2. See Harvard Circulars 63, 101 and 103. [E. C. P.]

THE heirs of Dr. J. Brettauer, of Trieste, have presented to the University of Vienna his collection of medical medals. They gave with it a small endowment for the maintenance and enlargement of the collection.

THE secretaries of the American Philosophical Society announce that the general meeting of 1907 will be held on April 17, 18 and 19, beginning at 2 P.M. on Wednesday, April 17. Members desiring to present papers, either for themselves or others, are requested to send to the secretaries, at as early a date as practicable and not later than March 20, 1907, the titles of these papers, so that they may be announced on the program which will be issued immediately thereafter, and which will give in detail the arrangements for the meeting. Papers in any department of science come within the scope of the society, which, as its name indicates, embraces the whole field of useful knowledge. The publication committee, under the rules of the society, will arrange for the immediate publication of the papers presented in the *Proceedings* or the *Transactions,* as may be designated.

THE Pennsylvania Farmers' Congress has passed the following resolution:

WHEREAS, we the members of the Pennsylvania Farmers' Congress in our annual session at The Pennsylvania State College, realizing not only the great work now being done by the School of Agriculture and the Experiment Station, but also realizing that the demands upon these institutions in the near future will be greatly enlarged if Pennsylvania is to assume and maintain the position that naturally belongs to her agriculturally among her sister states.

Therefore, be it resolved that we urge upon our representatives in the Pennsylvania Legislature the most careful consideration of, and the most generous response to, the needs of The Pennsylvania State College for the maintenance of these institutions for the coming two years.

THE Royal Swedish Academy of Sciences will publish early in the present year the first three volumes of Swedenborg's scientific works, edited from the original MSS. in the Library of the Royal Academy in Stockholm, by a committee of the Royal Academy, assisted by Alfred H. Stroh, of the Swedenborg Scientific Association of America. The first three volumes will be: Vol. I. Geology—Introduction by A. G. Nathorst. Vol. II. Chemistry, Physics, Mechanics—Introduction by Svante Arrhenius. Vol. III. Cosmology—Introduction by Svante Arrhenius. These volumes will be followed by others on anatomy with introductions by Gustav Retzius. The volumes are in Latin with the introduction in English. They are the result of the most notable efforts to put in the hands of the scientific world the earlier writings of Swedenborg. A new and revised edition of his *Principia Rerum Naturalium* in English, long out of print, is now going through the press in England, under the auspices of the Swedenborg Society of London.

THE report of the U. S. Geological Survey on the production of gold and silver during 1905, compiled by Mr. Waldemar Lindgren, has now been published. The figures showing the production of gold and silver, in approximate distribution by states and territories, are the result of conference and adjustment between the Geological Survey and the Bureau of the Mint, and are accepted as final by the two bureaus. The total production of gold was 4,265,742 fine ounces valued at $88,180,700; the total production of silver was 56,101,600 fine ounces valued at $34,221,976, making an entire total value of $122,402,676. The production of gold in the United States for 1905 represents an increase of $7,716,000 in value over the production of 1904. The rapid advance in gold production which began in 1892, but temporarily halted from 1901 to 1903, was resumed in 1904. This increase in 1904 over the output of 1903 was approximately $7,000,000 and in all probability the increase in 1906 over 1905 will be at least the same amount. The chief sources of the great increase are as follows: Alaska added about $6,000,000 to its output of $9,160,458 in 1904, and Colorado, Nevada and Utah added about $1,000,000 each to their product of the previous year. On the other hand, decreases are noted in Arizona, Idaho, and other states. The states producing over $1,000,000 in gold rank at present in the following order: Colorado, California, Alaska, South Dakota, Nevada, Utah, Montana, Arizona, Oregon and Idaho. The production of silver in 1905 represents a decrease of 1,581,200 ounces in actual output, but in spite of this the increase in the average price 4 cents an ounce (from 57 cents in 1904 to 61 cents in 1905) effected an addition to the value in 1904 of $765,952.

UNIVERSITY AND EDUCATIONAL NEWS

The *Experiment Station Record* states that a new agricultural college and research institute for Madras is now in course of erection. In 1905 a grant to the presidency by the government of India of $50,000 per annum, which was subsequently increased to $100,000, added to the allotment made by the government of Madras, removed all financial difficulty experienced by the Madras agricultural department. The result of this improved financial condition was the decision of the government to close the agricultural college at Saidapet and establish a new college and research institute, adequately equipped with laboratories and class-rooms and with a suitable farm near Coimbatore. The staff will consist of an expert agriculturist as the principal of the college, a superintendent of the central farm, a government botanist and an agricultural chemist. Ultimately an entomologist and mycologist may be added. The staff will combine teaching with research work. Problems connected with the agriculture of the presidency will be studied in the laboratory and the field, while the students will be given a general education in all branches of agricultural science.

A fire, originating in the photographic room of Marischal College, Aberdeen, caused damage amounting to about £500.

There will be two fellowships open in the department of zoology and entomology in the Ohio State University for the coming university year. These provide a salary of $300 and cover also tuition and laboratory fees. The holder is expected to carry on graduate work and may be called upon for assistance for laboratory work not to exceed half of the university time. Applications should be addressed to Professor Herbert Osborn, Ohio State University, Columbus, Ohio.

Dr. J. H. Wright has been appointed assistant professor of pathology in Harvard Medical School.

Mr. Bertram G. Smith, for the past three years an assistant in zoology in the University of Michigan, has been appointed instructor in biology in Lake Forest College, at Lake Forest, Ill.

Mr. Alfred Akerman, formerly state forester of Massachusetts and at present state forester of Georgia, has been given charge of the department of forestry which has been inaugurated at the University of Georgia.

The following appointments have been made at George Washington University: Isaac Wright Blackburn, M.D. (Pennsylvania), professor in Georgetown University, to be professor of morbid anatomy; George Albert Ross, A.M. (Columbian), professor of mathematics in Hardin College, to be instructor in mathematics, and Mr. Abraham Press, to be lecturer in engineering.

Mr. W. E. Colling has been placed in charge of a department of economic zoology at the University of Birmingham.

Dr. Devaux has been appointed professor of plant physiology, and Dr. Marchis, professor of physics, at the University of Bordeaux.

Mr. A. W. Andrews, M.A., has been appointed to the new lectureship in geography at University College, Aberystwyth.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, FEBRUARY 1, 1907.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE CONTRIBUTIONS OF AMERICA TO GEOLOGY [1]

IN speaking of the contributions of America to geology, I do not propose to give an inventory of the geological facts which have been made known as the result of work in this country. An area of three million square miles has been covered by geological reconnoissance, and much of that area has been surveyed in detail. It is, moreover, an area interesting in many respects. It is a country of vast mineral wealth, leading the world in the production of coal, petroleum, salt, iron, copper, silver and lead, and ranking with Australia and South Africa as one of the three great gold-producing regions of the world. It is a country presenting a remarkable variety of topography and geological structure; and some of its scenic features can be adequately described only in superlatives. It includes vast prairies of great fertility; the Missouri-Mississippi river system, with thousands of miles of navigable waters; the broad and complex mountain mass of the Cordillera; the Great Lakes of the Canadian border, a chain of fresh-water seas; the sublime cataract of Niagara; the remarkable region of interior drainage of the Great Basin; the geysers of the Yellowstone; the cañon system of the Colorado and its tributaries, unparal-

[1] Address of the vice-president and chairman of Section E—Geology and Geography, American Association for the Advancement of Science, New York meeting, December, 1906.

leled in depth and extent; the lava flood of the northwest, surpassed in area only by that of the Deccan. The exploration of a region so vast and so varied must, of course, have made an important addition to the facts upon which geological science is founded.

Of these facts, however, I do not propose to give even an outline. I propose rather to ask the question, 'What has our country contributed to the stock of geological ideas?' In that classical history of geological science which Lyell has given us in his 'Principles of Geology,' he calls attention to the fact that the share which different nations bore in the early development of geological science was dependent not alone upon the genius of individual workers, but in large measure upon the peculiar geological conditions of the various countries in which they worked. It was in the presence of the varied mineral wealth of the Erzgebirge that Werner laid the foundations of mineralogy and lithology; the magnificent display alike of igneous and of aqueous agencies in the Highlands of Scotland helped to guide Hutton to those theoretical views which were the beginning of modern dynamical geology; the remarkable completeness with which, for an area so small, the series of stratified formations is developed in England, gave William Smith the opportunity to lay the foundations of stratigraphical geology; and the abundant vertebrate fossils in the Paris basin enabled Cuvier to create the science of paleontology. That history of the labors which founded our science in the close of the eighteenth and the beginning of the nineteenth century, suggests the question whether there have been developed any characteristically American ideas in geology.

Of course, it must be admitted that there is to-day no department of geological science which is as characteristically American as mineralogy was German, and dynamical geology was Scotch, and stratigraphical geology was English, and paleontology was French, a century ago. The conditions of life in colonial days and in the early decades of our national history did not afford the opportunity for fruitful scientific investigation. We had no universities, no libraries, no museums, that could be compared with those of the Old World. The conquest of the land from forest and wild beast and savage man, the achievement of national independence, the beginning of the development of national wealth, necessarily preceded the beginning of scientific investigation. When the time came for scientific work in this country, geology had already become a recognized science. That early stage of incompleteness in which one department was developed here and another there, had passed. It was too late for any country to create a new department in geology. I believe, nevertheless, that there have been certain contributions to the stock of geological ideas which are characteristically American; and that it is not fanciful, in the spirit of that passage of Lyell which I have cited, to connect these characteristically American ideas with our geological environment.

I. THE PERMANENCE OF CONTINENTS

Among the early state geological surveys there were two commenced in 1836 which were destined to have in different ways a surpassing influence upon the development of geological thought. Those were the surveys of New York and Pennsylvania. In passing southwestward across New York, from the Adirondacks to the Pennsylvania border, one would traverse substantially the whole series of Paleozoic formations from the Cambrian to the Carboniferous, most of these formations extending across the state in approximately parallel east and west bands. That remarkably complete development of the Paleozoic series made the stratigraphy of New York a standard of comparison for the subsequent study of the Paleozoic strata throughout the eastern United States, as the stratigraphic series of England had already become, in its broader outlines, a standard of comparison for the world. But that regular succession of Paleozoic strata from the Adirondacks to the Pennsylvania border was suggestive of a dynamic idea. It suggested a gradual emergence of the land from the waters of the sea. In later years, as geological investigation extended westward, a somewhat similar succession could be traced in the Mississippi basin, southward from the Archæan mass north of the Great Lakes. The doctrine of the permanence of continent and ocean—the gradual emergence of continental lands and the withdrawal of the waters into the deepening ocean basins—was first enunciated by Dana in 1846. He had just returned from his voyage around the world in Wilkes's exploring expedition. In that voyage he had studied the phenomena of barrier reefs and atolls, had adopted Darwin's theory of their origin by subsidence, and had defended and illustrated the theory by a far greater wealth of observation than Darwin's route had afforded him the opportunity to make. It was, apparently, the thought of the subsiding ocean bottom, rather than the thought of the emerging land, by which Dana was first led to the doctrine of the permanence of continent and ocean; but, in his presidential address before the American Association for the Advancement of Science, in 1855, Dana refers to the stratigraphy of New York as illustrating the idea of continental emergence. The generation of students who have learned geology from Dana's 'Manual' and 'Text-book,' will remember how prominently he brings forward the succession of Paleozoic formations in New York, as illustrating his conception of the gradual emergence of the land from a continental sea.

The doctrine of the permanence of continents when announced by Dana was essentially a new one. Geologists and pseudo-geologists of all classes had felt at liberty to redistribute continents and oceans according to their own sweet will. After the biblical pseudo-geologists had become convinced of the impossibility of the deposition of the whole series of fossiliferous strata in the Noachian deluge, their next shift was the supposition that the fossiliferous strata had been deposited in the ocean in the interval between the creation and the deluge, and that at the time of the latter event continent and ocean were reversed. Hutton believed that the débris of the continents was carried far out to sea by means of ocean currents, and was deposited over substantially the whole floor of the ocean; and, when one continent was worn away, another might be uplifted in some other part of the world. Lyell eliminated the catastrophic element of Hutton's theorizing; but, like his predecessor, Lyell believed in an indefinite amount of change in the distribution of continent and ocean. In attempting to find a geological explanation for changes of climate, he felt at liberty to speculate on a series of changes in the distribution of continent and ocean which would sometimes bunch the continents around the poles, and at other times girdle the earth with an equatorial belt of land. The readers of Darwin's 'Letters' will remember his half comic, half pathetic protest, in a letter to Lyell, that the disciples of the great geologist 'in a slow and creeping manner beat all the old catastrophists who ever lived.'

There is now little doubt that Dana was right in his general conception. The greater density of the suboceanic masses in comparison with the subcontinental masses, as shown by pendulum observations, indicates that the distinction between continent and ocean has its basis in the heterogeneity of the material in the interior of the earth; and the determining conditions must, therefore, have had their origin in the initial aggregation of that part of the primitive nebula which formed the earth; or, perhaps, as suggested by Chamberlin and Salisbury, in changes attendant upon the beginning of the formation of the ocean. The study of the sedimentary rocks which cover our existing continents shows that almost all of them were deposited in shallow waters; many of the strata, indeed, in waters so shallow that the layers of mud and sand were from time to time exposed by the receding tide or the subsiding freshet, to dry and crack in the sun or to be pitted by raindrops. None of the sedimentary deposits seem to have been formed in waters of truly oceanic depth.

Certain it is, however, that Dana made the evolution of the continents too simple an affair. He recognized, indeed, that the progressive emergence of the continental lands was attended by continual oscillation; yet, even in the last edition of his 'Manual,' it appears that he did not duly appreciate the magnitude of those oscillations. We now know that in early Cambrian time the Mississippian sea was only a sound or strait, most of the area in which the Trenton limestone was subsequently deposited being then dry land. Only gradually did the Appalachian strait widen out into the Mississippian sea. A true conception of continental evolution must recognize two complementary truths: a wide range of oscillatory movement, and yet on the whole a progressive deepening of ocean basins and a progressive emergence of continental lands. Chamberlin has formulated the doctrine of an alternation of marine and continental periods due to an intermittently progressive deepening of the ocean basins, and has connected therewith an ingenious and beautiful theory of climatic changes in geological time.

The doctrine of the progressive evolution of continents, as taught by Dana, gave new clearness and emphasis to the general conception of geology as a history of the globe. Le Conte, in his cordial and generous eulogy of Dana, declared that 'geology became one of the great departments of abstract science, with its own characteristic idea and its own distinctive method, under Dana.' There is certainly somewhat of exaggeration in this commendation, yet the statement contains an important truth. More or less clearly, all geological investigators must have felt that the distinctive idea of geology is that the structures of the rocks of the earth's crust have their supreme significance as monumental inscriptions, the deciphering of which may reveal to us the history of the earth. Yet this conception was never before so clearly formulated, and the whole treatment of the subject so consistently adjusted thereto, as in the writings of Dana. The portion of previous manuals dealing with the local distribution of the series of strata had generally borne some such title as 'Stratigraphical Geology'; and very commonly, as in the well-known works of Lyell and De la Beche, the series had been traced backward, beginning with the most recent strata. In Edward Hitchcock's 'Elementary Geology,' with which, in my boyhood, I commenced the study of the science, the stratigraphic chapter bears the title, 'Lithological Characters of the Stratified Rocks.' It occupies only twenty pages in a book of more than four hundred pages. It traces the formations backward in Lyellian fashion. Separate from the stratigraphic chapter, is another and longer chapter on paleontology, which is arranged botanically and zoologically, and not chronologically. The phrase, 'Historical Geology,' which forms the title of the largest section of Dana's 'Manual,' involves a distinct clarification of the general view of the science. Starting with this conception, he, of course, dealt with the earliest formations first. In the treatment of each era he endeavored to reconstruct, from the evidence afforded by the kinds and distribution of the rocks, the physical geography of the time. The subdivisions of that chapter of the 'Manual' are characterized, not as series, systems and groups of strata, but as eras, periods and epochs of time. The common use, in recent geological writings, of such phrases as 'Silurian era,' rather than 'Silurian system,' etc., is a testimony to the influence of Dana's mode of treatment.

II. THE THEORY OF MOUNTAIN-MAKING

The Geological Survey of Pennsylvania, to which I have already referred, made known the folded structure—the alternate anticlines and synclines—of the Appalachians. The beautiful sections of these folded strata, in the Atlas of that survey, reveal the thoroughness with which the structure of the mountains was investigated by Henry D. Rogers; and can be studied with delight to-day, in spite of the fantastic nomenclature, which the student has to translate into the familiar language of the New York Survey. In those sections it appears that, in general, each fold is unsymmetrical, the dip on the northwest side being steeper than that on the southeast, if, indeed, the dip on the northwest side is not carried beyond the perpendicular and reversed. While in each fold, as a rule, the dip is steeper on the northwest side than on the southeast, if we compare the successive folds, we find the dips growing more gentle as we go from southeast to northwest. The nearly vertical or overturned dips of the folds on the eastern border pass by a gradual transition into gentle undulations on the western border of the Appalachian zone.

While the stratigraphy was worked out so beautifully in the first Geological Survey of Pennsylvania, the dynamic conception derived from it was crude indeed. The conclusions of the author are summed up in the following sentences: "The wave-like structure of undulated belts of the earth's crust is attributed to an actual pulsation in the fluid matter beneath the crust, propagated in the manner of great waves of translation from enormous ruptures occasioned by the tension of elastic matter. The forms of the waves, the close plication of the strata, and the permanent bracing of the flexures, are ascribed to the combination of an undulating and a tangential movement, accompanied by an injection of igneous veins and dykes into the rents occasioned by the bendings. This oscillation of the crust, producing an actual floating forward of the rocky part, has been, it is conceived, of the nature of that pulsation which attends all great earthquakes at the present day." The wave-like form of the Appalachian anticlines and synclines is a beautiful generalization of accurate and conscientious observation; but the dynamic theory suggested for its explanation needs to-day no other refutation than its simple statement.

But, however completely the Pennsylvania geologists failed to construct a satisfactory theory of mountain-making, their observations of Appalachian structure were of immense value in their destructive effect upon some of the notions of mountain-making prevalent at the time. In the text-books in the early part of the nineteenth century, a diagram often appeared representing a transverse section of an ideal mountain range. A vertical wall of crystalline rock forms the center and the crest of the range. Against this wall, on each side, the oldest strata lean in a nearly vertical position; farther from the center, strata of less antiquity lean with more gentle dips against the steeper and older strata; and finally, on the flanks of the mountain range, still newer strata rest nearly horizontally. The diagram represents a dislocation of the strata by repeated vertical upthrusts of the crystalline core. Of course, it was seen at once that there was nothing common to these ideal sections and the real sections of the Appalachians revealed by the Pennsylvania Survey. The billowy form of the Appalachian folds meant something, and something very different from the prevalent conception of mountain ranges.

The true interpretation of the Appalachian waves is probably to be found in the contractional theory of mountain elevation, of which Dana was the leading expounder. The views of Le Conte on the subject of mountain-making were in most respects similar to those of Dana. But, while Le Conte's discussions were of great value, the priority in the general development of the theory belongs to Dana. "To the North American geologists," says von Zittel, "undoubtedly belongs the credit of founding the theory of horizontally acting forces and rock-folding upon an ample basis of observation."

That the main cause of mountain elevation is tangential pressure in the crust resulting from internal contraction, is now generally acknowledged, though there may be doubt whether the main cause of contraction is the cooling of the earth from an incandescent condition, as assumed in the commonly accepted form of the nebular theory, or the gravitational adjustment of an incoherent mass of meteors, as assumed in the more recent planetesimal hypothesis of Chamberlin and Moulton.

The idea of the contractional origin of mountains was not, indeed, original with Dana. There was a glimmer of the idea in the writings of Leibnitz, and Constant Prévost developed the idea into a definite scientific theory; but the elaboration of the theory into its present form we owe chiefly to Dana. His discussion of the subject began in the *Journal of Science*, in 1847. In later years he returned to the subject again and again; and the theory, as shaped by his maturest thought, appears in the last edition of the 'Manual.' In his earlier writings his views of the origin of continents and mountains were developed on the assumption of a liquid globe. In later years he abandoned that view, and adjusted his theories to the more probable doctrine of a globe substantially solid.

The conception of the subsidence of the suboceanic crust, which led Dana to his views of the permanence of continent and ocean, is an important element in his theory of mountain-making. In the contraction of the earth's interior, the suboceanic crust necessarily flattens in its subsidence, so that its section continually approaches the chord of the arc, thus exerting a tangential thrust toward the continental areas. The rather abrupt change in the radius of curvature in passing from the oceanic to the continental areas, makes the continental borders lines of weakness which determine in general the location of the great mountain wrinkles. In the two continents of North and South America, with their mountain borders on the east and west and their vast interior plains, Dana found exemplified the typical continent. Their isolated situation seemed to allow a more typical development than was possible in the Old World, where continents are massed together; as the laws of crystalline form can exhibit themselves in perfection only where a single crystal in a solution or magma is allowed to grow without interference of other growing crystals.

According to the views of Dana and

Le Conte, mountain ranges have been only exceptionally developed by geanticlinal uplifts of the earth's crust. A mountain range, in general, has its birth in a geosyncline—a downward folding of the crust, forming a trough, in which sedimentation goes on *pari passu* with the subsidence. At length, after long ages of subsidence and sedimentation, the strata in the trough are crushed together into alternate anticlines and synclines, or one part is forced over another in great thrust faults, while slaty cleavage and more decided metamorphism may be produced. The mountain range thus produced Dana called a 'synclinorium,' while he suggested the name 'anticlinorium' for a mountain range formed by a permanent geanticlinal elevation. Apparently the actual history of most mountain ranges is complex. The Appalachian range, for instance, was formed as a synclinorium at the close of the Paleozoic, degraded nearly to base-level in Mesozoic time, and again elevated by a broad geanticlinal movement early in Cenozoic time. It represents, therefore, in its complex history, Dana's two types of the synclinorium and the anticlinorium.

There are unquestionably weak points in the theory of mountain-making as developed by Dana and Le Conte; and, in our ignorance of the conditions in the interior of the earth, and of the forces there in action, it ill becomes us to be dogmatic; but the contractional theory seems worthy of provisional acceptance as the most plausible explanation of orogenic movements yet suggested.

III. GLACIAL GEOLOGY

Surely no part of the world affords better opportunity for the study of glacial geology than America. Its ice sheet, four million square miles in area, far exceeds any of the ice sheets of the Old World. The imbricated sheets of till in the Mississippi Valley afford clear evidence of the complex series of glacial and interglacial epochs. The driftless islands in the vast area of till, and the interlobate moraines, show the division of the marginal portion of the ice sheet into lobes determined by the topography. The old beaches along the shores of the Great Lakes, the living Niagara, and the various extinct Niagaras, record the stages of the melting of the ice. The Malaspina Glacier affords illustrations of the formation of eskers and of other phenomena which must have marked the stagnant margin of the waning ice sheet. Surely our country affords most favorable conditions for the study of the history of the Glacial period.

It is a curious fact that the first published suggestion of the agency of ice in connection with the drift came from a cotton manufacturer in Connecticut, Peter Dobson by name. In the *American Journal of Science*, in 1826, he gives a very clear and satisfactory description of the glaciated boulders observed in the drift, and gives as his conclusion: "I think we can not account for their appearances unless we call in the aid of ice along with water, and that they [the boulders] have been worn by being suspended and carried in ice, over rocks and earth, under water." It was his idea that these boulders were lifted from the bottom of the sea by sheets of anchor ice. This theory was certainly more satisfactory than most of the theories in vogue before that time, and more satisfactory than many of the opinions held at a later date.

The credit of the introduction and championship of the glacier theory of the drift belongs, not to a native, but to an adopted citizen of this country. Louis Agassiz came to this country in 1846. In his early home in Switzerland, he had already adopted the belief of Venetz and Charpentier in the former great extension of the Alpine glaciers, and their agency in the transportation of blocks from the Alps across the Swiss lowland to the Jura. It is, indeed, a curious fact that these Swiss geologists were anticipated, in the conception of the transportation of these blocks through the agency of glaciers, by Playfair, who suggested the idea in 1802. In the early papers of Agassiz the conception of the Glacial period took a form which he himself later recognized as an exaggeration. He conceived at first of a fall of temperature so wide-spread and so extreme that a polar ice cap extended southward over the whole breadth of Europe and across the Mediterranean, reaching the Atlas Mountains. Later he recognized the ice sheet that covered the Alps as entirely separate from the ice sheet of northern Europe. The tendency to an exaggerated view of the Glacial period overcame him again in later years, when he maintained that, at the climax of the Glacial period, there was 'floating ice under the equator, such as now exists on the coasts of Greenland.' Incidentally, he based upon this extravagant conception of the Glacial period an argument against Darwin's views of the origin of species, maintaining that the wide-spread cold of the Glacial period produced a general extermination of life, necessitating a new creation. But the extravagance of some of his conceptions, and his vain attempt to stop the resistless progress of the doctrine of evolution, may well be forgiven, in memory of the great service which he rendered in bringing into general acceptance the glacier theory of the drift.

As Agassiz traveled in various parts of his adopted country, he recognized everywhere in the northern states the traces of glaciation, already familiar to him in Switzerland and in Scotland; and his views found more ready acceptance in this country than in some of the countries of Europe. Guyot, who had been associated with Agassiz in the study of the glaciers of Switzerland, came to this country in 1848; and he was, of course, a strong ally in the defense of the glacier theory. As early as 1841 Edward Hitchcock had been so strongly impressed by the writings of Agassiz that he recognized clearly the traces of glaciers in Massachusetts; and, in his presidential address before the Association of American Geologists, and in the postscript to his 'Final Report on the Geology of Massachusetts,' he shows himself almost persuaded to adopt the glacier theory for the explanation of the drift in general. He could not, however, quite bring himself to the acceptance of the conception of a glacier capable of moving with so little slope as to be able to transport material southward over the whole of northeastern America; and he accordingly limited the action of glaciers to those cases in which the drift appeared to be dispersed somewhat radially from local centers in mountainous or hilly regions. The general southward movement of the drift seemed to him to require the conception of submergence of the land, and transportation by icebergs floating southward from the Arctic regions. As early as 1852, one of the best and most popular text-books of the time, that of Gray and Adams, gave the preference to the glacier theory of the drift. Dana, in his presidential address before the American Association for the Advancement of Science, in 1855, manifestly inclined to the glacier theory; and, in the first edition of his 'Manual of Geology,' in 1862, he gave his adhesion more decidedly to that view. The next generation of American students of geology were brought up on the various editions of Dana's 'Manual' and 'Text-book,' so that thenceforward the glacier theory was recognized in this country as orthodox. In some of the countries of Europe the theory of submergence and transportation of boulders by icebergs held the ground longer. In 1864 Lyell still maintained the submergence of the plain of northern Europe, and of the glaciated region of North America; and it was not until 1875 that Torell, in a memorable meeting of the German Geological Society, convinced the German geologists that the drift of northern Germany was transported by an ice sheet whose center was in the mountains of Scandinavia, and the name Diluvium, though still used in German geological writings, was completely emptied of its original connotation.

Within the last few decades the labors of a large number of earnest and able investigators have developed the glacier theory more in detail, and have added vastly to our knowledge of Quaternary history. The imaginary polar ice cap has given place to ice sheets of more limited dimensions, though still vast, developed respectively about the Laurentide, Keewatin and Cordilleran centers. The series of terminal moraines, marking stages of readvance or halts in the retreat of the ice sheet, have been carefully mapped. From the experience gained in the study of terminal moraines in this country, Lewis was enabled, in 1886, to recognize and interpret the terminal moraines of the ice sheet in England; and Salisbury, in the following year, those of north Germany. The recognition of interlobate moraines and of driftless areas led to a clearer understanding of the nature of the movement of the great ice sheet. The driftless area of Wisconsin was noticed by J. D. Whitney as early as 1862, and has been more carefully studied by Chamberlin and Salisbury; and the latter investigator has called attention to a smaller driftless area in Illinois. The Quaternary period, instead of being brief and comparatively simple, has been shown to be of long duration and great complexity. It has been analyzed into a succession of glacial and interglacial epochs; and, from the vast amount of erosion in some of the interglacial epochs, it has been inferred that post-Glacial time is very short in comparison with inter-Glacial time. The history of the series of lakes held between the front of the receding ice sheet and the southern water shed of the Saint Lawrence basin, has been studied by Gilbert, Taylor, Fairchild and others. In Chamberlin's theory that the cause of the Glacial climate is primarily the diminution of the amount of carbon dioxide in the atmosphere, and that the location of the main centers of glaciation is due to the path of cyclonic storms, we have a theory which, if it can not be accepted with full confidence as the true one, is at least the only theory of the Glacial climate which has not yet been weighed in the balance and found wanting. There seems still to be some disagreement among physicists on the critical question, what effect small changes in the amount of carbon dioxide in the atmosphere would produce upon the climate. The views of Arrhenius, upon which the theory of Chamberlin is based, have been contested by Angström and Very, and are not accepted by Hann. If the physicists can be brought to an agreement on this fundamental point, we may feel that we have, at last, a theory of the Glacial climate. The alternative at present seems to be the acceptance of Chamberlin's theory, or the confession that we have as yet no explanation of the Glacial climate.

IV. SUBAERIAL DENUDATION AND THE EVOLUTION OF DRAINAGE SYSTEMS

In early years the study of geology in this country was substantially confined to the region east of the Mississippi; but, in due season, the weird and fascinating region of the Cordillera revealed itself to explorers and geologists. It is now more than half a century since American geologists began the study of that western wonderland. The earliest geological work was done in connection with expeditions undertaken for other purposes, as the Pacific Railroad explorations, which commenced in 1853, and Ives's Colorado expedition. It is, however, forty years since the national government established geological surveys in that western country. A period of a dozen years commencing with 1867 was marked by the achievements of four great organizations devoted specifically to geological work—the Survey of the Fortieth Parallel, the Survey West of the One Hundredth Meridian, the Survey of the Territories and the Survey of the Rocky Mountain Region. Since 1879 all these organizations have been superseded by the United States Geological Survey. A new world for geologists was that weird western land—that land of deserts, plateaus and cañons, with vast stretches of almost horizontal stratification broken by faults and monoclines, revealing its geological structure with wondrous clearness by reason of an arid climate whereby it has been left naked and destitute of any mantle of soil and vegetation. There is revealed, as nowhere else in the world, the power and the method of subaerial denudation. The Grand Cañon of the Colorado is a stupendous object lesson of erosion; and, if one is compelled to recognize river erosion in the cañon itself, scarcely less strenuous is the compulsion to recognize subaerial denudation in the vast platform of Carboniferous strata with little outlying buttes of Permian and later formations. The study of that country has been fruitful in its contributions to the knowledge of aqueous agencies in dynamical geology. Very early appeared those three monumental works: Powell's 'Exploration of the Colorado River,' Gilbert's 'Geology of the Henry Mountains' and Dutton's 'Tertiary History of the Grand Cañon District.' The value of those early studies in a new field is not greatly lessened if some conclusions must be modified by later study. It may be that the course of the Green River through the Uinta Mountains is not a typical case of antecedent drainage; and it seems certain that the esplanade in the Kanab section of the Grand Cañon is not due to a long pause in the movement of elevation, in which the river nearly attained base-level: but the conceptions introduced into dynamical geology as the result of those early studies are no less valuable and fruitful.

The first lesson that geologists learned in that western land was the efficiency of subaerial denudation—the power of atmosphere, rain and river to remove vast quantities of material, and shape the topography of wide areas. From Hitchcock's 'Elementary Geology' I learned in my boyhood, in regard to cracks and fissures in the strata, "If the fissure is open and of considerable width, it is called a gorge; if it be still wider, with the sides sloping or rounded at the bottom, a valley is produced." Hitchcock was convinced, indeed, that the Niagara gorge was due to erosion; but he declared that the gorges of the Connecticut between Mount Tom and Mount Holyoke, and below Middletown, could not be due to the action of the river, and that the gorges of the same river at Bellows Falls and Brattleboro were too wide to have been formed by the river alone. In his 'Illustrations of Surface Geology,' in discussing the criteria by which to distinguish fluviatile from oceanic work, he declared, "Rivers have little power to form wide valleys." Apparently geologists were confused with a vague idea that the ocean is bigger than the rivers, and that, therefore, it can do more work in erosion. As early as 1847, Ramsay had recognized that the summits of the mountains of Wales were remnants of a former plain of erosion, but he conceived of it as a 'plain of marine denudation,' and that phrase holds its place to-day in English writings. With genuine British loyalty and conservatism, Sir Archibald Geikie retains the phrase even in the latest edition of his 'Text-book,' in explanation of table-lands of erosion; though, even in his earliest edition, he declared that, in the production of plains of marine denudation, "the sea has really had less to do than the meteoric agents. A plain of marine denudation is that sea-level to which a mass of land has been reduced mainly by the subaerial forces." He attributes to ocean waves and currents only 'the last touches in the long process of sculpturing.' Elsewhere Geikie bears emphatic testimony to the influence of American geologists, in the words: "Unquestionably the most effective support to Hutton's teaching has been given by the geologists of the United States, who, among the comparatively undisturbed strata of the western territories, have demonstrated, by proofs which the most sceptical must receive, the potency of denudation in the production of the topography of the land." It is, indeed, marvelous with what prophetic vision Hutton and Playfair conceived some of those ideas of river action which the geologic world in general learned only from the work of American geologists in the Cordilleran region. On this subject, those two Scotchmen were a half-century in advance of their time.

That western land has taught us not only to recognize the fact of subaerial denudation, but also to formulate its methods. In Powell's 'Exploration of the Colorado River,' he distinguished rivers as consequent, antecedent, and superimposed. Davis has carried the analysis somewhat further, giving us subsequent and obsequent rivers. Powell formulated the doctrine of base-levels; Davis has given the conception greater accuracy and consistency by distinguishing base-level from profile of equilibrium. The base-level of a district is a portion of the ideal spheroidal surface of the earth. Disregarding the curvature of the earth's surface, base-level may be represented in profile as a straight line; profile of equilibrium as a curve, concave upward, tangent to base-level at the mouth of the river, gradually approaching base-level with the progressive denudation of the country, but never quite reaching it except at the point of tangency. To Davis also we owe the full development of the conceptions of youth and age in river valleys and in drainage systems, and of cycles of erosion ending in the formation of peneplains. We have learned to search in every rugged mountain region for remnants of ancient peneplains. We have learned, in general, that geological history is to be read, not only in deposits, but also in erosion forms.

V. Tertiary Mammals and the Doctrine of Evolution

Half a century ago the exploring expeditions connected with the Smithsonian Institution began to collect fossils from the Tertiary deposits of the western plains. Later the work was followed up by the Geological Surveys under the auspices of the national government, and by numerous private expeditions under the auspices of universities, scientific associations and individuals. Over those western plains were found to stretch vast continental deposits, certainly not all of lacustrine origin, as at first reported, but in part piedmont alluvial formations, in part eolian deposits, and, in limited areas, deposits of volcanic dust. These continental deposits of the western plains yielded in unparalleled richness mammalian fossils, which have been studied by Leidy, Marsh, Cope, Osborn, Scott, Wortman, and others. No other single series of discoveries has been so potent in changing the bearings of paleontology upon the doctrine of evolution.

In Darwin's two chapters on geology in the 'Origin of Species,' he marshaled with great skill the geological facts then known which appeared favorable to evolution. Yet he recognized in the facts of paleontology 'perhaps the most obvious and serious objection which can be urged against my theory.' He cited a long list of recognized authorities in geology and paleontology, still living or recently dead at the time of the publication of his first edition, who were believers in the immutability of species—Cuvier, Agassiz, Barrande, Pictet, Falconer, Forbes, Lyell, Murchison, Sedgwick. Of these Lyell alone lived to become a convert to evolution.

Of course the objection which Darwin felt so strongly himself, and which seemed conclusive to so many paleontologists at the time, was the absence of gradation between different forms. The theory of evolution, and especially the strictly Darwinian form of that theory, requires fine gradation between species—not indeed between different species now existing, but between existing species and species now extinct, and between fossil species of successive periods. In general, such gradations do not appear. Fossil species are about as sharply defined as recent ones; and whole groups of species—orders, classes, sub-kingdoms—have appeared without recognizable ancestry. Darwin's answer to this objection was given in the phrase now become classical, 'the imperfection of the geological record.'

In the half-century since the publication of Darwin's first edition, the attitude of paleontologists has completely changed. Not only is it true at present that paleontologists are substantially unanimous in accepting the doctrine of evolution; but it has come to be generally believed that the very science which afforded a half-century ago the strongest objection to evolution now affords its strongest support. This change is in large part due to the discoveries which have so shattered the objection that once appeared so strong. Innumerable links then missing have been brought to light. Intermediate forms between orders and classes formerly supposed to be widely separated from each other have been discovered in great abundance. Numerous series of genera may be traced through successive geological periods, exhibiting a gradually progressive change which almost irresistibly suggests to the mind the belief that the series are truly genetic. The fossils of our western plains have afforded a goodly share of the most important of these new evidences of evolution.

When the first edition of the 'Origin of Species' was published, the classes of birds and reptiles seemed to stand widely asunder. But in the very next year (1860) an odd feather of Archæopteryx was discovered, and a year later the skeleton now preserved in the British Museum. But Archæopteryx was a solitary representative of the birds of markedly reptilian character until the discovery of Ichthyornis and Hesperornis in the Cretaceous of Kansas, of which preliminary descriptions were published by Marsh in 1872. Both these remarkable types show reptilian affinities, in the possession of teeth, in the structure of the skull (though unhappily the palatal region is but imperfectly known), and in the pelvis; and Ichthyornis very notably in its slightly biconcave vertebræ, contrasting strongly with the saddle-shaped articulating surfaces of the vertebræ of modern birds. However strongly these genera suggest the idea of an evolutionary connection between reptiles and birds, their own place in the evolutionary series is not easy to determine. Ichthyornis may be in or near the direct line of descent from Archæopteryx to some such generalized dromæognathous type as is represented by those curious living fossils, the Crypturi, from which divergent lines of evolution may have led, on the one hand, to the ostriches and other flightless Dromæognathæ, and, on the other hand, to the Carinatæ. Hesperornis, a degenerate and in some ways highly specialized form, stands certainly at the end of a side branch, and has left no descendants.

But the discoveries of most evolutionary significance, as already intimated, have been among the Tertiary mammals. A number of series have been traced, leading from generalized types in the Eocene, through forms of gradually increasing specialization, to genera which still survive. The first of these genetic series to be brought to notice was the genealogy of the horse, as traced by Marsh in 1874. Marsh's views were adopted by Huxley in his brilliant 'American Lectures,' and thereby gained a larger share of public attention than they would otherwise have received. Probably no single fact or group of facts brought to light since the appearance of the 'Origin of Species' has been so influential in bringing the theory of evolution into general acceptance. The genealogy of the horse has been corrected in detail and completed by later investigations. The line of descent may now be traced through Hyracotherium and Eohippus of lower Eocene, Protorohippus and Orohippus of middle Eocene, Epihippus of upper Eocene, Mesohippus of Oligocene, Anchitherium of lower Miocene, Parahippus, Protohippus, and Pliohippus of middle and upper Miocene, to Equus of Pliocene and Quaternary; while side branches lead to Hipparion, Hippidium, and other forms which have died without issue.

A similar series, though with not quite so fine gradations, reveals the genealogy of the camel. From the Eocene Protylopus this line is traced, through Oligocene Poebrotherium, to Pliocene Procamelus, whence one branch leads to Camelus, and the other to the South American Auchenia. It is indeed remarkable that the characteristically old-world types, Equus and Camelus, should have been evolved in North America and have become extinct in this their original home. Another series, beginning in the lower Eocene Systemodon, ends in the modern tapirs.

In like manner, among the very primitive carnivores which have been classified as the order Creodonta, the ancestors respectively of the dog and the cat have been recognized in the Eocene genera Vulpavus and Palæonictis.

Of extraordinary interest in an evolutionary point of view is the most primitive Tertiary fauna from the Puerco beds discovered by Cope in 1880. In that fauna is found the culmination of the Multituberculata, which made their first appearance in Triassic time, and whose teeth reveal their close relation to the Monotremata. But, with those survivals from Mesozoic time, appear generalized and primitive forms of placental mammals, wherein may be traced the ancestry of mammalian groups of later Tertiary and recent time. Hemiganus and Psittacotherium may be recognized as the ancestors of the Edentata. And, among the most primitive and generalized ungulates, the Phenacodontidæ, Protogonodon has been recognized as possibly the ancestor of the Artiodactyla, and Euprotogonia with more probability as the ancestor of the Perissodactyla.

In the American Museum, where some of our sessions are to be held, the evidence of these genetic series running through the Tertiary is placed before our eyes with incomparable fullness. As we behold the unparalleled richness of those collections, we do not wonder that not even a call to be the official head of the army of science in this country could induce Professor Osborn to leave the museum which is at once the monument of his work in the past and the material for his work in the future.

But, however numerous are the gradational forms which have been brought to light, Darwin's principle of the imperfection of the geological record is in no wise superseded. It still remains true that the theory of evolution must stand or fall according to our judgment of the adequacy of that principle of Darwin. If the fossils accessible to observation and collected in our museums afford an approximately complete representation of the life that has existed in past ages, there is certainly no standing ground for any theory of evolution. But, while we have seen numerous chasms bridged by series of gradational forms, we have also come to a fuller appreciation of the significance of Darwin's principle of the imperfection of the geological record.

In the conclusion of Darwin's chapter on the subject, he used a striking illustration: "I look at the natural geological record as a history of the world imperfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating only to two or three countries. Of this volume, only here and there a short chapter has been preserved; and of each page, only here and there a few lines." In the light of our present knowledge of geological history, we are able to see that even this striking illustration fails to do full justice to the subject. The imperfection of the record consists not merely in the fact that some of the chapters are missing. It appears most strongly when we inquire just what chapters are missing. In the conception of continental history to which I have already referred, we have come to recognize a truth which, in somewhat distorted form, found expression in the old catastrophism. We have come to recognize that comparatively short periods of rapid geographical change alternate with long periods of relative stability or slowly progressive change. This is, in substance, the doctrine of critical periods as formulated by Le Conte. It is precisely in those critical periods that the record fails, and the gap is indicated by unconformability. Darwinians and Lamarckians alike must recognize that the periods of rapid geographical change must be the periods of most rapid change in fauna and flora. Evolutionary change must be directly or indirectly the result of a failure of adjustment between organism and environment.

The doctrine of critical periods has taken somewhat more definite form in Chamberlin's discussion of the effects of intermittent subsidence of the ocean bottom. The critical periods in geological history are the times when the rigidity of the crust yields to accumulating strain, when the ocean bottom subsides, when the continents emerge to larger area and higher altitude, when more or less of mountain-making takes place, and when the geographical changes bring in their train the diminution of the atmospheric supply of carbon dioxide and a tendency to cold and arid climates. Then come the long periods in which the continents are slowly denuded, the continental shelves are extended landward by encroachment of the sea and seaward by sedimentation, the quota of carbon dioxide is slowly replenished, and the fauna and flora which had been impoverished gradually expand to their former luxuriance. The chapters which are lost from the record are precisely the chapters which would contain the story of those critical periods, marked by extinction of manifold species, and by rapid change in adjustment to new and more rigorous conditions. The geological record of the progress of life is like a history of the United States, in which, among other less important chapters, the chapters on the Revolution and the Civil War are lost.

CONCLUSION

I have not attempted to give a history of geological investigation in this country. Of the great number of earnest and able investigators whose names illustrate the scientific history of this country—of those who have finished their work, but whose memory and influence can never die—of those still living whose achievements in the past are only the promise of greater work in the future—I have named but few, though many others are equally worthy. Of the men whose names I have mentioned, I have doubtless not in all cases mentioned the work which has been most meritorious or important. I have mentioned only those investigations which have a bearing on a few special subjects. Nor have I referred, except occasionally and incidentally, to the work of European students which has gone on parallel with that of students in our own country. American geologists have had no patent rights giving them a monopoly of any particular department of investigation. The limited time of such an address as the present renders impossible a critical discussion of the precise share in the study of the various subjects which belongs to American geologists. But I believe it may be fairly claimed that on the five subjects which I have discussed—the permanence of continents, the theory of mountain-making, the history of the Glacial period, the laws of subaerial denudation, the evolution of mammalian life—the work of American geologists has been relatively so important that the results deserve recognition as, *par excellence*, The contributions of America to geology.

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SCIENTIFIC BOOKS

The Development of Symbolic Logic: A Critical-Historical Study of the Logical Calculus. By A. T. Shearman, M.A. London, Williams and Norgate. 1906.

As the subtitle indicates, the author has attempted a history of symbolic logic accompanied by a critical examination and estimate of the various systems as they may have contributed severally to the discipline from its earliest stages to the present time. He claims that in spite of the great variety of systems and methods there is clearly to be recognized but one logical calculus, and that the unity among the various symbolists abundantly compensates for the obvious differences. While this is true it should not be overlooked, however, that the progress of symbolic method has been retarded owing to the lack of a common symbolism such as we find in mathematics. The variety and the multiplicity of symbolical representation is, in my opinion, a serious defect. It is not merely that different writers are using different methods of symbolism—that in itself is sufficiently confusing—but also that any new operation is apt to give rise to some entirely new form of symbolism which might be represented equally as well by some new combination or new manipulation of the existing symbols already at hand. Within the scope of a few elementary symbols an indefinite range of differing processes and devices is possible, just as in mathematics the symbols used are exceedingly few—but they lend themselves easily and adequately to the exact expression of an innumerable array of operations and processes. The desideratum in a symbolic logic is, therefore, twofold: a common and a simplified symbolism. The simplicity of the symbolism of Leibniz, the founder of symbolic logic, is most striking; but the drift has been from this characteristic simplicity towards increasing difference and complexity. The author, by the way, does not give Leibniz his full due as the founder of the symbolic logic. Mr. Shearman insists that Boole is to have the complete credit of this on the ground that Boole worked independently and without any knowledge of the early work of Leibniz. The latter assumption seems somewhat gratuitous, at least Mr. Shearman cites no authority for his statement. Be that as it may, it is true that Leibniz first suggested the problem of the symbolic logic, although he did not attain the solution of it. How often, however, the pioneers in a new field of thought have merely started inquiry without achieving the reward of discovery.

In reference to the problem of the symbolic logic which has been mentioned above, it might be a matter of general interest to the lay reader to learn somewhat more explicitly as to the characteristic features which it presents. The problem in the main is this: to devise a method by which any given relations expressible by symbols may be made to exhibit the full range of possibilities which these relations imply both affirmatively and negatively —that is what they render necessarily true or necessarily false. Moreover, there has been a constant endeavor through the whole development of symbolic logic to make such a method as general as possible. The generalizing of method, indeed, has been one of the chief characteristics of this development. For instance, the earlier symbolists dealt almost exclusively with logical classes; the later, as Frege, Peano, MacColl and others, extended their method so as to include propositions, and to represent every other relation as well as the ordinary relation of logical subsumption; the later symbolists also endeavor to embrace in their method the quantitative as well as the qualitative relations. In this connection I am constrained to refer to what may be called a fetich of symbolic logic and which has proved a snare to many. It is the notion that by representing certain ideas by symbols —the ideas themselves for a time being thus placed in the background—the merely formal processes of the accepted logical operations will disclose some entirely new relation of the symbols employed, which being reinterpreted in terms of the original ideas will reveal a new significance never before conceived. This is a vain delusion, for it labors under the misapprehension that there is something mysterious about certain formal processes by virtue of which new material content will be revealed. It has been urged that scientific discoveries of some moment may be stumbled upon merely by following out the subtle workings of formal processes whose significance can be appreciated only when such processes have been finally completed. It is as though the stream of reason was able to cut out certain subterranean channels to emerge again into the light of day. Leibniz had this idea as a kind of a will-o'-the-wisp, in his 'Characteristica Universalis' by which he thought that formal rules might take the place of brains and the conscious processes of thought. "If we had it," he says, "we should be able to reason in metaphysics and morals in much the same way as in geometry and analysis" (G. vii. 21). "If controversies were to arise, there would be no more need of disputation between two philosophers than between two accountants. For it would suffice to take their pencils in their hands, to sit down to their slates, and to say to each other (with a friend as witness, if they liked): Let us calculate" (G. vii. 200). The practical utility of the symbolic logic is not, however, in the direction of the discovery of new possibilities never before conceived, but rather in the line of providing a method by which every possibility is embraced in one comprehensive survey. In a field of complex relations it is very easy to overlook one and another of the many possibilities, and a method is valuable both theoretically and practically which provides that no single possibility can escape the attention, and which thus shows that logical implications are both manifold and complex. It is a question largely of whether every possibility has been brought to the attention of the observing mind, and not whether a possibility can be discovered by certain logical processes as an entirely new result never before imagined. Mr. Shearman cites as an example of a new truth discovered by calculation that of the existence of the planet Neptune by Adams and Leverrier. It must be remembered, however, that each one of these men in his calculation had clearly before him from the beginning the desired end which he expected his calculations to prove. In other words, these men were not working in the dark, simply trusting themselves to a process which might be leading them any whither.

This volume contains an excellent historical sketch of the various systems of symbolic logic and as such is a most valuable book of reference. To read it with profit, however, some knowledge of the several systems is necessary. It would have been a manifest advantage had the author given, for instance in his chapter on The Process of Solution, a more detailed and elementary account of the original method of Boole, or of the method of Venn, being as it is a developed form of the Boole method. Thereby the difficulties for the lay reader would have been overcome to a great extent.

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Thought and Things, or Genetic Logic. Vol. I. *Functional Logic.* James Mark Baldwin. London, Swan, Sonnenschein & Co.; New York, The Macmillan Co., 1906. Pp. xiv + 273.

This is the first of three volumes, appearing in both French and English, on a subject never before given comprehensive systematic treatment. It brings into use a somewhat unusual terminology. The terms pragmatelic, semble, sembling, autotelic, heterotelic, syntelic, psychonomic, autonomic, heteronomic, syndoxic, progression, mode, schema are used with restricted although clearly defined meanings, and we might add to the list. Some readers will wish for more elucidation and continuity in places and there are some passages whose meanings are rather elusive. But the methodological difficulties of the subject are unusually great and have been handled with a remarkable degree of success. The author's evident interest in the subject itself, rather than in the style of the discussion, is neither surprising nor reprehensible.

The author realizes that the title, Genetic Logic, is likely to provoke criticism (pp. vii, 18). We should say the place of genetic logic among the philosophical disciplines is not unlike that of sociology among the social sciences. Sociology is neither history, economics, psychology nor anthropology and some whose contributions to these subjects give them a right to speak say there is no separate science of sociology. Genetic logic is neither straight logic, straight psychology, nor straight epistemology. Logic is not genetic, psychology is not interested in questions of logical validity, and epistemology, although broader and more elastic than the other two, does not involve as much psychology as this book presents. And yet the author is correct in assuming that the problems here discussed are real and pressing. They are not new. The time seems to have come for a systematic presentation of all this material and it is to be hoped that this is only the first of several works on the subject. It is a colossal task for which the philosophical *Zeitgeist* has been long in training, and one can only praise the keenness and comprehensiveness of this treatment.

The author confines himself to the same rules of observation and hypothesis as those observed in 'the empirical sciences generally' (p. 9), asking the questions—How? and Why?—as well as the question—What?—with reference to each form and mode of knowledge. Neither the formal logician's logic, nor the metaphysician's 'logicism,' will concern us here, but rather the knower's logic, cognitive processes viewed from the knower's standpoint. We are to study the genesis of knowledge and thought, and construct a genetic theory of reality. The former topic is discussed in the first two volumes, the latter, in volume third. The first volume presents a genetic theory of what the author calls the pre-logical cognitive functions, the second, a genetic theory of thought and judgment, and the third, a genetic theory of real logic or the 'hyper-logical functions' (p. 15). 'Genetic psychology of cognition' and 'genetic epistemology' (p. 18) are other names for the two main topics of the work. It seems to us that 'genetic epistemology' would be a good title for the entire work.

The author asks (1) 'what are the conditions determining the construction of objects at any given stage of mental development, and (2) what are the psychic characters of the objects thus determined' (p. 30). He distinguishes "in the actual results to which the research has led, the following phases of consciousness traced in each case along with the objects through a series of modes: (1) The controlling conditions of the determination (that is, the control of the object); (2) the motive to each of the determinations (the problem of interest as 'practical,' or 'theoretical,' or other); (3) the function involved in each determination; (4) the meaning of the object over and above its actual objective marks' (p. 30 f.). On page 34 seven sorts of objects are distinguished and defined as follows. (1) The projective object of sense, (2) the image object (of memory and fancy), (3) the make-believe object ('the first determination of the semblant object'), (4) the substantive object (either mind or body), (5) object of experience (object to a subject), (6) judged or logical object (an object of experience which the psychic subject as such is aware that it is in some sense acknowledging or controlling), (7) the esthetic object (of higher semblance in which the dualism of inner and outer controls is annulled in a state of immediate contemplation). Corresponding to these are seven modes, namely, sense, image, play, substantive, subject, logical, æsthetic (p. 32).

A criticism suggests itself here. This is a work on the knower's logic, but the first four 'objects' in this list are not objects to the knower. They are objects only to the psychological observer who analyzes. They are (to use words which the author uses to characterize a different subject) 'abstract meanings of our reflection, that is meanings only to a consciousness that can have an object that means this.' This suggests another fundamental objection, namely, to the author's treatment of mere presence in consciousness as knowledge, 'projective cognition.' This treatment leads to setting up a 'subject,' or a 'consciousness,' or etc., as the other term of the cognitive relation, and gives us an epistemological dualism for which there is no solution.

This epistemological dualism runs through the entire book and is especially evident in Chapter III., on How Knowledge is Made. Here a system of 'grasping' and 'habitual dispositional processes' is set over against an extra-psychic stimulus which, in some cases, 'is not content with knocking down our fortifications,' but 'rides full-armed through our walls, and compels its recognition in certain of its characters *for what it is'* (p. 50, footnote). This 'outside world' whose impacts (p. 54) are represented in consciousness by the 'sense residuum or datum' is as much a thing-in-itself as Locke's outside world or the explanation of our modifications of sensibility in Kant's epistemology. If the author means that this dualism of controls is merely psychological, the uncritical dualism of naïve consciousness and one which he does not regard as real or valid, we should say it is a needlessly artificial way of describing the content of consciousness, and also that it is misleading to cite and criticize in this connection an epistemological doctrine of control such as that of Dewey.

In Chapters VII., VIII. and IX., on Meaning, the limiting and negative sense datum is defined as a 'meaning,' and on page 172, 'meanings arise as variations which presented complexes take on for the satisfaction of varying dispositions.' But how is this to be reconciled with the doctrine of a 'foreign' sense-datum, and with the general doctrine of 'outer control'? At the conclusion of the book the author claims to have found that there is no sort of discontinuity or dualism between prelogical function and thought, and that the positive dualism is one within the operation of the developing function of cognition, the dualism of meanings (p. 272). A consummation devoutly to be wished at the present, but what shall be said of the foreign warrior who 'rides full-armed through our walls'?

In a foot-note on page 50 the author seems to ascribe to Dewey an epistemological dualism of which we understand the latter to be the sworn enemy. We may be in error as to the teachings of both men, but so far as they discuss the same aspects of the subject at all we find many points of similarity between the author's doctrine of control (and also his outlined experimental logic) and the teachings of Dewey. The latter holds that *to the knower* control always means objectivity, while Baldwin teaches a dualism of subjective and outer controls; but in other respects they are not so far apart.

Play, sembling and experimentation are central in this theory of knowledge. In play we semble, that is, we treat an object which we have invented, one 'freely' determined by 'subjective control,' as though it possessed certain coefficients of reality which it lacks. By experimentation we test these play constructions and find that they are either mere fancies (belonging to the inner world of subjective control) or else sense objects (possessing universality and belonging to the outer world of foreign control). Thus play and experimentation, leading to judgment, mediate between the 'inner' and the 'outer,' between 'subjective control' and 'outer control.' The author does not refer to language, or to sympathy, imitation, jealousy, bashfulness, gregariousness and other instinctive or impulsive reactions which involve social situations. Why should play be singled out as the only impulsive reaction contributing to the development of judgment? Universality is not involved in mere play because (1) few can enter into a game and (2) both the objects and the self of play are tentative and fictitious. Mere semblance is not characteristic of objects of knowledge as such. In short, we find a gap between play and experimentation, between sembling and judging, which the book has not filled, the gap between perception and conception, between sense and reason, between mere sentience and reflection. The author's theory seems to lead to the doctrine that facts are all 'outer'—that they are ultimately trans-objective—while meanings and values are all subjective (see pp. 135 f.), and judgment must perform the miracle of joining them. Beyond this difficulty, the author's dualism of subjective and outer controls would make genuine experimentation and judging impossible. These brief critical suggestions, of course, need elaboration, but the reviewer's respect for the writer's results as well as his sense of the importance of this discussion incline him to let them stand.

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THE WAYS OF SHEEP.

The Flock. By MARY AUSTIN. Pp. 266, illustrated. Houghton, Mifflin and Company. 1906.

Scientific observation as conducted by scientific men is rigorous, repeated and checked by the ingenious employment of experimental control of conditions. Observation by the nature lover may not be so guarded and tested. And every publication by word of mouth or impress of type of obviously mistaken record of seeing or of misinterpretation of the really seen that comes from the nature lover confirms the rigorous-minded scientific man in the belief that only his sort of observation reveals the truth. Hence we do not search literary books for contributions to science: which is a habit of omission that may lose to us some valuable data.

Mary Austin, an author known especially to readers of the *Atlantic Monthly* and to those generally who seek to acquaint themselves with the better sort of American writing, has included in 'The Flock' a host of singularly interesting and suggestive observations on the ways of sheep. The author has lived near (in more ways than one) sheep and sheep dogs and sheep men for seventeen years, and is a keen and careful observer and an honest and gifted recorder of her observations. Hence 'The Flock' is a book which the driven scientific man may read for recreation and information at once. How unusual!

I shall take space to refer to but two or three of Mrs. Austin's observations or summations of observation. The 'mob mind' of sheep is a very real thing in determining the ways of the flock. In the flock

there are always leaders, middlers and tailers, each insisting on its own place in the order of going. Should the flock be rounded up suddenly in alarm it mills within itself until these have come to their own places.

Suppose the sheep to scatter widely on a heather-planted headland, the leader feeding far to windward. Comes a cougar sneaking up the trail between the rooted boulders toward the meanest of the flock. The smell of him, the play of light on his sleek flanks startles the unslumbering fear in the meanest; it runs widening in the flock-mind, exploding instantly in the impulse of flight.

Danger! flashes the flock-mind, and in danger the indispensable thing is to run, not to wait until the leader sniffs the tainted wind and signals it; not for each and singly to put the occasion to the proof; but to run—of this the flock-mind apprizes—and to keep on running until the impulse dies faintly as water-rings on the surface of a mantling pond. In the wild pastures flight is the only succor, and since to cry out is to interfere with that business and draw on the calamity, a flock in extremity never cries out.

Consider, then, the inadequacy of the flock-mind. A hand-fed leader may learn to call the herder vociferously, a cosset lamb in trouble comes blatting to his heels, but the flock has no voice other than the deep-mouthed pealings hung about the leader's neck. In all that darkling lapse of time since herders began to sleep with their weapons, affording a protection that the flock-mind never learns to invite, they have found no better trick than to be still and run foolishly. For the flock-mind moves only in the direction of the original intention. When at shearings or markings they run the yearlings through a gate for counting, the rate of going accelerates until the sheep pass too rapidly for numbering. Then the shepherd thrusts his staff across the opening, forcing the next sheep to jump, and the next, and the next, until, Jump! says the flock-mind. Then he withdraws the staff, and the sheep go on jumping until the impulse dies as the dying peal of the bells.

Have sheep inherited acquired characters of habit or increased in this way their mental equipment? Not according to what has just been written. But consider this:

I do not know very well what to make of that trait of lost sheep to seek rock shelter at the base of cliffs, for it suits with no characteristic of his wild brethren. But if an estray in his persistent journey up toward the high places arrives at the foot of a tall precipice, there he stays, seeking not to go around it, feeding out perhaps and returning to it, but if frightened by prowlers, huddling there to starve. Could it be the survival, not of a wild instinct—it is too foolish to have been that—but of the cave-dwelling time when man protected him in his stone shelters or in pens built against the base of a cliff, as we see the herder yet for greater convenience build rude corrals of piled boulders at the foot of an overhanging or insurmountable rocky wall? It is yet to be shown how long man halted in the period of stone dwelling and the sheep with him; but if it be assented that we have brought some traces of that life forward with us, might not also the sheep?

But, from the other side, consider this:

Where the wild strain most persists is in the bedding habits of the flock. Still they take for choice, the brow of a rising hill, turning outward toward the largest view; and never have I seen the flock all lie down at one time. Always as if by prearrangement some will stand, and upon their surrendering the watch others will rise in their places headed to sniff the tainted wind and scan the rim of the world. Like a thing palpable one sees the racial obligation pass through the bedded flock; as the tired watcher folds his knees under him and lies down, it passes like a sigh. By some mysterious selection, it leaves a hundred ruminating in quietude and troubles the appointed one. One sees in the shaking of his sides a hint of struggle against the hereditary and so unnecessary instinct, but sighing he gets upon his feet. By noon or night the flock instinct never sleeps. Waking and falling asleep, waking and spying on the flock, no chance discovers the watchers failing, even though they doze upon their feet; and by nothing so much is the want of interrelation of the herder and the flock betrayed, for watching is the trained accomplishment of dogs.

Our 'amelioration' of the sheep has certainly lost them much—even though we have gained. This is, of course, the familiar story of artificial selection. It is chiefly artificial degradation. Mrs. Austin records this:

Of the native instincts for finding water and knowing when food is good for them herded goats have retained much, but sheep not a whit. In the open San Joaquin, said a good shepherd of that country, when the wind blew off the broad lake, his sheep, being thirsty, would break and run as much as a mile or two in that direction; but it seems that the alkaline dust of the desert range must have diminished the keenness of smell, for Sanger told me how, on his long drive, when his sheep had come forty miles without drink and were then so near a water-hole that the horses scented it and pricked up their ears, the flock became unmanageable from thirst and broke back to the place where they had last drunk.

And this:

Sheep will die rather than drink water which does not please them, and die drinking water with which they should not be pleased. Nor can they discriminate in the matter of poisonous herbs. In the northerly Sierras they perish yearly, cropping the azaleas; Julien lost three or four hundred when wild tobacco (*Nicotiana attenuata*) sprang up after a season of flood water below Coyote Holes; and in places about the high mountains there are certain isolated meadows wherein some herb unidentified by sheepmen works disaster to the ignorant or too confiding herder. Such places come to be known as poison meadows, and grasses ripen in them uncropped year after year.

What have the sheep come to know of man in their fifty centuries' association with him? Mrs. Austin answers:

It is doubtful if the herder is anything more to the flock than an incident of the range, except as a giver of salt, for the only cry they make to him is the salt cry. When the natural craving is at the point of urgency they circle about his camp or his cabin, leaving off feeding for that business; and nothing else offering, they will continue this headlong circling about a boulder or any object bulking large in their immediate neighborhood remotely resembling the appurtenances of man, as if they had learned nothing since they were free to find licks for themselves, except that salt comes by bestowal and in conjunction with the vaguely indeterminate lumps of matter that associate with man. As if in fifty centuries of man-herding they had made but one step out of the terrible isolation of brute species, an isolation impenetrable except by fear to every other brute, but now admitting the fact without knowledge, of the God of the Salt. Accustomed to receiving this miracle on open boulders, when the craving is strong upon them they seek such as these to run about, vociferating, as if they said, In such a place our God has been wont to bless us; come now let us greatly entreat Him. This one quavering bleat, unmistakable to the sheepman even at a distance, is the only new note in the sheep's vocabulary, and the only one which passes with intention from himself to man. As for the call of distress which a leader raised by hand may make to his master, it is not new, is not common to flock usage, and is swamped utterly in the obsession of the flock-mind.

Then there are the sheep dogs, those wolves that we have ameliorated to protect the sheep from other wolves. But our space prevents even a tasting of the interesting notes on dog ways that the book offers. Let us but just note the strong insistence of our author observer that dogs are not bred sheep dogs, but trained to be sheep dogs. "What good breeding means in a young collie is not that he is fit to herd sheep, but that he is fit to be trained to it." Rather against the inheritance of acquirements, this.

There is much keen observation, much shrewd suggestion, and no end of delight in 'The Flock.' And trained in the scientific method or not, Mrs. Austin is honest and truthful as one may be. That is, she tells only what to her eye and ear and mind comes with the seeming of truth. No rigorous scientific pundit can do more. For truth is for any of us too often at the bottom of the well.

VERNON L. KELLOGG

STANFORD UNIVERSITY,
CALIFORNIA

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

THE 185th meeting of the society was held on January 9, 1907, with President Lindgren in the chair and fifty-four persons present.

Regular Program

The Paleozoic Section of the Upper Yukon, Alaska: ALFRED H. BROOKS and E. M. KINDLE.

This paper was presented by Mr. Brooks, who, in company with Mr. Kindle, devoted part of the field season of 1906 to a detailed study of the rocks exposed between the international boundary and Fort Yukon, along the banks of the Yukon and Porcupine Rivers.

The total thickness of Paleozoic strata, comprising this section, is estimated to exceed 15,000 feet, but as the bottom was not determined, it may be very much greater. *The lowest member* of the succession is a series of quartzites with intercalated limestones and shales, which is well developed on the Porcupine near the international boundary. Provisionally, at least, these rocks may be correlated with the more highly altered rocks called the Birch Creek schists which occur in large areas south of the Yukon. On the Porcupine these rocks are comparatively little altered and intrusives appear to be entirely absent, except for occasional small dikes, while to the south the same series is much metamorphosed, sometimes to complete recrystallization, and intrusives are abundant. *The next member* is made up of limestone which on the Porcupine is about 600 feet thick and carries Ordovician fossils. This terrane comprises bluish-gray, non-magnesian limestones, distinguishable lithologically from the buff dolomitic series which follows them, but has not been definitely recognized in other parts of the province. The relation of these Ordovician beds to the older rocks is probably one of unconformity.

The Silurian is represented by at least 2,500 feet of magnesian limestone with some quartzites and slates, which are probably conformable to the Ordovician. Its correlation with the Fortymile series of Spurr is suggested but not proven. When *the Devonian* is considered, the problem becomes more complex, as no complete section of these rocks has yet been found. In general terms it can be said to be made up of shales, slates and cherts, with some limestones, while locally ancient lava flows and tuffs dominate over the sediments. On the Yukon the igneous rocks are very much more abundant than the sediments, while on the Porcupine the sediments are most abundant. The thickness is much in doubt, but can not be less than 3,000 feet and may be twice as great. The relation of the Devonian to the Silurian is unknown, but is probably one of unconformity. A black shale and slate bed some 1,300 feet thick constitutes the upper member of the Devonian and within this formation is probably the dividing line between the Devonian and the Carboniferous.

In this province the Carboniferous embraces about 4,000 feet of sediment, usually characterized by an absence of igneous rock. Overlying the shale mentioned above is about 1,000 feet of a thin-bedded limestone and shale series which carries Carboniferous fossils which have not yet been studied in detail. After these limestones and shales were laid down, the character of the sediments changes abruptly, indicating possibly a period of erosion. A heavy bed of chert conglomerate forms the basal member of the higher series and this is succeeded by shales, sandstones and conglomerates with an aggregate thickness of 1,600 feet. A limestone bed some 200 feet thick constitutes the youngest member of the Carboniferous. This is overlaid in different parts of the province by younger terranes of various ages. On the Porcupine the lowest member of the Devonian is a limestone 300 feet thick, while olive and chocolate-colored shales, in part fossiliferous, appear to comprise a large share of the Middle Devonian sediments.

The Stratigraphic Succession in the Region Northeast of Cook Inlet, Alaska: Sidney Paige and Adolf Knopf.

In this communication the combined results of the field studies of this region in 1906 were presented by Mr. Paige. The rocks exposed in this geologic section were found to range in character from garnetiferous mica schists of probable pre-Silurian age to unconsolidated Pleistocene stream and glacial gravels. Jurassic strata are very extensively developed and are divided into two unconformable formations, the lower of which consists chiefly of andesitic breccias and amygdaloids, 1,000 feet in thickness and overlain by graywackes, shales, sandstones and conglomerates. Marine shells, found both in the tuffs and in the graywackes, fix its age as Lower Middle Jurassic. The younger Jurassic rocks consist of shales, arkose, sandstones, conglomerates with some interstratified tuffs. The age indicated by fossil evidence ranges from the upper part of the Middle Jurassic through the Upper Jurassic. Conformably overlying these rocks is a Lower Cretaceous limestone 300 feet thick.

The Upper Eocene (Kenai) rocks of this section are freshwater sediments, consisting of shales, sandstones and conglomerates folded and carrying workable coal beds, of a maximum thickness of 17 feet. Their total thickness is not less than 3,000 feet; their age was determined on the fossil flora collected. Basaltic lavas and tuffs overlie the older rocks. They attain a thickness of 1,000 feet and form the summit topography of the region.

The Volcano Aso, in Kiushiu, Japan: ROBERT ANDERSON.

The volcano Aso-san is situated in the central part of the island Kiushiu in southern Japan. It has an active, modern crater a mile and a half in circumference which has been in eruption frequently during the past century, but its chief geological interest lies in the fact that it has an ancient crater ten or eleven by fourteen miles in diameter, larger than any other known crater. The crater basin, occupying an area of about one hundred square miles, is contained within sheer walls of andesite that rise on the average to a height of two thousand feet above the floor. A range of volcanic mountains, probably of subsequent origin, extends diametrically across the old crater and rises out of it over four thousand feet to a maximum altitude of five thousand six hundred feet above the sea. The vast crater of Aso-san is thought to have been formed either by the blowing off of the overlying mountain mass, or by the subsidence to a depth of three thousand feet or more of the oval region now occupied by the great bowl.

FRED E. WRIGHT,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 623d meeting was held on November 24, 1906, President Abbe in the chair.

Mr. Bigelow presented a request from a central committee in Geneva that this society approve and use in its publications the auxiliary language *Esperanto.*

Mr. Cyrus Adler presented a very interesting and felicitous memorial address on the late Professor Samuel P. Langley; his most notable services were, the establishment of uniform time, the study of the sun and its infra-red spectrum, and the researches in aerodromics, which for the first time 'made the subject respectable.'

Mr. F. W. Clarke spoke on 'The Quadri-Centennial at Aberdeen,' at which he represented the Smithsonian Institution, describing briefly the magnificent new buildings and elaborate ceremonials.

Mr. E. Buckingham then discussed 'The Thermodynamic Scale of Temperature.' Since temperatures are not quantities in the ordinary sense of the word and are not additive, a scale of temperatures is an arbitrary thing and scales founded on the properties of different materials will not agree. In attempting to reconcile the readings of different gas-thermometers and reduce them to a theoretical thermodynamic scale, reliance has been placed on the observations on the change in temperature of gases forced through a porous plug, and excessive extrapolation has been necessary. The speaker has attempted to coordinate the discordant results by applying the 'law of corresponding states' and finds the discrepancies are very much reduced.

THE 624th meeting was held on December 8, 1906, Vice-President Bauer in the chair.

The evening was devoted to the annual address of the president, Professor Cleveland Abbe. His subject was 'The Progress of Science as exemplified by Meteorology.' He gave a rapid sketch of the history of the science with references to the Americans who had contributed to its advancement, and in conclusion exhibited a number of lantern-views of a water-spout photographed by numerous observers last summer near Marthas Vineyard, Mass., and thoroughly studied by Professor Bigelow.

THE 625th meeting, the 36th annual meeting, was held on December 22, 1906.

The report of the secretaries showed a resident membership of 130; fifteen regular meetings have been held; Vol. XIV. of the *Bulletin* has been completed and distributed.

The treasurer's report showed a sound financial condition.

The following officers were elected for 1907:

President—John F. Hayford of the Coast and Geodetic Survey.

Vice-Presidents—L. A. Bauer, A. L. Day, E. B. Rosa, C. K. Wead.

Treasurer—B. R. Green.

Secretaries—G. K. Burgess, R. L. Faris.

General Committee—C. G. Abbot, C. Adler, L. J. Briggs, W. A. DeCaindry, W. S. Eichelberger, L. A. Fischer, R. A. Harris, J. Page, I. Winston.

CHARLES K. WEAD,
Secretary

THE NEW YORK ACADEMY OF SCIENCES
SECTION OF GEOLOGY AND MINERALOGY

At the meeting of December 10, 1906, the following papers were presented:

Present Structural Character and Probable Former Extent of the Palisade Trap: Alexis A. Julien.

In the Palisades, the trap sheet along the lower Hudson River, the structures most generally known are the vertical or columnar and the coarsely bedded structure with foliation parallel to that of the whole stratum. Less familiar is a coarse concentric structure visible on horizontal surfaces. A thinly lamellar structure has also been brought out by natural etching on weathered vertical surfaces, which appears to represent a flow structure, or, perhaps more properly, a pressure lamination.

At a certain zone along the face of the escarpment an interrupted or continuous sheet of decayed rock crops out, a principal cause of destruction of the columns by undermining. It indicates a process of preglacial decay of extreme antiquity, and is attributed to a balance of conditions of perpetual moisture in a coarsely granulated band of the trap. Two systems of faults traverse the trap sheet from north, or a little east of north, to the opposite point, and from north-northwest to south-southeast. Eight or nine faults have been located by previous observers, and some of these extend for several miles. But there is abundant evidence of a large number of other faults, those of one system marked by depressions which furrow the top of the ridge in the direction of its trend; those of the other system indicated by nicks or clefts (cloves) along the edge of the escarpment. The original thickness of the trap sheet shows that these hollows represent the bottoms of ancient gorges of a drainage system guided by the faults over the summit of the ridge. This signifies an enormous amount of denudation, doubtless effected by the continental glacier, which, in connection with the faults, has also resulted in the southwestward slope of the ridge down to the sea-level. As to the mooted question of the eastward extension of this sheet of the Newark formation beyond the Hudson River, direct evidence appears in the low rock terraces between Dobb's Ferry and Peekskill, formerly occupied by the overlap of this formation; in the common distribution of zeolites, etc., in cracks of the gneisses on the east side of the river, which could only have been derived through infiltration of thermal waters from an overlying trap sheet; and in the correlation of depressions on opposite sides of the river, which have been impressed by an ancient drainage system over the uplifted Mesozoic terrane and across the present line of the Hudson River.

Development of the Inner Wall in Paleozoic Corals: G. E. Anderson.

From serial sections of *Craspedophyllum subcæspitosum* it was shown that the primitive union of septa is retained even after radial condition of septa is reached. The wall is formed by the turned-over and fused ends of the septa and at one stage resembles the fossular wall of certain corals. Final closure of the wall by bridging of the fossular gap occurs in accelerated mutations of this type and is normal in *Eridophyllum.* No true inner wall exists in *Acervularia* and similar genera.

The Geographical Classification of Marine Life Districts: A. W. Grabau.

After a discussion of principles the following tentative classification was outlined:

Geographic Divisions	Bathymetric Life Districts Present
I. Intercontinental seas or oceans.	Littoral, pelagic, abyssopelagic, abyssal.
II. Intracontinental seas.	
a. Mediterraneans.	Littoral, pelagic, abyssopelagic, abyssal.
b. Epicontinental seas.	Littoral, pelagic.
III. Continental seas or lakes.	Littoral, pelagic, more rarely abyssal.

The speaker advocated that the term littoral be extended so as to cover all that district from high water to the edge of the continental shelf (or beyond, to the point where the photic or lighted portion of the sea bottom ends), instead of restricting it to the shore zone, as is often done. He also advocated the restriction of the term epicontinental sea to that type of intracontinental sea which has no abyssal district. The advantage in precision thus gained and the natural character of the classification proposed were pointed out.

A. W. GRABAU,
Secretary of Section

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

THE third regular meeting of the season of 1906–7 was held at the Chemists' Club, 108 W. 55th Street, on January 11.

The Nichols medal, awarded annually for the best paper read before the New York Section, was presented to Howard B. Bishop for his paper 'On the Estimation of Minute Quantities of Arsenic.' Favorable mention was made of the paper of E. H. Miller and J. F. Thompson on the 'Silver Platinum Alloys' and of the papers of F. B. Power and Frank Tutin on the 'Chemical Examination of *Æthusa Cynapium*' and on the 'Chemical and Physiological Examination of *Chailletia Toxicaria*.'

The rest of the evening was devoted to a symposium on the pure-food law by Messrs. H. W. Wiley (address read by chairman), Virgil Coblentz, R. Z. Doolittle and M. D. Foster. Further discussion followed, in which Messrs. Wm. J. Schieffelin, Albert Plaut, J. B. F. Herreshoff and L. L. Watters took part.

C. M. JOYCE,
Secretary

DISCUSSION AND CORRESPONDENCE

FACTS AND INTERPRETATIONS IN THE MUTATION THEORY

THE foremost champion of de Vries's mutation theory in this country undoubtedly is Dr. D. T. MacDougal, and he has largely contributed to the popularity of this theory. In a recent article[1] he takes up certain objections made by various writers, and attempts to show that they are without foundations or opposed to the known facts. But the criticism of the objections made by C. H. Merriam, D. S. Jordan and the present writer fails to convince, and only serves to demonstrate that the vital points have been misunderstood.

Before I try again to give a review of my objections to de Vries's theory, I shall prove in detail that MacDougal's criticism of them, as well as of those of Merriam and Jordan, is unsatisfactory. It may appear as presumptuous, when I take it upon me to talk in behalf of the latter two gentlemen, who are well able to take care of themselves,[2] but I may be excused on the ground that I hold precisely the same views, and am thus defending my own opinions.

I. MacDougal first takes up Merriam's contention, that the study of geographical distribution of animals shows no evidence of 'mutation' (in the sense of saltation or discontinuous variation), since there are gradual transitions, which point to a progressive development of minute variations. This is not admitted by MacDougal, because he maintains (p. 209) that 'once a mutant has appeared, no evidence of its distribution can be taken to account conclusively for its origin.' Jordan has answered this in the article just referred to. But there is yet another aspect. Merriam did not express any view as to the *origin of mutation* (*saltation*); he only wanted to bring out the fact that mutations, in the sense of discontinuous variations, seem to be extremely rare in nature, which is indicated by the fact that, morphologically, varieties and even species are often very close to each other, and that, if there are cases where a discontinuity is apparent, a closer investigation of the distribution, not only of the supposed mutant, as MacDougal puts it, but of the mutant and its allied forms, reveals the existence of intermediate forms.

With reference to this latter case, I should like to make a few additional remarks. Granted the existence of a connecting form between two extremes, which appear to fulfill the morphological requirements of mutation,[3]

[1] 'Discontinuous Variation in Pedigree-Culture,' *Pop. Sci. Monthly*, 69, Sept., 1906, pp. 207–225.

[2] See Jordan's rejoinder in SCIENCE, September 28, 1906, p. 399.

[3] We always are to remember that, strictly speaking, there is no morphological difference between fluctuating variation and mutation; the latter can only be recognized by experiment, according to de Vries, and also MacDougal. Thus it is not correct to talk, as MacDougal does, of

the question is, whether this connecting form represents a stage of development from one extreme to the other, or, as the de Vries school supposes, a hybrid between the extremes. This question, in my opinion, may be settled in many, if not in all, cases, by the facts of the geographical distribution. If the overlapping area of the ranges of the two extremes is occupied by an intermediate form associated with the two original forms, hybridization is indicated; if, on the contrary, in the intermediate area the intermediate form is exclusively represented, the latter very likely marks a connecting step in the development from one to the other extreme.

I am much tempted to illustrate this here by an example I have discovered in the distribution of certain species of river crawfishes (group of *Cambarus propinquus*). But it would take up too much space to give all the facts. It will be presented to the public in due time, in fact, the paper is just now going through the press. Suffice it to say that both of the above cases are illustrated in this example, but that the final decision was possible only after a thorough investigation of the geographical distribution of these forms had been made, by researches covering the whole of the state of Pennsylvania, and parts of Maryland, West Virginia and Ohio.

MacDougal further says (p. 209) that "a number of zoologists have assumed to speak of the distribution of plants, with apparently no basis except 'general information' to the effect that closely related species do not have the same habitat. This has been variously put, but the general meaning is as given." On page 210 he talks of this as 'the idea * * * that closely related species do not occupy the same region.' I believe I am included in this number of zoologists, although I never expressed such an idea; but since I do not know of any other zoologist who did, I think MacDougal refers to something similar I have said. I expressed it thus:[4] 'two closely allied species never occupy absolutely the same *range* under identical ecological conditions.' This, however, does not exclude the possibility that *parts* of their different ranges may overlap, and that in certain regions they may be found together, and, moreover, a case in point has been described by me.[5] Thus it is evident that MacDougal has misunderstood me, and that he battles against a fancied idea, which, indeed, is disproved by the instance given (*Opuntia fulgida* and *mammillata*), and to which I am able to add numerous other examples of plants[6] as well as animals. But this does not influence my contention, *that closely allied species of plants or animals never possess precisely the same geographical range.*

With reference to Jordan's opinion that *Œnothera lamarckiana* might be a hybrid, which is also held by the present writer, MacDougal thinks that it possibly might be a good, natural species. This question, however, is not essential for the interpretation of de Vries's experiments. The suggestion that it might be a hybrid, or the fact that, in Europe, it is an escaped garden-form, is advanced only to explain the remarkable variability of it. De Vries, assuming that it is as good as a natural species, tries to account for its wonderful capacity to throw off mutants, which is not generally found among natural species, by believing that there may exist, in any species, a time or period of especially vigorous and frequent mutation. But con-

mutation as a synonym of discontinuous variation. A mutation may be due to discontinuous variation, and, as de Vries declares, generally is, but not always. In this respect, MacDougal, in the article referred to, certainly goes beyond de Vries. If this is borne in mind, it is clearly seen that Merriam's objection does not affect at all de Vries's theory as a whole, but only the part of it that says that discontinuous variation or saltation is a necessary or frequent attribute of mutation (in the sense of creating the faculty to become a true breeding form).

[4] SCIENCE, June 22, 1906, p. 949.

[5] SCIENCE, March 30, 1906, p. 504.

[6] For instance, *Orchis ustulata* and *tridentata,* two closely allied species, but of quite different aspect, grow side by side (forming hybrids) upon the meadows of the valley of the river Saale, near Jena, Germany; on a certain hillside near Jena, *Ophrys muscifera* and *aranifera* grow together (also forming a hybrid).

sidering the fact that, in Europe, *Œnothera lamarckiana* is surely an escaped garden-form, and that it actually has been there under the care of florists for a long time, the possibility that it might be found in the wild state in America does not affect in the least degree the very tempting assumption that its strange behavior is due to cultivation with all its inherent and accessory incidents, exactly as is the case in other garden-forms.

MacDougal regards it as a specimen of 'literary license' and 'inaccuracy' (p. 213), when I say that de Vries entirely failed to take notice of the principle that the discovery of intermediate forms serves to show that two supposed true breeding forms do not possess the rank of species as understood by the taxonomist, and that he also failed to show that his so-called elementary species are not connected by intermediate forms. I do not see where the 'license' and 'inaccuracy' come in, for it is *a fact* that de Vries nowhere reported that he went over the whole area of any of the species used or referred to by him, and tried to ascertain whether there are anywhere such forms. Indeed, he reports that on rare occasions he found something which might be taken for connecting forms, but he never searched carefully and conscientiously for them. On the other hand, I know positively from my own experience that such forms do exist at least in some of the elementary species discussed by de Vries: I found them myself in nature in the group of *Viola tricolor* and *lutea,* and saw them in the case of *Draba verna* in de Bary's laboratory.[7]

As regards my 'estimate of the futility of experimental methods' and my 'mistrust' of them, MacDougal (p. 213) has not understood my standpoint. I have never said anything that might be construed as if I 'mistrusted' experiments or believed them to be 'futile,' on the contrary, I fully agreed that experiments ought to be made, but warned against too great complexity and improper interpretation.[8] I chiefly called attention to the complexity of conditions offered in cultures in the botanical garden, which is met by MacDougal (p. 213) by the statement that it is not the case, that 'domesticated races' have resulted from the 'effects of tillage.' But, disregarding the fact that 'tillage' is only one of the many factors contributing to the peculiar features of environment in the garden, I never said that the effect of tillage (or any other environmental factor) is the production of 'domesticated races.' I attribute to the environment the power to influence 'variation,' but in order to obtain 'domesticated races,' that is to say, forms which breed true, I always insisted that pedigree-culture is necessary. In nature the analogous process, selection and segregation, leads to the formation of species.

II. It is possibly well to present here again my objections to de Vries's mutation theory, and, to further the correct understanding of my views, I shall try to represent the matter in a somewhat different form, emphasizing chiefly what are the undoubted facts, and what are their interpretations on the part of de Vries and on my part.

My first and fundamental contentions are:

1. *De Vries's conception of 'elementary species' is inadequate.* There are, indeed, forms in nature which have a tendency to breed true, but which are not isolated from other forms, but these forms should not be called species. They have been called, for instance, by Darwin,[9] 'varieties,' and are distinguished by this quality from 'variations.' On the other hand, there are in nature true 'species,' characterized by the fact that the tendency to breed true is fully developed, and that there are no connecting links any more with allied forms: they are separated from the latter. This character furnishes a good definition for the term 'species,' which ought to be the taxonomic species. This, however, does not mean that in every case it should be easy or even possible to distinguish sharply between a variety and a species, since there are actual cases of transition in nature. Yet at the present state of our knowledge, the insufficiency of the latter alone prevents in many cases a final decision.[10]

[7] See also: Stone, W., in SCIENCE, May 4, 1906, p. 701, with reference to *Viola.*

[8] SCIENCE, June 22, 1906, p. 952.

[9] Darwin, 'Origin of Species,' p. 33.

[10] See *Pr. Amer. Philos. Soc.*, 35, 1896, p. 191.

2. *The essence of de Vries's experiments, pedigree-culture, consists of 'selection' and 'segregation.'* This becomes most evident in MacDougal's description of his methods as given in the paper under discussion (p. 214 f.), and anybody may see at a glance that MacDougal, as well as de Vries, did nothing that has not the purpose of selecting certain variations and their seeds, and of segregating (separating) them from disturbing influences.

Granting that these fundamental views are correct, we may now look at the bare facts represented in de Vries's experiments, without any attempt at explanation or theoretical speculation. The following two stand out prominently:

1. *By pedigree-culture de Vries succeeded in making certain variations breed true.*

2. *In other cases of variations he did not succeed.*

The first sentence tells an old story. The same has been done since times immemorial, and scientific investigation has taken notice of this fact since the time of Darwin. The process is now rather well understood, that is to say, with reference to the essential features of the action required of man: they are selection and segregation. De Vries did not change the old method in the slightest degree, he only introduced additional precaution and refinement in detail, taking particular pains to insure the full efficiency of these two factors by carefully excluding all possible interference with them. In addition, he was the first to keep proper scientific records of what he was doing.

The second fact, on the contrary, is new, and it is the point in de Vries's experiments which needs explanation and a theory. Why is it that certain variations did not breed true under de Vries's hands, although they were treated exactly like those belonging to the first group? The general belief, up to this time, was that any variation might be transformed into a true breeding form by proper treatment.

De Vries's explanation of this fact is given in his mutation theory. Believing that his experiments are conclusive, and that, since he himself did not succeed in cases of the second group, *nobody would be able to do so,* he propounds the theory that there are actually certain variations, in which selection and segregation (pedigree-culture) are impotent to produce true breeding, and, consequently, that there are two classes of variations, the one of which he calls 'mutation,' which produces forms which respond to the effort of the breeder, the other in which the art of the breeder has no effect, and which he calls 'fluctuating variation.' Then the first is, of course, all important for the species-forming process, while the other is of no consequence. There is no saying, with respect to any particular variation, whether it may belong to the one or to the other class, before the actual test (pedigree-culture) has been made, although it seems that mutations often or generally differ from fluctuating variations in the degree of deviation from the original form. This is the essence of the mutation theory.

The above conclusion and theory would be perfectly correct, if the proposition was correct that it is actually impossible to make certain variations breed true. But just in this point, I believe, de Vries is wrong, since his experiments were not conducted in such a way as to absolutely preclude the possibility that even so-called 'fluctuating variations' may be successfully transformed into true breeding forms. We always are to bear in mind that it is at least thinkable that a particular form may be bred true only under particular conditions, under conditions which are congenial or essential to its very existence. To present an imaginary example: a plant species may possess a peculiar variation, which is due to lack of direct sunlight (shade-form). Suppose this shade-form is cultivated according to de Vries's method in the botanical garden, in beds where it gets its full share of sunlight. I never believe, in such a case, that pedigree-culture will succeed in making this shade-form breed true, since always the conditions of environment will have the tendency to paralyze the effort of the breeder. If, however, this particular shade-form is bred in the shade, under proper environment, the attempt possibly may not be in vain.

This is only an example to illustrate what I think may be correct. Every plant breeder knows that many of our garden forms will come true only when treated in a certain way. These various ways, including everything that comes under the head of gardener's 'tricks,' are familiar to the professional and amateur,[11] and their results are regarded among laymen as due to a 'lucky hand.' Pure strains of seed, of course, obtained by pedigree-culture, are the first condition, but pedigree-culture is not all of the secret, since the proper handling of the seeds is also material, as well as the observation of certain 'tricks' with the growing plants, *which have no other object but to furnish the congenial environment to the object.*

I think that any one who has ever done actual garden work, trying to raise particular strains of flowers or vegetables, will understand what I mean by these 'tricks.' This essential element is obviously lacking in de Vries's experiments: he uniformly bred all his mutants 'in the botanical garden,' and 'in well-manured soil,' and apparently also under the same conditions of climate, season, subsoil, insolation, etc., that is to say, under a uniform set of ecological conditions, such as are generally found in a botanical garden. Indeed, it has been questioned that a change of these conditions may influence the true breeding of a strain, but without sufficient reason, since such an assumption is surely unwarranted as long as the question has not been actually tested in a scientific way. The necessity, in certain cases, to observe certain 'gardener's tricks,' in order to get the best results in raising particular races, strongly favors the opinion that environment actually has something to do with it, and scientific experiments with this in view should be made by all means. Where de Vries succeeded in breeding true his 'mutations,' the environment of the botanical garden was not averse to the experiment, and in this connection it is suggestive that his chief success was attained with *Œnothera lamarckiana*—an escaped garden-form, to which apparently the botanical garden was congenial.[12]

Finally, in order to define my standpoint as precisely as possible, and in order to obviate unnecessary discussion of minor and irrelevant points, I shall condense everything I have said into five questions, and if anybody wants to challenge my propositions, I ask him to do so in terms as laid down here.

1. *Does the 'elementary species' of de Vries correspond to Darwin's conception of 'variety,' and is my definition of 'species' ('taxonomic species') acceptable?*

2. *Are selection and segregation the essential features in pedigree-culture?*

3. *Are de Vries's experiments, aside from their greater accuracy and refinement, essentially identical, in their method, with those of the earlier breeders, as, for instance, recorded by Darwin?*

4. *Is it advisable that breeding experiments should be repeated with due regard to environment, before a final judgment is to be pronounced, and are de Vries's experiments defective on this point?*

If the answer to these four questions is '*Yes*,' then my contentions are recognized as well supported, and the answer to the next question should also be '*Yes*'—

5. *Should the validity of de Vries's mutation theory be doubted, since he makes an unwarranted distinction between two kinds of variation, which further experiments possibly will prove to be identical?*

If, however, anybody should be inclined to answer '*no*' to any or all of these questions, I ask him to give *reasons* for so doing. I have given my reasons for answering them in the

[11] For those who have no practical experience in gardening, the study of a few items in Bailey, 'The Cyclopedia of American Horticulture,' will give an idea of the immense variety of these tricks.

[12] I call attention to Jordan's account of some of Burbank's experiments (in *Pop. Sci. Monthly*, January, 1905), where also the influence of the environment in the production of variation is repeatedly emphasized, chiefly on pages 205 and 206. Possibly, if Burbank's attention is called to it, he may be able at once to quote instances where the true breeding of a certain strain depends largely on the environment offered. This, of course, should be the general rule, but it can be clearly observed only in such cases where a particular feature of the environment is known to be responsible for a particular variation.

affirmative; consequently, it should be demonstrated that my reasons are no good. Nobody ever attempted this, and when arguments were given purporting to be opposed to my ideas, these invariably were not my views but only what the critic fancied to be my views.[13]

A. E. Ortmann

Carnegie Museum, Pittsburg, Pa.,
October 4, 1906

SPECIFIC NAME OF NECTURUS MACULOSUS

In the last number of the *American Naturalist* (Vol. XLI., January, 1907, pp. 23–30) there is an elaborate paper by Professor F. C. Waite under the above title, in which he shows that the name employed there has the priority over *N. maculatus,* the term most commonly adopted by anatomists. Towards the end of the paper (p. 27) he makes the following statement: "In the past ten years although many papers have been written on *Necturus,* two only have, as far as I know, used the correct nomenclature."

I wish to say that the 'correct' name was pointed out and the proper references given by the late Dr. G. Baur as early as 1897 (*Zool. Bull.,* I., p. 41). Since then it has been employed by various systematists. Thus the name *N. maculosus* is used in the eighth edition of D. S. Jordan's 'Manual of the Vertebrate Animals of the Northern United States,' 1899, p. 175, in which I tried to bring the nomenclature up to date. It has since been used, both in this journal (Science, N. S., XI., 1900, p. 555) by Fowler, and in the *American Naturalist* (XL., 1906, p. 159) by Stone.

Leonhard Stejneger

Smithsonian Institution,
January 14, 1907

THE DEFINITION OF SOLID AND FLUID

To the Editor of Science:—The point I have raised (October 26) as to the definition of solid and fluid seems quite timely in view of the discussion going on between Hoskins and See, and the letter of Mr. Willcox (November 9). Note the use of the term 'solid' in one, of 'substance' in the other, of the two definitions of rigidity cited by Hoskins. Their difference seems to be as to whether it is proper to speak of the rigidity of a fluid or a gas. The real question of fact, how much the interior of the earth yields to a certain variation of pressure, has not thus far entered the discussion.

Again, Mr. Willcox defines fluid and solid quite other than was suggested by me and the line between as the curve of the plastic yield point.

His definition is quite tenable, if we agree to it, may be made as exact, and fits quite as well the Latin derivation of the word fluid, but I am not sure that it agrees as well with usage or is as practical. We could then speak of no substance as solid or fluid without knowing under what pressure it is. Whether a body were solid or fluid would then depend not merely on the state of the body itself, including its temperature, but also on its surroundings—the pressure. We cannot, then, as he writes me, 'properly refer to any substance as a plastic solid.'

The earth's interior would be classed as a fluid, and not, as has been lately common, on account of its high rigidity, as solid.

The one point which is not quite clear, as he brings it in parenthetically, is whether the plastic yield point, and so his definition, depends on the time or rate of application of pressure. I judge not, according to the molecular theory which he adopts (dear to T. Sterry Hunt) that there are three states of molecular aggregation, solid, fluid and gas, and that the solid molecules are heavy and complex aggregates of the liquid molecules, as these are in their turn of the gas, and that sufficient temperature and pressure will break up the large solid molecules.

The definition which occurred to me, that a fluid is a body that can not rest under stress, *i. e.,* in a strained condition, is, however, just as definite and draws just as sharp line as that of Mr. Willcox. We may express it in his terms thus—*a fluid has a temperature such that its plastic yield point is reached even at zero pressure.* The relative content of the two concepts can be expressed graphically thus.

[13] See also my reply to Gager's criticism in Science, August 17, 1906, pp. 214–217.

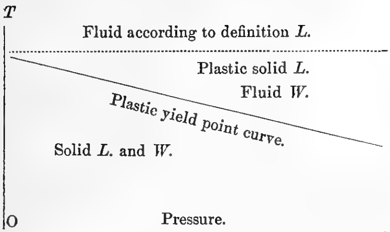

Figure illustrating varying conceptions, as to where the line between fluid and solid should be drawn.

Neither definition is absolutely rigorous, perhaps. It may be that every fluid can rest under a very minute amount of strain, and that every solid has plasticity.

No discussion of facts can settle usage, so I call for others, either here or in the scientific meetings, to express themselves as to what usage will best fit that of the past and present, and be most practical for the future.

ALFRED C. LANE

A CORRECTION

TO THE EDITOR OF SCIENCE: To a communication by the writer, which appeared in SCIENCE for January 18, 1907, the name of the U. S. Geological Survey was attached without authority of the director. The writer wishes to record his disavowal of any desire to commit the survey to an indorsement of the sentiments expressed in said note. For these he alone is responsible. In explanation he would add that the note was written before the writer became a member of the U. S. Geological Survey. It was not offered for publication, however, until about the time he was planning to enter on field work, when his new address was attached without due consideration.

Moreover, on deliberate reading, the writer is conscious that unintentionally there appears to be in the language employed a tone of discourtesy, which he regrets.

C. H. GORDON

January 22, 1907

SPECIAL ARTICLES

THE CASE OF ANASA TRISTIS

IN a paper read before the December meeting of the American Society of Zoologists in New York, illustrated by a very beautiful series of photomicrographs taken from smear-preparations, Miss Foot and Miss Strobell announced the following conclusions, which have since been published in a preliminary form in the January number of the *Biological Bulletin:* (1) There is no odd or 'accessory' chromosome in *Anasa tristis.* (2) The number of spermatogonial chromosomes is 22. (3) All the chromosomes divide in both maturation divisions. The so-called odd or accessory chromosome is only a 'lagging' chromosome, and it divides with the others in the second division. (4) The so-called 'chromosome-nucleolus' of the growth period is not a chromosome, but a nucleolus.

These results are at variance with my own, and since the differences in regard to the first three involve the important more general issue of the relation of the chromosomes to sex-production, I will make the following reply.

The fourth of the above conclusions, though materially different from my own, is not altogether irreconcilable with it. I have for some time had reason to suspect (in case of certain other genera) that a stage may have been overlooked in the prophases in which the odd chromosome temporarily loses its compact nucleolus-like form. For the study of this question smear-preparations offer decided advantages; and I am ready enough to admit that in regard to these stages Miss Foot and Miss Strobell may have made an important addition to our knowledge, though I still believe that the chromosome-nucleolus of the earlier stages is the odd chromosome. On the other and more vital points their results are irreconcilable with my own, and only these will further be considered here.

Since the announcement of these results I have carefully reexamined my old preparations (including those of Paulmier) and a series of new ones from material collected during the past summer. They include sections of material fixed in Flemming's, Hermann's, Bouin's and Gilson's fluids, of both *Anasa tristis* and *A. armigera,* that of the first-named species from Massachusetts, New Jersey, Michigan and Arizona. The reexamination not only confirms but fortifies anew at every point my former results. The evidence given by these preparations is, in my judgment, completely demonstrative that in *Anasa tristis:* (1) There is an odd chromosome that is one of the largest three spermatogonial chromosomes. (2) The number of chromosomes in the spermatogonia is 21, in the ovarian cells 22. (3) The odd or 'accessory' chromosome divides in the first spermatocyte division, but passes undivided to one pole in the second. Half the spermatozoa thus receive 10 chromosomes and half 11.

These facts appear with irreproachable clearness in a great number of cells, and with a uniformity of result that I think precludes the possibility of error. The number, size and grouping of the spermatogonial chromosomes, and the history of the odd chromosome in the second division, are clearly shown in Dr. Leaming's photographs (which will be published hereafter, should it seem desirable); and both these and the preparations are at the service of any who may wish to examine the evidence on which my conclusions rest. I may add that an odd chromosome also exists in the following genera of coreids that have not hitherto been reported on (the spermatogonial numbers in brackets): *Pachylis* (15), *Euthoctha* (21), *Narnia* (21), *Leptoglossus* (21), *Chelinidea* (21), *Margus* (23), *Catorintha* (25), *Leptocoris* (13), and in the pyrochorid genus *Largus* (*L. succinctus,* 13, *L. cinctus,* 11). In *Catorintha,* as in *Protenor,* the odd chromosome is the largest of the chromosomes. In *Metapodius,* alone among the coreids thus far examined, a pair of unequal idiochromosomes appear in place of the odd chromosome.

What is the explanation of this contradiction of results? Is *Anasa tristis* a kind of cytological Jekyll and Hyde which presents itself in different guise to different observers? Possibly; for I have found in *Metapodius terminalis* that certain individuals possess a small unpaired chromosome and an odd spermatogonial number (23), while other individuals, unquestionably of the same species, lack this chromosome and have an even spermatogonial number (22). I believe (but am not certain) that a similar relation occurs in *Banasa calva.* The unpaired chromosome is here of different nature from the usual type (as I shall show hereafter); but such cases show that differences of result regarding the number of chromosomes should not too hastily be attributed to error on either side. As regards *Anasa,* I can only state that the same result is uniformly given by many different individuals, from widely different localities (including Woods Hole, where the material of Miss Foot and Miss Strobell was procured) and after all the methods of fixation. The contradiction is, I think, probably due to the difference of method employed, Miss Foot and Miss Strobell having placed their faith in smear-preparations, while I have relied on sections. Without undertaking any discussion of the relative merits of the two methods, I may say that whatever be the disadvantages of sections for the purposes of photomicrography, really well fixed and stained sections are certainly competent for the demonstration of such points as are here at issue, while they have the advantage of leaving the topographical relations undisturbed. But the certain determination of the number of chromosomes in a given species by means either of smears or of sections, demands a more critical treatment than the mere matter of counting (or photographing) the number that may lie in a given field of view or even in many fields. The real task is to determine the relation normal to the species by the elimination of occasional variations (such as certainly occur in some species) and of plus or minus errors due to accident; and this again is not merely a matter of frequency of occurrence, but also of evidence given by the nature of the chromosome-groups taken in detail and as a whole. As far as sections are concerned, it is almost too elementary to require mention that a minus error may arise from the failure of one or more of the chromosomes to lie within the plane of section. This might be due either to a position that is normally at a different level from the others, or to accident. The first of these possibilities is eliminated by side views, which show that at full metaphase the chromosomes lie in a flat plate; while the early anaphases show the daughter chromosomes separating in two flat parallel plates. The second possibility is, I think, wholly excluded in the case of *Anasa* by (1) the great number of cells from different individuals that show 21 chromosomes (often a smaller number, in my experience never in a single instance 22); (2) the equally constant appearance of 22 chromosomes in the female; and above all (3) the fact that in *Anasa* and some of the other genera *the smaller number in the male is shown by the size relations to be consistently owing to the absence of a particular chromosome*—in *Protenor* always one member of the largest pair that appears in the female, in *Anasa* always a member of one of the largest two pairs. It is manifestly impossible that this should be due to accident. As to the passage of the lagging odd chromosome without division to one pole in the second division, I do not think the most critical observer who examines the demonstrative evidence given by very large numbers of cells in my preparations can have the slightest doubt.

I, therefore, think that as far as the first three points are concerned, my results on *Anasa* will sooner or later receive full confirmation at the hands of other observers. Paulmier himself, after reexamining his own preparations, was thoroughly convinced of his error regarding the spermatogonial number—an error that was a natural one at the time his work was done. The most careful search has failed to discover the original of the group from which he figured 22 chromosomes, or any other agreeing with it in number. Montgomery's confirmation of my result as opposed to his own earlier one is known. The consistent and cumulative corroboration given by so many other genera of Hemiptera (including one of the Homoptera recently studied by Miss Stevens), some of them far more favorable for study than *Anasa*, speaks for itself.

Edmund B. Wilson

Zoological Laboratory,
Columbia University,
January 20, 1907

NOTES ON ORGANIC CHEMISTRY

DIAZONIUM PERCHLORATES

The chemical activity of perchloric acid and the fact that, under certain conditions, it is explosive, are matters of common knowledge, it is also generally known that many of the diazonium (diazo) salts, such as benzenediazonium nitrate, $C_6H_5N:NNO_3$, are likewise explosive and their instability is often very great. It might be anticipated, therefore, that diazonium perchlorates, if they could exist at all, would prove to possess an unusual degree of energy and that their study, although it might be attended with danger, would yield results of considerable general interest. A number of such compounds have recently been described, simultaneously, by D. Vorländer,[1] and by K. A. Hofmann and H. Arnold.[2] The object of the former was to endeavor to discover some basic substance which, with perchloric acid, would form a sparingly soluble salt that could be employed instead of potassium perchlorate for the quantitative determination of the acid. This result has not been attained, but it is found that an immediate, voluminous precipitate is formed when phenylacridine sulphate is added to a 1 per cent. perchloric acid solution, and that a turbidity is produced when a 0.1 per cent. solution of the acid is used. Hofmann and Arnold desired to obtain some sparingly soluble diazonium salts. The preparation of *benzenediazonium perchlorate*, $C_6H_5N:NClO_3$, is very simple. Aniline (2 grams) is mixed, at a temperature of 0°, with water (200 c.c.), concentrated hydrochloric acid (6 c.c.), commercial perchloric acid (10 c.c.) and sodium nitrate (1.5 grams). The perchlorate immediately crystallizes out in colorless needles, which are strongly doubly refractive. Even in the moist state this compound explodes with great violence if rubbed or struck with articles of stone or iron. When dry the explosive power of the substance is, of course, very much greater. If a few centigrams are dropped on blocks of hard wood, deep holes are torn in them, but the explosion is so local in its effect that vessels of thin glass, placed a

[1] *Ber.* **39**, 2713 (1906).

[2] *Ibid.*, **39**, 3146 (1906).

few inches away from the seat of disturbance, are uninjured.

The sensitiveness of the orthotoluene derivative is even greater than that of the above benzene compound, because in the moist state it explodes if it be touched with a porcelain stirring rod.

Chemically, these compounds are relatively stable and can be retained in a cool place for a day without change.

A considerable number of other diazonium perchlorates are described in the two papers quoted; in general, they exhibit the interesting contrast of being slightly *less* explosive than the two salts described above, but considerably *more* unstable chemically.

PREPARATION AND PROPERTIES OF BENZOYL NITRATE

THE formation of an acid chloride, such as acetyl chloride, CH_3COCl, from an acid and phosphorus pentachloride, is one of the most generally applicable reactions known. The corresponding bromides are available in moderate numbers, but only a very few of the iodides or fluorides have been prepared and, with these exceptions, no compounds are known containing an inorganic acid radicle in place of chlorine. For this reason, as well as on account of its properties which are described below, the isolation of *benzoyl nitrate,* $C_6H_5COONO_2$, is a matter of considerable interest. A preliminary announcement on the subject some months ago by F. Francis[1] has been followed up with a fuller account by him self[2] and T. H. Butler.[3] The compound is prepared by mixing together, at about $-15°$, benzoyl chloride and well-dried, finely divided silver nitrate. The product is an oil which is extremely sensitive to moisture. It may be filtered through dry filter paper, but if the latter contains the ordinary amount of moisture the nitrate reacts with explosive violence. In sealed tubes, at the ordinary temperature, the nitrate changes gradually into benzoic anhydride and, apparently, nitrogen pentoxide. At 100° the change is similar, except that nitrogen peroxide and oxygen are formed, and at higher temperatures the reaction is so rapid as to produce an explosion. In nitrobenzene solution the nitrate is transformed slowly, to the extent of 60 per cent. in six months, into metanitrobenzoic acid. Other non-ionizing solvents bring about the same change, but less rapidly.

In its reactions benzoyl nitrate behaves as a nitrating agent, as an oxidizing agent, or as a means of introducing a benzoyl radicle. Alcohol yields ethyl nitrate; aromatic hydrocarbons, phenols, and their ethers give, with ease, chiefly mono-nitroderivatives. As examples of oxidation may be mentioned the transformation of thiophenol and hydrazobenzene into diphenyldisulphide and azobenzene, respectively. Primary aromatic and secondary aliphatic amines yield benzoyl derivatives, C_6H_5CONHR, but secondary aromatic amines form nitrosamines. With the exception of paratolylmethylnitramine, $CH_3C_6H_4N(NO_2)CH_3$, these are unstable and pass into nitro secondary amines, such as $CH_3C_6H_3(NO_2)NHCH_3$.

One of the chief points of interest in connection with the nitrating action of benzoyl nitrate is that it permits of this reaction being realized in the complete absence of water, a condition which, hitherto, has been unattainable. The nearest previous approximation to it was by the use of diacetyl orthonitric acid, $(CH_3COO)_2N(OH)_3$. One result of such change in the conditions of reaction is that orthonitroaniline, which can only be obtained to the extent of about 10 per cent. in admixture with its isomers by the ordinary methods of nitration, is formed quantitatively by the action of benzoyl nitrate on acetanilide.

Metanitrobenzoyl nitrate, $O_2NC_6H_4COONO_2$, and *butyryl nitrate,* $CH_3CH_2CH_2COONO_2$, are also formed by the action of the respective chlorides on silver nitrate, hence it would appear that such compounds are always produced, as intermediate steps, in the reaction of an acid chloride on a metallic nitrate.

J. BISHOP TINGLE
JOHNS HOPKINS UNIVERSITY

[1] *Jour. Chem. Soc.,* **89**, 1 (1906).

[2] *Ber.,* **39**, 3795 (1906).

[3] *Ibid.,* **39**, 3804 (1906).

QUOTATIONS

THE BIOLOGICAL SURVEY

What Representative Wadsworth of New York, chairman of the House Committee on Agriculture, hopes to do by striking out from the agricultural bill before it is reported to the House the provisions for the maintenance of the biological survey is not clear. If this menace is agreed to by the House it will only serve to add to Mr. Wadsworth's unpopularity with the farmers of his section of the state. On the face of it, the plan to abolish the biological survey is an utterly foolish thing to do.

Because no reason is apparent for the hostile action of Mr. Wadsworth's committee, some persons have suspected a personal animus back of the move. If the appropriation for the support of the survey is allowed to be stricken out on the grounds of economy it is a policy of penny wise and pound foolish. It costs only about $55,000 to do the work of the survey. Naturalists, bird lovers, and all others interested in game protection say that the work of the bureau is done efficiently.

Dr. C. Hart Merriam, the chief of the biological survey, is a New York physician, who gave over a large practice in order to take up the government work at a small salary because of his love for natural science. He is one of the noted ornithologists and mammalogists of the world. Dr. A. K. Fisher, assistant in charge of economic investigation, is also a physician who dropped his practice and went in to the government service for reasons akin to those which moved Dr. Merriam to the same course. Dr. T. S. Palmer has direct charge of the enforcement of the game laws and is the chief adviser on game and song-bird protection matters for the game protection officials of the different states. Under Dr. Palmer's direction within the last few years the work of protecting the game birds and game mammals of the country has been put upon a plane of efficiency.

The present expectation is that the usual appropriations for carrying on the work of the survey will be put in while the bill is under consideration in the House. Even should representatives support their committee, not much doubt is expressed that the senate will do what is necessary. President Roosevelt is not loath to take a hand in legislative enterprises, and he would certainly not be backward in coming to the assistance of the chief of the survey should his assistance be necessary.—New York *Evening Post*.

LECTURES ON PROBLEMS OF INSANITY

The Psychiatrical Society of New York has arranged for a series of four lectures on problems of insanity, to be held under the auspices of the Academy of Medicine at 17 West 43d Street, on Saturdays, January 19, February 2, February 16 and March 2, 1907, at 8:30 P.M. The purpose of these lectures is to put within the reach of the medical profession and also of the non-professional leaders of sociological interests a program of work and facts for orientation, with a view to the organization of a movement towards prophylaxis and the development of sound interests in this eminently important topic.

The first lecture will be given by Dr. Adolf Meyer, on Modern Psychiatry, its possibilities and opportunities; the second lecture, by Dr. August Hoch, discusses the manageable causes of insanity, exclusive of heredity; the third lecture by Dr. C. L. Dana, the data of heredity and their application in psychiatry; and the fourth lecture by Dr. Allan McLane Hamilton, the development of the legal regulations concerning the insane.

Encouraged by the welcome which the broad movement against tuberculosis has received, the society considers a public discussion of the facts of insanity of fundamental importance for a natural development of public and personal hygiene, and the only way to replace the traditional horror and disregard by a profitable interest on the part of the thinking and active citizens of the community. Where general cooperation is so much needed as it is in the handling of abnormal mental developments, it is especially necessary to bring together the many interests which now work independently in social reform, schools, hospitals, courts and institutions.

C. Macfie Campbell, M. B.,
Secretary

THE ERECTION OF A MONUMENT TO THEODOR SCHWANN

On the seventh of December, 1910, a century will have elapsed since the day that Theodor Schwann was born at Neuss on the Rhine.

His native town has resolved to raise to this great son of hers a monument to be unveiled on that memorable day. An appeal to the public having been made to this effect by the physicians of the Rhine country and the medical faculty of the University of Bonn, a considerable sum of money has been contributed both by the inhabitants of Neuss and especially by the physicians of Rhineland and Westphalia.

A primary fund for the realization of the project being thus secured by his countrymen, we may be pardoned if we address to the biologists and especially to the physicians all the world over the urgent request, to enable us by their generosity to accomplish a work destined to glorify one of theirs, and of whom they may feel justly proud.

Theodor Schwann's merit, we need scarcely say, is so eminent, that it has been equalled by few savants, surpassed by none. It was he who, sixty-five years ago, joined his efforts to those of M. J. Schleiden, and under the auspices of his master, Johannes Müller, he was the first to substantiate the cellular theory. In the course of time he proved to be a pioneer no less successful than indefatigable in the domains of histology, physiology and biology; an authority on the processes of fermenation, decomposition, spontaneous generation and digestion and last, not least, the discoverer of pepsin.

Every gentleman and friend of scientific progress satisfies, therefore, but a duty of honor in contributing for a monument in honor of Schwann.

Whosoever had the good fortune of knowing this modest man in his life and of looking in his gentle and intelligent eye, will know that his heart was not set on exterior things. Ever ready to acknowledge the merit of others, he aspired to no honors for himself: all the more reason to honor him in his death.

To his immortal master, Johannes Müller, a statue of bronze has been lately erected in his native town of Coblenz; his collaborator Schleiden has been similarly honored at Jena, the chief scene of his labors. Let us, therefore, not be remiss in discharging our debt of gratitude to Schwann whose name is linked preeminently and forever to the substantiation of the cellular theory, one of the greatest achievements of modern science.

Contributions are, please, to be addressed to the 'Städtische Sparkasse zu Neuss am Rhein. Schwanndenkmal, Germany.'

Germany.—Berlin: Geh. Rat Professor Dr. Waldeyer, Geh. Rat Professor Dr. Hartwig, Geh. Rat Professor Dr. Franz Eilhard-Schulze, Professor Dr. Posner. Bonn: Geh. Rat Professor Dr. Pflüger, Geh. Rat Professor Dr. von la Valette St. George. Breslau: Geh. Rat Professor Dr. K. Hasse, Professor Dr. K. Hürthle. Erlangen: Professor Dr. Hauser, Professor Dr. Fleischmann. Freiburg i. Br.: Geh. Rat Professor Dr. Bäumler. Giessen: Professor Dr. K. Eckhard, Professor Dr. H. Strahl. Göttingen: Geh. Rat Professor Dr. Ebstein, Geh. Rat Professor Dr. Esser. Greifswald: Professor Dr. Bleibtreu, Geh. Rat Professor Dr. Mosler. Halle: Professor Dr. Roux, Professor Dr. Grenacher. Heidelberg: Geh. Rat Professor Dr. Fürbringer, Professor Dr. A. Kossel. Jena: Hofrat Professor Dr. von Bardeleben, Professor Dr. Häckel. Kiel: Professor Dr. Graf F. Spee, Professor Dr. Hensen. Königsberg: Professor Dr. Stieda, Professor Dr. L. Hermann. Leipzig: Professor Dr. Babl, Professor Dr. Hering. Marburg: Professor Dr. Gasser, Professor Dr. F. Schenk. München: Professor Dr. Rückert. Münster, Professor Dr. Ballowitz. Rostock: Professor Dr. Barfurth, Professor Dr. Langendorff. Strassburg: Professor Dr. Schwalbe, Professor Dr. Hofmeister. Tübingen: Professor Dr. Froriep, Professor Dr. Grützner. Würzburg: Professor Dr. Rindfleisch, Professor Dr. Th. Stöhr.

Austria.—Graz: Professor Dr. Holl, Professor Dr. Zoth. Innsbruck: Professor Dr. Hochstetter. Prag: Professor Dr. Siegmund Mayer, Professor Dr. J. Gsad. Wien: Hofrat Professor Dr. Ritter von Ebner, Hofrat Professor Dr. E. Zuckerkandl.

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Belgium: Professor Dr. Swaen, Professor Dr. Ed. van Beneden, Professor Dr. Frédéricq.

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Holland: Professor Dr. Pekelharing.

Italy: Professor Dr. Guglielmo Romiti.

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Hungary: Professor Dr. von Lenhossek.

United States: Professor Dr. Charles S. Minot.

Canada: Professor Dr. Ramsay Wright.

Chile: Professor Dr. Jzquierdo.

Argentine: Professor Dr. Rodolf de Gainza.

Brazil: Professor Dr. Jhering.

Mexico: Professor Dr. Fernando Altamivano.

Asia—Calcutta: Professor Captain J. B. Kelly.

Japan: Professor Dr. J. Kaganii.

Australia: Professor Dr. James F. Wilson.

SCIENTIFIC NOTES AND NEWS

DR. CHARLES D. WALCOTT, director of the U. S. Geological Survey, was elected secretary of the Smithsonian Institution by the regents at their meeting on January 23.

M. CHAUVEAU, of the section of agriculture, has been elected president of the Paris Academy of Sciences to succeed M. Poincaré, of the section of mathematics.

M. BOUQUET, director of technical instruction in the French Ministry of Commerce, has been appointed director of the Conservatoire National des Arts et Métiers, to succeed M. Chandèze, who has retired from active service.

PROFESSOR A. MÖLLER has been appointed director of the Forest Academy at Eberswald.

PROFESSOR W. G. FARLOW, having withdrawn from the American editorship of the *Annals of Botany,* Professor Roland Thaxter has been chosen as his successor.

MR. ROOSEVELT has been elected one of the ten honorary members of the Royal Geographical Society.

DR. THOMAS A. STOREY, director of the gymnasium of the College of the City of New York, has been elected to the presidency of the Society of College Gymnasium Directors of the United States.

PROFESSOR CHARLES W. BROWN, of Brown University, has gone to Jamaica to study the geological phenomena connected with the recent earthquake.

MR. ARCHER M. HUNTINGTON has been elected president of the American Geographical Society in succession to Commander Robert E. Peary.

DR. GEORG SCHWEINWURTH, the African explorer and botanist, celebrated on December 39 his seventieth birthday.

PROFESSOR JULIUS ARNOLD, who has held the chair of pathological anatomy at the University of Heidelberg, has retired from active service.

DR. THEODOR SCHOTT, of Nauheim, will give lectures in Boston and elsewhere on diseases of the heart.

PROFESSOR ANGELO HEILPRIN lectured before the Franklin Institute on January 17, on 'Conceptions regarding Earthquake Phenomena and the Relationship of these Phenomena to Volcanic Disturbances.'

DR. S. A. MITCHELL, of Columbia University, will lecture at Vassar College on March 1, on 'Personal Experiences in Spain at the Recent Total Eclipse.'

PROFESSOR FRANK H. BIGELOW, of the U. S. Weather Bureau, gave, on January 22 and 23, two public lectures in meteorology at the University of Chicago, under the auspices of the departments of geography and economics. His topics were 'The Circulation of the Sun's Atmosphere as the First Cause of the Annual

Changes in the Weather,' and 'The Circulation of the Earth's Atmosphere, and the New Theory of Storm Energy.'

The winter course of free and illustrated popular lectures for 1907 of the Chicago Academy of Sciences will be given in the Assembly Hall of the Matthew Laflin Memorial Building, Lincoln Park, on Friday evenings, at eight o'clock, as follows:

January 25, 'American Museums and their Work,' Frank Collins Baker, Curator of the Academy.

February 1, 'The Evolution Theory, Pro and Con,' Dr. Winfield Scott Hall, Professor of Physiology, Northwestern University Medical School.

February 8, 'Deep Sea Fishes and Fishing,' Dr. Seth E. Meek, Assistant Curator, Department of Zoology, Field Museum of Natural History.

February 15, 'The Every-day Application of Ohm's Law,' a discussion of elementary electricity, Dr. Howard N. Lyon, Chicago.

February 22, 'The Place of Bacteria in the Causation of Disease,' Dr. Adolph Gehrmann, Bacteriologist, Columbus Medical Laboratories, Chicago.

March 1, 'Other Worlds than Ours,' Dr. Forest Ray Moulton, Assistant Professor of Astronomy, University of Chicago.

March 8, 'The Work of Leaves,' Dr. Charles B. Atwell, Professor of Botany, Northwestern University.

March 15, 'From the Big Horn Basin to the Yellowstone and Jackson's Hole *via* Sylvan Pass,' Mr. Charles A. Heath, Chicago.

Mr. William Wells Newell, of Cambridge, Mass., known for his researches in folk-lore, especially in connection with the Authurian tales, secretary of the American Folk-lore society and fellow of the American Association for the Advancement of Science, died on January 21, at the age of sixty-eight years.

Professor David Irons, professor of philosophy at Bryn Mawr College, died suddenly on January 24. He was born in Scotland in 1870, and received the M.A. at St. Andrews in 1901 and the doctorate of philosophy from Cornell University, 1894. After holding various positions in Cornell University, he was elected professor of philosophy in Bryn Mawr College in 1900. He was the author of contributions to ethics and psychology.

Charles B. Simpson, entomologist of the Department of Agriculture of the Transvaal, died of typhoid at Pretoria on January 14. Mr. Simpson was a graduate of the University of Idaho, and took his master's degree at Cornell University. In 1900 he became connected with the Bureau of Entomology of the U. S. Department of Agriculture as a special field agent, and was placed in charge of the investigation of the codling moth in the northwest. His investigations were brilliantly successful and resulted in the publication of Bulletins Nos. 35 and 41 of the bureau. In 1904 he was selected to fill the post of entomologist of the Department of Agriculture of the newly created Transvaal colony, and has since resided in Africa, making important investigations on malaria, on locust damage, and on the tick-borne diseases of cattle.

Dr. Clemens August Schlüter, for the past twenty-five years professor of geology and paleontology at Bonn, died on December 25, at the age of seventy-one years.

Professor Otto Benndorf, director of the Archeological Institute of the University of Vienna, has died at the age of sixty-eight years.

A bill has been introduced in the legislature incorporating the New York Observatory and Nautical Museum, to which reference has already been made in Science. It is stated in the charter that the museum is "for the purpose of encouraging and developing the maritime interests of New York City, of advancing the general knowledge of the safe navigation of the sea, of the development of harbor facilities, of prosecuting original researches in astronomy and navigation and in kindred subjects, and of affording instruction in the same." The incorporators are Frederick G. Bourne, Cornelius Vanderbilt, John E. Bourne, Edward S. Isham, J. D. Jerrold Kelley, Allison V. Armour, Edward H. Wales, George A. Cormack, John Neilson, Charles Lane Poor, William M. K. Olcott, Edward D. Adams, D. Delahanty and Addison Brown. Substantially the museum would be placed on the same basis as the Museum of Natural

History and the Metropolitan Museum of Art. The city is to provide the land and is to erect the buildings, while the corporation is to secure by private subscription not less than $300,000 for equipping the nautical museum and observatory and for prosecuting the other objects of the institution.

The different chapters in future volumes of the series 'Mineral Resources of the United States,' which the U. S. Geological Survey has been publishing for the past twenty-one years, will hereafter be written by members of the survey. In this report they will differ from their predecessors, which have been prepared mainly by outside experts for each of the mineral industries, working under appointments as special agents of the survey. This corps of experts included Messrs. Charles Kirchhoff, George F. Kunz, James M. Swank, Joseph D. Weeks, Charles D. Yale, John Birkinbine, Joseph Hyde Pratt, F. H. Oliphant, Heinrich Ries, Edmund Otis Hovey and F. J. H. Merrill. In the introduction to the last volume, which covers the calendar year 1905, Dr. David T. Day, chief of the division of Mineral Resources, in making this change, acknowledges his great indebtedness to them for their monumental work in building up and maintaining this series and his regret at the breaking of the ties of a score of years of joint endeavor. This series of reports was begun in 1883. Its object then, as now, was to give an account of the known mineral resources of the United States and present a statistical statement of the production of these materials and the uses to which they were applied. At that time the study of economic geology in the United States Geological Survey was greatly limited by insufficient appropriations from congress and by the fact that the training possible for an economic geologist in the leading colleges and in the geological survey itself was not sufficient to secure an efficient corps of trained men for studying in any comprehensive way the mineral deposits of the entire country. The cooperation was therefore invoked of those who were recognized as the best experts of the country for each of the mineral industries. In most cases this work was entered upon by the experts without the possibility of obtaining any such compensation as they would have demanded for private reports. In general, their services have been practically for an honorarium sufficient only to cover the clerical aid.

The London *Times* states that considerable additions have been made during the past year to the natural history collections of the Bristol Museum. The skin of Rajah, the fine tiger which lived for about ten years in the Clifton Gardens, was presented to the museum, for which it was well mounted by Rowland Ward, who also supplied about a dozen nesting groups of British birds, arranged on the plan in vogue at the Natural History Museum. The local collection of birds has been increased; Mr. H. J. Charbonnier presented a fine collection of two-winged flies, and another of bees and ants, collected in the district; and the display of game trophies has been made more interesting by collections of heads, horns and antlers deposited on long loan by friends of the museum.

We learn from *Nature* that the preliminary program of the second International Congress on School Hygiene, to be held on August 5–10, 1907, at the University of London, South Kensington, has been issued. The work of the congress will be divided into eleven sections, each presided over by an authority on the subject dealt with. The organizing committee is inviting educational and public health authorities, universities, colleges, schools, societies and others to appoint delegates to the meeting, and is appealing for donations to meet the large expenditure involved in organizing the congress, which it is estimated will be not less than 3,000*l*. The president of the congress is Sir Lauder Brunton, F.R.S., and the hon. secretaries are Dr. James Kerr and Mr. E. White Wallis.

Noting that all persons in the United Kingdom whose incomes exceed $850 per annum are assessed for income tax, Consul R. W. Austin, of Glasgow, summarizes an official report just issued, showing the amounts assessed for the year ended April 5, 1905, which indicates the various gross incomes in that country. Under the heading of businesses, professions, and employments, it appears that there were assessed 456,571 persons with gross incomes amounting to $624,349,605; 57,244 firms with a gross income of $420,-244,765; 30,129 public companies with a gross income of $1,204,046,240, and 9,582 corporations and other local authorities (assessed for profit or interest) with a gross income of $99,068,330. The total gross income was $2,-347,708,940. Dealing with the incomes of individuals the report shows that there were assessed in Great Britain 6,137 persons with incomes over $5,000 and not exceeding $10,-000; 1,405 with incomes of over $10,000 but below $15,000; 533 with incomes over $15,000 and below $20,000; 304 over $20,000; 442 with incomes over $25,000 but not exceeding $50,-000; 212 with over $50,000 but not exceeding $250,000; and 24 whose incomes exceeded $250,000.

According to a foreign journal, Dr. Erich Zugmayer, a young naturalist who has already gained some experience as a traveler during a journey in Western Asia, described in his book, 'Eine Reise durch Vorderasien,' has lately undertaken an expedition to Tibet, which he hopes to cross from north to south, returning through India. He had reached Polu, on the southern border of Chinese Turkestan, in June, 1906, having traveled by the newly opened trans-Aral railway to Tashkend, and thence proceeded *via* Kashgar, Yarkand, and Khotan. From the last-named place he had executed a route survey, making also astronomical and meteorological observations in addition to those relating to his more special subject, natural history. He had already sent back from Polu three cases full of the collections made during the earlier part of the journey. From Polu he hoped to cross the Shu-bashi pass, and to examine an area hitherto blank on the map before striking the route of Dutreuil de Rhins.

UNIVERSITY AND EDUCATIONAL NEWS

Through the recent death of the last child of the late John C. Frye, a Boston produce merchant, Tufts College comes into a bequest of $200,000.

It is announced that the late Dr. Gustave Schorstein bequeathed 500*l.* to the regius professor of medicine at the University of Oxford for the pathological department of the medical school to the London hospital, and a sum, which will amount to some 10,000*l.*, in trust to the University of Oxford, subject to certain life interests.

Nature says: "The report of the Board of Education for the year 1905–6 is of an encouraging nature. There is plenty of evidence provided that our national system of technical education continues steadily to improve. The report points out that much attention has been paid throughout the country to the extension and improvement of the facilities provided for continuative education. There has been marked activity in the establishment of courses of instruction affording special technical training, and the effective character of the many courses organized under varied conditions shows that local circumstances have received the consideration necessary for success in this kind of educational work. Technical institutions affording whole-time training for those who can give two or more years to study after completing a secondary school course have improved and multiplied their courses of technical instruction. The multiplication of courses, requiring the whole time of students is a gratifying indication of the growing appreciation of the value of the work of the technical school; but this appreciation is not confined to whole-time instruction. The improved organization of the varied institutions engaged in supplementing the training which a youth receives in the office or workshop has borne fruit in many practical developments, demonstrating the extent to which such further education may become a recognized element in the lives of our youths. The report, which runs to 106 pages, deals fully with every department of elementary, secondary and technical education, and shows conclusively that, political controversy notwithstanding, valuable work is being accomplished in the schools."

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, FEBRUARY 8, 1907.

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE INFLUENCE OF PARASITISM ON THE HOST[1]

THE line of development within the field of zoological research has shown a distinct tendency within recent years to move in the direction of biological study, to view the organism as a living thing and to seek an explanation for the various problems of life which present themselves in connection with it. One of the earliest phases of biological study found its origin in the condition presented by parasitism. The class of Helminthes, or intestinal worms, of the earliest authors, was seen early in the course of morphological study to be unwarranted as a systematic grouping. The animals included under the term were not those which were in any genetic sense related to each other. Like the earlier designations of land animals and water animals, these forms were grouped together by virtue of similarity in conditions of existence. The term is accordingly a biological one and its purely biological significance was stoutly maintained as early as 1827, by the great embryologist, Carl Ernst von Baer, and by F. S. Leuckart. The idea received finally due acceptance through the efforts of Carl Vogt, who dissociated the earlier group and united its subdivisions with those free living animals to which they were most closely morphologically con-

[1] Address of the vice-president and chairman of Section F—Zoology, American Association for the Advancement of Science, New York meeting, December, 1906.

nected. Even thus early it was apparent that parasitic animals were derived from free living forms, that they were in fact but degraded members of the same groups; in some cases with such little modification in structure that their affinities were recognized at a glance, in other cases, however, the excessive modification had carried the parasitic form very far from the ancestral type, and yet the existence of a large number of intermediate stages suggested at once that these changes had been gradual.

In spite of the fact that these studies were among the very earliest of biological investigations, it appears that the reciprocal influence has rather generally escaped discussion. It is my desire, accordingly, in so far as may be practicable within the limits of time and space of a single address, to set before you the main facts in connection with this other side of parasitism—the influences which are exerted upon the host animal, the changes wrought in it, and the part they play in the problems of general biology. There is a vast amount of information and detail regarding individual species; and in some cases the relations have been investigated with great care from the standpoint not only of the morphologist and of the physiologist, but even also from that of the chemist; but in the main these observations stand as isolated and unrelated facts. I have neither the knowledge nor the ability to bring them all together into a concrete whole, but there are certain general headings under which it is possible without especial effort or explanation to group some of the phenomena, while others must wait for better knowledge or clearer perception than I possess before they can be included in any generalizations.

In rough fashion it is the custom to class parasites as harmful and harmless. The harmful forms induce pathological changes, stand in some definite relation to disease and become, accordingly, of prime importance to the investigator in medical fields. The other forms, while spoken of as harmless or without effect, certainly should not be classed as exercising none. With the exception of certain striking instances, especially among those forms which parasitize man, the effects of parasitism are almost unknown, hence they are largely ignored. In the course of time opinions on this matter have changed radically. As I have said in another paper:

"In the belief of the medical profession two hundred years ago there was no disease, real or imaginary, which was not due to the presence and effect of some kind of parasite. Each ailment had its particular 'worm' in its characteristic location. This was a direct result of the endeavor to reduce every malady to some definite cause, and from a joining of the unknown sickness with the parasites, of which they knew as little. Under the influence of study and of increase in knowledge regarding the parasites, such a theory was seen to be untenable, and the movement in the opposite direction began—a tendency which may be said by this time to have passed its height. In fact, there has prevailed during recent years among the medical men of this country an exaggerated idea of the unimportance of human parasites." This must give way to a proper conception of the pathological significance of these organisms, based upon careful investigations of their actual influence upon the host.

Yet there are some parasites of which it may fairly be said that even careful study has failed to show any manner in which they affect the host. Thus Looss records of a distome (*Heterophyes*) commonly found in the human alimentary canal among Egyptian laborers, that, although present in considerable numbers, most careful scrutiny fails to disclose any influence which it exerts upon the host. This is traceable to the fact that it neither burrows into nor feeds upon the mucous lining of the canal, but contents itself with taking its food from the partially digested contents of the intestine. Inasmuch as the organism is very small, this is evidently a negligible factor in the economy of the host; but even here, as I shall show later, there is the possibility that under some circumstances the organism may become a menace. Again, *Filaria loa,* the African eye worm, lives for many years in the connective tissue of the human body, wandering from point to point, often not far below the skin. In the course of its migrations it does apparently no injury to the host, who is indeed unconscious of its presence until it happens to come into the connective tissue over the surface of the eyeball. Here it appeals to the sense of sight, and from here it has most frequently been extracted. But in this case again there are swellings which appear from time to time on the surface of the body and which are believed by some to be due in one way or other to this parasite.

In the group of indifferent bodies we must also include many, if not all, of the resting forms, those which, like the bladder worms, or the young trichinæ, are encysted at a given point and which consequently are not to any appreciable extent absorbing nutrition or producing toxic materials. The encapsulated trichina is but a grain of sand, the encysted bladder worm no more than a globule of fat in the tissue. During its entire life in the body the guinea worm does not seem to exercise any influence whatever upon the host organism until the female appears at the surface and is desirous of securing an opening through which it may discharge the young into the outer world, where they will find conditions for further existence.

These few preliminary statements have paved the way for a somewhat clearer general idea of the factors which determine the degree of influence exerted by the parasite on its host, and the first is evidently the numerical factor. Commonly, the single parasite leaves no effect; it is the multiplication of parasites which is to be feared. Evidently this multiplication will be most serious when it takes place within the host and leads directly to a multiple infection. This is known among the protozoa, where, in at least one host, there is usually a recurrence of generations of the same type, and this feature is so characteristic that Doflein designates this cycle of the life history as the multiplicative cycle, defining its purpose as being to achieve the multiple infection of the host. There are also some worms among the Nematodes in which the same thing takes place and by virtue of which the infection of the alimentary canal is enormously increased in direct fashion.

Among most metazoan parasites, however, including all Trematodes, Cestodes and some Nematodes, such is not the case. The eggs of the individual must be transported to the outer world before they can carry out their development, and they reach a new host after a more or less complicated life history which may involve alternation of generations and parasitism in one or more intermediate hosts, so that when the stage comes in which the infection of the first species is possible, it is very unlikely that the original individual will be reinfected. Among such parasites the effects from a single individual are not serious and the real danger lies in a multiple infection through the increase in numbers which such a species often experiences in the intermediate host, or within a limited area in the outer world, so that by the taking in of a single external object a large number of parasites may be introduced. Where alternations of generations exist the dangers of the parasitic existence are so great that the number of parasites is kept down. Yet under a favorable combination of circumstances the numbers of a given parasite may be enormously increased and with this the dangers from the species also enhanced.

Naturally, immediately related to the factor of numbers is the question of comparative size. In a general way the effects of a parasite are related to its mass as compared with that of the host, and this will be clearly manifest in the subsequent discussion. From a special point of view, however, this is absolutely untrue and the secondary effects of an individual species may be out of all proportion to its size. This will also receive more detailed discussion at a later point.

Evidently, both of the factors which have just been mentioned are largely relative and distinctly such with regard to the seat of the injury, *i. e.*, the organ of the body which is attacked; thus a parasite may be quite harmless if located in connective tissue, or between the muscle fibers, while if the same specimen were to be located in the brain or in the eye its effect would be very serious. A small organism would pass entirely unnoticed in the alimentary canal, but in the heart or blood vessel, it might well cause serious disturbances, or under certain conditions even the death of the host, so that the pathogenic significance of a parasite depends essentially upon its location.

The effects of a parasite on the host may be roughly classified as mechanical, morphological and physiological. To be sure, a sharp separation is not possible at all points and frequently the one influence involves concurrently or subsequently such as belong to another group.

Animal parasites, for the most part, produce only local changes and these are explicable on purely mechanical grounds, or they are structural or functional within limited areas. But physiological influences sometimes manifest themselves as symptoms of a general disease. Such manifestations ordinarily accompany the multiplication of the parasite within the host or at least its presence there in large numbers.

Many of the effects upon the host produced by parasites may be explained in purely mechanical fashion, and under this heading are included those which produce even some of the more complicated and far-reaching results of parasitic infection through the secondary effects which they induce.

Regarding the parasite as a passive mass, a purely inert body, it may bring about a stoppage of various passageways and thus induce serious consequences for the host. Such an occlusion of a canal may follow under perfectly normal conditions, but more frequently there is something unusual that tends to induce the condition; either that the parasite is in an abnormal location or that some factor enters into its normal habitat to aid in purely mechanical fashion in the stoppage of the passageway. Thus, the accumulation of a group of common round worms into a ball will suffice at times to occlude entirely the alimentary canal, and the worms, tangled and twisted together, stop the passage of food materials and waste. Unless the condition is accurately diagnosed and promptly alleviated by operative means, this stoppage of the canal results in the death of the host. Such has been the result in man in several cases on record and similar instances are more frequent among other hosts. But in one of the more carefully investigated cases it was found that the mass of seventy-two ascarids were inextricably tangled together by a long hair swallowed by accident and in some way twisted tighter and tighter through the contortions of the parasites.

The young sclerostomes of the horse live in the large arteries of the abdomen. Their presence in these arteries explains the aneurisms frequent at this point, as they obstruct the vessels and raise the arterial tension behind the points where they have formed a blockade. This induces in a purely mechanical way a distension of the vessels at the region of increased pressure.

Even much more complicated changes in the host may be reduced in final analysis to a mechanical effect. In the case of the Egyptian blood fluke, the female goes into the venous plexus of the pelvis to oviposit and the oval eggs which are supplied with a rudimentary spine at one end are carried into the capillaries by the blood current and there gradually work their way through the wall of the alimentary canal, or the urinary bladder, so as to reach the cavity of the organ and to escape from the body. The large numbers of these eggs which are produced fill up the capillaries, interfere with the flow of blood, lacerate the capillary walls and tissues through which they are forced mechanically until a series of serious changes is invoked in the organs in question. The condition is even more serious when these same eggs chance to be carried about the body of the host by the circulating blood, become entangled in the capillary network at different points and constitute the foci of small emboli. Such may arise in the brain where retardation of the blood current and the resulting emboli are sources of serious danger to the host, since they give rise to brain tumors and may evoke apoplectic symptoms. The effects produced by such brain tumors of parasitic origin do not differ from those of other tumors or other foreign bodies. All of these effects are at basis mechanical and the same results would apparently be produced by any inert bodies of similar form and number.

One may go still further and call attention to the fact that in some cases it is a mass of embryos which constitute the mechanical influence at the basis of serious changes. Some of the filariæ which inhabit the connective tissue are viviparous and produce countless numbers of embryos; these are carried by the lymph and blood stream all over the body; accumulating, evidently by chance, in considerable numbers at certain points of the lymph vessels, for instance, they act mechanically to produce lymph stasis and dilation of the parts. The long-continued working of this cause will produce an enormous distension of some regions of the body, giving rise to one form of the condition which is known as elephantiasis.

Another mechanical influence of the passive parasite is traceable to the pressure which it exerts upon surrounding tissues. In so far as the parasite is stable this pressure will be constant and its effect is of minor consequence; however, whenever the parasite is in a condition of active growth the gradually increasing pressure becomes an important element to consider, but the discussion of this falls naturally under a subsequent heading.

There are certain influences which the parasite exerts that are mechanical, and still are not traceable to it as a mere inert body; they rather are determined by its activity, and yet they properly deserve consideration under this head, for the parasite, though active, is exerting a purely mechanical influence, and one might conceive of the same results following upon the movement of any inanimate object. If the parasite moves about it tends to irritate and inflame or destroy the surface upon which it lies, even though it remains relatively fixed in location and merely twists from side to side. The irritation here will be evidently local and will be due to, or at least emphasized by, the spines or other roughness of the body. It will be also increased if the parasite possesses suckers or other organs of attachment. The delicate mucous surfaces with which the parasite usually comes in contact are of all parts of the body the most susceptible to injury in this fashion, and, as we shall see later, many parasites combine with the mechanical lacerations of tissue by movements and their hold-fast organs, a physiological influence of far-reaching import.

A complicated case which indicates the combined effects of mass and motion, with possibly additional features of a physiological character not yet well understood, is illustrated by the smaller liver flukes of the genus *Opisthorchis*. These species occur in the gall ducts of the liver and there they evoke changes which, though radical, are in general uniform for different species and hosts. As the flukes advance into the finer canals of the liver, the ducts become completely occluded. The first result is bile stasis and consequent dilation of the canal until this acquires considerable size. Both the epithelial layer and the connective tissue of its wall undergo profound modifications. The epithelium shows an active catarrhal irritation. The glands manifest considerable hypertrophy. Many new accessory ducts are formed alongside the original one. Among the secondary effects one may note that while the connective tissue proliferates actively and acquires enormous thickness, the liver tissue undergoes granular degeneration as the cells gradually atrophy, thus the functional portion of the organ is gradually replaced by inert tissue. The arrest of the bile is followed by digestive disturbances and compression of the branches of the portal vein produces venous stasis, from which follows naturally the ascites so frequently observed in the course of the malady. Now all of these modifications follow the mechanical stoppage of the duct naturally, though no doubt the effect is increased by the irritation produced by the movements of the parasites. Some investigators hold that the flukes feed upon the mucous lining of the ducts; this is true of other liver flukes, but according to my observations does not apply to *Opisthorchis*.

Parasites, however, not only carry on movements through their natural territory, but they also from time to time indulge in migrations, the causes of which are not clear, but the effects are serious in the extreme.

The extent of such abnormal migrations is well illustrated by *Ascaris lumbricoides*. This form has been known to migrate along evident pathways from the duodenum into the biliary ducts, and liver, where it has induced hepatic abscesses, or into the pancreas with like results, into the larynx and trachea with the result of suffocating the host, into the eustachian tube to emerge from the auditory canal after perforating the tympanic membrane, or even into the frontal sinuses, or the internal angle of the eye.

Such erratic parasites do not always confine themselves to normal passageways of the body. Even where the penetration of tissue is distinctly exceptional, it not infrequently happens that under some unknown stimulus the species brings itself to transgress natural limitations and to open an abnormal communication between regions otherwise entirely separated. Ascarids have perforated the intestine, penetrating the peritoneal cavity, have come out from abscesses at various points, or have been discovered on the occasion of a post-mortem in the most varied regions of the body. The liver fluke as it feeds upon the hepatic cells may chance to open a small vessel, or the lung fluke may similarly effect an entrance into the circulatory system and either be thus carried into distant and unnatural parts of the body, reappearing in abscesses, and in expansions of the eyelids, or being caught in brain tumors, which sooner or later arouse the disturbances that naturally result from the presence of foreign bodies in that organ.

The migrations of smaller forms, even though they may be numerous, are not accompanied ordinarily by the same effects as those of larger species, since the orifices they make are sufficiently minute to close immediately after the animal and prevent the secondary effects which are due to such abnormal connections. Thus the minute embryo of tapeworms migrate from the alimentary canal to the point of encystment without influencing appreciably the host, and other larvæ migrate through the tissues with such little disturbance that unless the numbers be large the host suffers no inconvenience.

Emphasis should be laid upon the extreme importance in the economy of the host which the secondary effects exert. The abrasion and destruction of surfaces and cells and the opening of abnormal communications is not *per se* of such vital importance as the results which may follow through the admission of bacteria from the canal into the blood and tissues of the animal. It is frequently held that the uninjured mucous surface is resistant to the action of bacteria and that typhoid and cholera germs must depend to some extent upon diminished resistance, functional or structural, for their original introduction into the tissues of the body. It is certainly true that many pathogenic organisms exist in the alimentary canal without detriment to the host animal, although if permitted to pass into other parts of the body they excite immediately dangerous symptoms. The perforations of the intestinal wall by *Ascaris* and the escape of such organisms into the body cavity gives at once the conditions for a serious if not fatal peritonitis, and *Ascaris* is not alone in this respect.[2]

Examples of this can be multiplied, but one will suffice. In the etiology of appendicitis certain factors are regarded as predisposing, others are direct causes of this malady. As early as 1724 Santorini recorded the presence of worms, probably *Trichuris*, in the appendix. Numerous later authors found at necropsies *Ascaris* and trichurids in this organ, as well as calculi containing eggs of *Ascaris* and *Oxyurias*. In 1901 Metchnikoff noted that in several persons who manifested symptoms of appendicitis when microscopical examination of the feces demonstrated the eggs of *Ascaris* and of *Trichuris*, the administration of a vermifuge effected a cure. He maintained that nematodes were the cause of many cases of this disease and explained the rôle of the parasites as first a direct mechanical or chemical action on the appendix and second an indirect action by the introduction of microbes into the mucosa. Metchnikoff did not commit the error attributed to him by some authors of regarding all cases of appendicitis as of parasitic origin, but specifically stated that there are certainly appendicites of different origin. Subsequent authors furnished additional evidence of the direct or indirect action of parasites in producing appendicitis, while others, though admitting the possibility that nematodes may inoculate the intestinal mucosa with bacteria, regarded this as an inappreciable factor in

[2] Piana was the first to note that the migration of *Cysticercus pisiformis* into the liver of the rabbit could introduce bacteria. In two cases of tubercular peritonitis of dog associated with *Dioctophyme renale* in the abdominal cavity Galli Valerio advanced the view that migration of the nematode made possible the development of the bacillus or carried it into cavity. It has recently been clearly shown that the pin worm, *Oxyurias*, has burrowed into the wall of the canal and produced there microscopic ulcerations, while it seems probable that it has actually made its way through the wall into the cavity of the pelvis. This perforating action places *Oxyurias* also in the ranks of parasitic introducers of bacteria.

appendicitis since their presence in this organ constitutes a pathological rarity.[3]

Evidently in producing ulcerations of the intestinal mucosa parasites facilitate the absorption of toxins from the canal and permit the inoculation of this layer with pathogenic bacteria from the intestinal contents. They can thus be the agents of inoculation for numerous diseases. Guiart, who defends this view most strongly, believes that intestinal parasites play an important rôle in the etiology of diseases of the intestine and liver, such as insects play in the etiology of blood infections. He advances evidence to support the view from the records of both human and comparative parasitology. While recognizing fully that the infections are bacterial, he emphasizes the necessity of some inoculating agent as in a sense the most important element, since pathogenic bacteria are generally present in the alimentary canal. No one can doubt, he maintains, that Eberth's bacillus is the agent of typhoid fever, but there is reason for regarding it as innocuous if the intestine is undamaged. In a population drinking contaminated water only a few persons in reality are infected. Any intestinal parasite capable of inflicting a wound may infect the host if the bacillus is present. The infecting agent may be an Ascarid, a hook worm, a fly larva; most commonly Guiart believes it to be the whip worm (*Trichuris*) which bores into the folds of the intestinal mucosa with its attenuated anterior end. This parasite Guiart calls the lancet of inoculation and demonstrated its presence in eleven out of twelve typhoid cases in one group. It is interesting to note that in 1762 this species was looked upon as the cause of typhoid and its abundant presence was noted in many epidemics by early investigators. After having gone to the opposite extreme, scientific opinion now shows a tendency to return to its earlier position and to regard the parasite an active factor in the introduction of the disease.

This unfortunate function as introducers of bacteria is, however, not confined to intestinal parasites. The chigo, or 'jigger,' a sand flea of some tropical regions, which burrows into the feet of natives, renders its host thereby exceedingly liable to infection, which in its secondary effects in the tropical clime, makes of a trivial injury one of serious consequence. The guinea worm, which burrows through connective tissue, approaching the surface at the time when it desires to deposit its embryos and producing there small ulcers or openings to the exterior, menaces the well being of its host, not by virtue of its own activity, but through the chance for infection to which it has subjected the host.

Parasites in many cases produce morphological or structural changes in their hosts, which may be classed in general as progressive histological changes in that they lead to the accumulation of material through the multiplying of the host cells.

Billroth indicated the extremely important fact that the multiplication of epithelial cells is caused by infection with micro-organisms and it has been shown that protozoa as well as bacteria may cause infected cells to multiply. Thelohan has shown that myxobolid infection produces a proliferation of muscle cells. Hofer and Doflein ascertained that in kidney infec-

[3] Galli-Valerio has subjected a recent fatal case of appendicitis to most careful examination and finds in the contents of the perforated appendix numbers of *Oxyurias vermicularis* and eggs of *Trichuris*, while sections demonstrated numerous perforations of the mucosa made apparently by the parasites and in one case the worm still within the tissue. Neither the presence of the parasites nor the evidences of their work could have been determined without a microscopic examination, and in view of the usual lack of such examination this is sufficient answer to the objections cited above to the probable rôle of parasites in the etiology of this disease.

tion by myxobolids increased growth in the skin resulted. It is also a distance effect, if not so great, when according to Leuckart the epithelial cells in coccidial nodules of the rabbit liver increase strongly in numbers. Among the epizoa there are many which produce such growths in the epithelium. The various species of itch mites excite a proliferation of the cells until there are formed crusts or thickened masses of considerable extent in which the galleries of the mites are constructed. These proliferations are said to be due merely to the mechanical stimulus exerted by the mites, and the crusts are formed by the addition of serous exudates. Such formations are thoroughly characteristic of the work of these parasites. Less well known are other cases of the same type such as that of gall-producing copepods which infest actinia.

But such stimulation is not always purely indefinite. Parasites often produce unusual forms in the region of the host in which they reside; thus, Woodworth, examining a skull of the common skunk, found that prominent swellings in the frontal bones near the sagittal plane were due to a nematode. The bone was extremely thin and in the subjacent frontal sinus lay a nest of fifteen to twenty of these parasites. The belief was expressed that the prevalent frontal enlargements of other American Mustelidæ are probably due to the same parasite.

Perhaps the most common form of morphological change on the part of the host is the production of a cyst about the parasite. It is composed in most instances, in part at least, by the host animal, and consists in its simplest form of an enveloping mass of connective tissue. This reaction against the invasion of the parasite is found in almost all groups.

The formation of pearls appears to be due regularly, if not exclusively, to the intrusion of parasitic larvæ. The host responds to the mechanical or chemical stimulus of the in-wandering larva by producing an epithelial sac which surrounds the parasite. Such a cyst formation by the host very generally follows when a parasite settles down to enter upon a resting stage in the body of the host. But here the character of the cyst wall leads normally to the formation of a deposit of lime of the same sort as the inner lining of the mollusk shell and the larval parasite becomes the nucleus of a pearl. This stimulus to pearl formation is not given by any particular species of parasite, but is traceable even to members of different classes of animals. Thus in European mussels pearls are formed about the cercariæ of Trematodes.[4]

But in the Ceylon pearl oyster, which is more nearly related to the mussels than to the oyster, the formation of pearls is due to certain cestode larvæ which undergo a portion of their development in the tissue of the liver, gills and mantle of the pearl oyster. Of these larvæ, such as for some yet unexplained reason do not succeed in carrying out their life cycle, become immured in the center of a pearl. Shipley and Hornell, who have investigated with success these phenomena, say "economically these unpleasant little creatures are of supreme importance to the Ceylon pearl fishery, as their presence in the oyster causes the formation of the finest quality of pearl and those of the highest luster." Another economic factor may be merely noted in passing. In 1859 Kelaart called attention distinctly to the possibility of infecting other beds with the larvæ of the pearl-producing parasites in order to increase the quantity of pearls. Beds of

[4] In one case carefully elucidated by Jameson the adult fluke inhabits the eider duck and the scoter, its sporocyst occurs in the cockle, while the tailless cercaria is found in the mussel and forms the nucleus of the small, lusterless pearls of that species. Another trematode is the cause of pearl formation in the fresh-water anodons.

Ceylon pearls might thus be grown in other parts of the world. Kelaart says that the nucleus of an American pearl, drawn by Möbius, is of nearly the same form as that found in the pearl oyster of Ceylon.

It is interesting to note in comparison the record of Johnstone that in the gurnard small pearl-like bodies are found adhering to the peritoneal investment of the intestine. The concretion is made up of a great number of concentric lamellæ which seem to consist of wavy bundles of connective tissue. These structures are probably derived from *Tetrarhynchus* cysts from which the larvæ had escaped and which had then undergone calcareous degeneration.

The amount of growth may be much greater than thus far indicated and a condition among animals closely analogous to the formation of galls on plants has been observed by Nutting. In certain Alcyonaria a tunnel formed of excessively enlarged spicules is found along one side of the stem or branch. The abnormal structure is due to an annelid. Greatly enlarged polyps in another genus owe their origin to the presence of crustacea or some other form of parasite.

Continued efforts have been made to demonstrate that the extensive pathological growths found in animals are due to the stimuli exerted by minute parasitic forms. It has been shown that in plants such abnormal growths are due to the encroachments of parasites; but the effort to identify animal organisms as the cause of cancerous and other abnormal growths has in the opinion of most investigators failed to establish a case.

When we seek to ascertain the causes of the morphological changes which result from the influence of the parasite on its host it is difficult in the present state of knowledge to find any very definite explanation. Localized growth is ordinarily a factor of differentiation, but here it has no reference whatever to that process. Davenport in his admirable treatise on experimental morphology has given a very clear discussion of the factors in the effect of chemical and physical agents upon growth, and has brought together the evidence which shows the acceleration of growth by contact, by cutting and by chemical stimulus. Now an examination of the cases in which growth is induced by parasites leads to the conviction that neither contact nor cutting can be a stimulating factor, since there are too many cases of parasitism in which no growth is produced, while in the exceptional case the presence of the parasite stimulates considerable growth activity. To be sure, it has already been suggested that the formation of a cyst about a resting parasite is a normal activity of the host and occurs very generally if not universally. This may be the result of a contact stimulus; it may also be explained on the same basis as the other cases, viz., as a result of chemical stimulus. Under the discussion of the acceleration of growth by chemical stimulus, Davenport says: "It is clear from this table that the addition of even small quantities of innutritious and poisonous substances may so affect the hylogenic processes as to cause twice or even far more than twice the normal formation of dry substance in a given time, and that this excessive growth increases with the concentration of the poisonous substances up to a certain optimum beyond which growth declines again to below the normal."

In suggesting that the stimulation of growth by parasites is traceable to chemical stimulus, and that the stimulating substance is a poison, I am advancing a hypothesis which, even though it is a purely tentative one, may well engage our consideration for a moment. Each parasite in the course of its activities produces a certain amount of waste material. It may safely be affirmed that this substance, which in the case of a resting parasite must be discharged directly into the tissue of its host, is to that tissue poisonous. If such material is given off in small amount it will evidently act as a stimulant upon the surrounding cells and be the factor in bringing together the leucocytes which accumulate about the invading organism. An inert body of the same size, if introduced into the same position, would not induce the formation of a cyst or certainly the tissue proliferations which accompany in some cases the attacks of parasites. It may even be doubted whether under the conditions for pearl formation, grains of sand, as formerly believed, will give the proper stimulus for the formation of the pearl. The important factor then must be one which is associated with the organism, not as an object occupying space, but as a living thing, and the most evident characteristic of this is the giving off of waste matter. This leads naturally to the next subdivision of the subject.

As physiological effects may be grouped together those influences of the parasite on the host which express themselves in the limitation or modification of the normal physiological processes of the latter. In some cases it is often true here that the primary effect is hidden and that the secondary result alone can be seen. It is sometimes possible to determine the primary result by study of the secondary, and to see the way in which it has been brought about. In other cases we merely know the secondary effects without being able to disentangle from the complicated series of phenomena the primary cause. Furthermore, those interferences with normal function which are grouped under this heading are not such as might be traced to the action of the parasite as a foreign body, but such as are related to its own activity as a living organism. It will be seen that the distinction here is not perfectly clear, and perhaps somewhat artificial, for the mechanical disturbances of parasitism interfere with the normal activities of the host in the same way that other foreign bodies may modify or limit the working of the same organism. However, it is my intention under this heading to consider the results which come from the contact of life with life, the interaction of function with function.

Perhaps the most evident factor and the most frequently mentioned, certainly the first to be noted in this connection, is the absorption of nutriment. The parasite demands a certain amount of food matter to carry on its own vital processes. This food matter is furnished, so far as endoparasites are concerned at least, in a partially or fully digested condition, by the host animal. Many observers have maintained that the actual loss to the host in this way is so slight as to be negligible.

Leuckart says that a *Dibothriocephalus latus* 7 meters in length weighs 27.5 grams, and gives off in a year a total of 15 to 20 meters of proglottids of a weight of approximately 140 grams, and that *Tænia saginata* produces daily on the average 11 proglottids, an amount equal to about 550 grams. These figures have been taken by some to indicate the actual loss of food material on the part of the host; this would be evidently insignificant in comparison with the amount consumed by a man within the year. It has even been suggested that the increased appetite induced by the presence of the parasite more than compensates for this slight loss. It would appear, however, that such a method of statement is exceedingly inadequate. It is very difficult to estimate the amount of food consumed by an animal in proportion to its weight, but it is certainly grossly insufficient to indicate this in any way as commensurate with the amount of growth which the animal achieves within a given period.

There is also another factor which must be taken into account, viz., that the parasite consists almost exclusively of actively functioning organs, and that there is a minimum of such inert parts as skeletal structures which add greatly to the weight of an animal, but do not involve the consumption of food material for their maintenance. It has been shown recently that parasites contain a large amount of glycogen; in analyses from one fourth to one third of the dry weight consists of this material. The highest value previously known was 14 per cent. in *Cardium* (a mollusk) and mammals apart from the liver have only about 3 per cent. Later the same author (Weinland) demonstrated that the vital processes of *Ascaris* proceed without oxygen and involve a fermentation process, resembling that of bacteria and yeast in producing butyric acid and alcohol. The possibility of such a wasteful process, measured in calories actually used, is found in the especially favorable conditions of existence of the parasite, which afford it carbohydrates in superabundance and both protection and warmth from the host. But naturally the waste of the process means draft on the host.

Furthermore, the presence of the glycogen, which is unquestionably reserve material, indicates unmistakably that metabolic processes are exceedingly active in the organism. Another indication of the same thing is to be found in the reproductive activity. The tapeworm is producing and maturing an enormous number of eggs, each of which is supplied with a considerable yolk dowry. It is safe to affirm that during reproductive periods the draft of any animal upon its food supply is at a maximum. All of these three items, then, the rapid growth of the individual, the production of an unlimited supply of reserve material and the extreme reproductive activity, point to a heavy draft upon the host. I have been unable to find any calculations which might be applied to such organisms as parasites with any likelihood that they would yield even approximate estimates of the material actually consumed. It would seem clear that previous calculations are incomplete and that the draft on the host is far greater than has been imagined heretofore.

An important effect on the host is traceable to the increase in the size of the parasite. This normal accompaniment of growth is most significant when the parasite occupies a limited space or when the increase in size is marked. The brain cysticercus, ordinarily found in a ventricle, grows until the cavity is occupied and then pressure upon the nervous tissue brings a sudden end to the life of the host. A liver hydatid may continue to grow almost without limit and only when important structures become involved is the pressure of serious moment. Most bladder worms do not exceed insignificant limits in growth and consequently exert little or no influence on the host. The echinococcus, through its indefinite growth, is sure to reach a size which will interfere with the activity of the host, and from its usual location in the liver is likely to include important vessels, or nerves, and lead to fatal interference with normal functions.

Another general influence which parasites are thought to exercise on the host is explained by the hypothesis of reflex nervous action. According to this view intestinal parasites affect the host by irritating the nerve terminations and provoking in reflex fashion the varied troubles of helminthiasis which the clinician recognizes. This is a rôle which has been regularly ascribed to them and yet, as Guiart says, this view is in fact a pure hypothesis. It has been invoked as an easy way to explain the results of parasitism, and though attractive by its very simplicity, no evidence has been elucidated in its support, while at the same time many grounds have been advanced for other views. Accordingly, it is my intention to pass this hypothesis with the mere mention.

It has been frequently noted that parasitic infestation tends to retard the development of the host organism. In fact it does not hinder the general growth of the host strikingly, but arrests primarily its sexual development. This has been especially investigated by Alfred Giard, who denominates the phenomenon parasitic castration and defines it as the sum total of modifications produced by a parasite on the reproductive apparatus of its host, or on the portions of the organism indirectly in relation with that apparatus. The phenomenon appears to be wide-spread, instances being found in all branches of the animal kingdom and as the result of the most varied sorts of parasites. The character of this influence becomes evident through a simple comparison.

When an organ acquires undue importance one of the first physiological results of its hypertrophy is the arrest of sexual reproduction. Similarly when a parasite develops in an organism it causes sterility in its host. Such parasitic castration may be direct or indirect. The first case is met when the parasite destroys directly by mechanical means, or for its nutrition, the genital glands of its host. Parasitic castration is indirect when the producing parasite is not directly in relation with the genital glands, but in some other part of the body of the host. Parasitic castration may also be temporary and then disappear when the parasite is suppressed.

The modifications caused by parasitic castration affect the genital organs, the secondary sexual characters and the sexual instincts of the infested animals; it may be partial to any degree and may exert the same influences on the secondary sexual characters as age or as artificial castration. Each one of the sexes loses more or less its characteristic attributes and tends to acquire in the same degree those of the opposite sex. Thanks to the effective labors of Professor Giard, this phase of the influence of parasites on the host is more carefully worked out in comparative fashion than any other in the entire category. However, it would be clearly impossible within the necessary limits of this address to present the mass of evidence which he has collected from all parts of the animal and plant kingdoms.

A change which is mechanical to a certain extent and yet which has results of far deeper character, is the destruction of tissues by such parasites as actually feed upon the cells of the host animal. This is evidently due to the functional activity of the parasite and for that reason will lie outside of the limits of a purely mechanical process and may be considered here. Schaper has shown that the liver fluke (*Fasciola*) feeds upon the substance of the organ in which it lives. Now this destruction of liver tissue removes from functional activity a certain portion of a most important organ. In addition to that it is followed by a growth of connective tissue, so that there is a permanent loss in the functional activity of the organ. Many different kinds of alimentary parasites actually feed upon the wall of the canal, with the result that by the feeding and burrowing of the parasites through the mucosa this important structure is destroyed over considerable areas; the wall of the canal comes to be covered with ulcers and suffers at these points permanent functional injury. The serious secondary effects in such cases have already been noted and they are evidently not in any sense directly proportioned to the extent of the injury, but the primary effect upon the functional process of the organ is directly related to the number of such injuries, or, in other words, to the number of parasites at work. Other parasites burrow into the intestinal wall and produce there nodules of various sorts. Nematodes particularly determine the formation of such nodules in various organs of the host.[5] These nodules may be of considerable size, but in any event they involve the destruction of some functional tissue and the consequent impairment of the functional activity of the organ.

Among the physiological activities of parasites none is more striking in its reaction upon the economy of the host than the power that has been acquired by some forms which live upon blood to secrete a substance that inhibits the coagulation of the blood. Leo Loeb and Smith have recently shown that the hook worm secretes in glands of the anterior body region a substance which is exceedingly effective in inhibiting the coagulation of blood. This is antagonistic to the normal reaction of the mucosa. Consequently the points at which these parasites have attached themselves to the membrane become seats of continued hemorrhage, and in case of a numerous infection by the species there are myriads of minute hemorrhages constantly discharging blood[6] into the cavity of the canal. The powerful anemia which is associated with the parasitism of the hook worm receives in this way a ready explanation.

There are many cases in which production of diseased conditions in the host appears distinctly traceable to the presence of a parasite and removable by the removal of the parasite. These diseased conditions are general, not local. They are apparently due to some abnormal stimulation and have usually been explained on the basis of the influence of toxic materials which the parasite produces. That parasitic organisms, like all other animals, produce waste matter and give it off into the fluids by which they are surrounded is not open to question. If the view already advanced of the active metabolism carried out by the parasites, and if the extremely highly developed excretory system are correct indications, then the amount of such waste material eliminated would be proportionately large. It is also undoubtedly true that various investigators have been able chemically to isolate toxic principles from the bodies of various parasites, and that in a number of instances these substances have been tested in their effects on living organisms with the result of producing changes or invoking symptoms distinctly analogous to those which have been recorded as the consequences of infection by the specific parasite. It also appears that the evidence which has been collected heretofore seems to indicate a difference in the degree of the effect exercised by living and by dead helminthes, for the latter are much more

[5] Thus *Sclerostoma equinum* produces tumors on the intestinal lining of the horse; *Spiroptera megastoma* in the submucosa of the horse; *Spiroptera sanguinolenta* in various organs of the dog, fox, etc.; *Œsophagostoma columbianum* in the intestine of sheep; *Strongylus Ostertagi* in the fourth stomach of cattle; *Gnathostoma siamense* in the subcutaneous tissue of man.

[6] As a matter of fact Looss has demonstrated that blood is not the normal food of the hook worm, as these parasites feed on the mucous membrane of the host, and blood is sucked in only when the parasite accidentally pierces a vessel. Looss takes the position that a toxin is produced which acts hemolytically, pointing out the fact that in some fatal cases of severe anemia the number of parasites found in the intestine was too few to explain the severity of the illness. It does not seem to me to be necessary to infer the production of some unknown toxin, since the possession of a secretion inhibiting the coagulation of the blood would account for the persistence of the hemorrhages, and it is this factor of continuance which makes of them dangerous elements. It is well known that the leech also produces such a substance in its glands.

dangerous than the former; and yet it should be remembered that our precise knowledge of these matters is exceedingly limited. The production of a substance by extraction from a parasite is not sufficient evidence that the animal actually eliminates this material. It may be that in the final step of elimination there are changes which radically alter the character of the substance, and that consequently there is under natural conditions no such material in position to act upon the host individual. It should also be remembered in this connection that the parasites are generally located in those organs into which waste is normally eliminated and by which it is discharged from the body; and, furthermore, that the parasitic organism sets free but a small amount of such waste material at any one time. These conditions would appear to indicate a minimum effect upon the host, if, indeed, any such existed.

On the other side, there are also certain incontestable facts. Not only is the recovery of such toxic substances from parasitic organisms by chemical means undoubted, but also it is known that as a result of injury or surgical intervention when a hydatid cyst is ruptured and the liquid content is diffused through the body, there are absorbed from it toxic substances which provoke serious results. Normally, the bladder worm is surrounded by a cuticula which retards osmosis, so that only a negligible quantity of toxin can be dispersed, while the rest is accumulated in the vesicle.

Another argument which has been advanced to question the actual relation of these toxic substances to the pathological conditions of the host is the great amount of variability in different cases, not only as between the effect of different parasites which would naturally be explained on the basis of different types of toxic substance, but also with regard to the effect of the same parasite in different cases. Now this may easily be due to individual susceptibility on the part of the host animal, but there is another feature which would go far towards explaining this variable effect and yet it has never been suggested in this connection. Under certain conditions an animal will absorb toxic substances from its own alimentary canal and induce diseased conditions within itself as a result of this perverted function. The application of the same principle to the case of parasites would account for the absorption of toxic materials by one host and their elimination in another case.

So far as the protozoa are concerned, the evidence is positive that in some cases toxic materials are the cause of the effect produced by the parasite. Thus the Hemosporidia of malaria undergo their development within the red blood corpuscles, and at the time of breaking up into spores the corpuscle is destroyed and the accumulated toxic material set free in the blood. Since the phenomena are synchronous for a large number of the parasites the amount of toxic material set free at once is considerable and is followed immediately by a febrile reaction, the periodicity of which is related to the successive reproductive cycles of the parasite.

The case of the Trypanosomes which are the cause of sleeping sickness is less definitely demonstrated, but apparently of the same type. In this disease no pathogenic changes can be observed in the host save a slight inflammation of the vascular membranes surrounding the spinal cord and brain. The toxins which the organisms in the spinal fluid produce must be set free in this fluid and thus act directly upon the central nervous system with the lethal effect characteristic of the disease. The result here is certainly not morphological and is satisfactorily explained on the basis of the production of a deleterious chemical stimulation.

But little is known regarding the chemical character of the substances under dispute. The indefinite idea that some toxic substance is produced has been replaced in one or two instances by a definite idea regarding the material and its manner of working. Such is the case in hook-worm disease, already discussed, and it may be that more extended study will furnish clearer ideas with regard to other cases. Some observers have determined the substances obtained extractively as leucomaines, while in other cases they have been found to be more nearly like enzymes. Certain of them react upon the blood with marked hemolytic power, while others of the ferment character affect nerve centers. An alkaloid which arrests muscular action has also been isolated.

The most difficult case to explain on any other theory than that of the production of toxic principles is the progressive, pernicious anemia, present in some cases of bothriocephalid infection. In spite of numerous investigations the case still remains obscure, but apparently one must admit that at least in special instances this parasite does excrete a particularly active toxin having hemolytic properties. Undoubtedly, the production of toxins by bacteria leads us to expect similar substances in this case also, but the argument from analogy is a dangerous one in scientific demonstrations. Perhaps the strongest argument in favor of the view that parasites produce toxic substances is to be drawn from the occurrence of eosinophilia, which will be considered next.

Among the white blood cells are such as from the affinity of their granular contents for certain stains are known as eosinophile cells. They constitute from 2 per cent. to 4 per cent. of the normal leucocyte count, and an absolute increase beyond the normal number of 250 per cubic millimeter is designated eosinophilia. The conditions which produce an eosinophilia vary so widely that the cells have been termed the most capricious element of the blood. To a certain extent this seems true of the many varied reports of parasitic invasions on record, as some observers in the case of almost every species record the absence of any eosinophilia. Yet evidence is growing that eosinophilia is a strikingly constant symptom of infections with animal parasites, and experiments on lower animals as well as the most careful and extended observations on man are nearly uniform in their testimony to the existence of abnormal numbers of eosinophile cells in the blood.[7]

Some authors indicate what may be the explanation of the negative results of other observers. Thus Calvert noted in cases of *Filaria Bancrofti* that the development of the eosinophilia followed a cyclical course, being more marked when the embryo round worms are absent from the peripheral blood and decreasing as the embryos increase these. The percentage of eosinophiles varied in one case from 3 per cent. to 15 per cent. during twenty-four hours. The observation has been confirmed exactly by Gulland, while other observers record similar or greater variations, though the minimum figures are higher. In fact, such fluctuations seem characteristic not only for this parasite, but also of most other species. In some cases the eosinophilia does not make its appearance at the first of the infection, and after a marked increase subsequently declines or even disappears as the disease becomes chronic. A reappearance of the eosinophilia and also irregular fluctuations in it during the course of the

[7] Thus tapeworm infection, hydatid cysts, the Egyptian blood flukes, and many other parasites, are associated with an increase in eosinophile cells in the blood. In trichinosis it is the rule and in hook-worm disease the main feature of the blood is an eosinophilia both relative and absolute.

malady are also noted. Such variations have been recorded in infections with tapeworms, with hook worms, with trichinæ and with various filariæ. To what extent predisposition and immunity influence the phenomenon can not be judged at the present time, owing to insufficient data and incomplete observations.

Equally characteristic with these fluctuations one must regard also the excessive degree of eosinophilia in parasitic diseases. While under other conditions an eosinophilia of 7 per cent. to 10 per cent. is usual, with rare instances of 35 per cent. to 50 per cent., in infections with animal parasites 10 per cent. to 30 per cent. is the average and 70 per cent to 80 per cent., or even 87 per cent., the extreme. The number of cases with such high percentages is large, even though the records in general are not numerous, indicating even more distinctly the usual character of the exceptional eosinophilia in such cases.

The intimate relation of the eosinophile cells to the parasite is strikingly indicated by two well-established facts. The first already referred to is the increase and decrease of these cells in the peripheral blood as the embryos of *Filaria Bancrofti* appear in the superficial capillaries and disappear from them. The second was established by Opie experimenting with trichina on guinea-pigs. The increase in eosinophile cells does not begin until the embryos start to migrate and reaches its maximum when the majority of the embryos are in transmission from the intestinal mucosa by way of the lymphatic vessels and the blood through the lungs to the muscular system. Sabrazes also found a great accumulation of eosinophile cells in the vicinity of hydatid cysts. All these facts would seem to indicate not only a stimulus, probably chemical, inducing the multiplication of the cells, but also a positive chemotactic influence which led the cells towards the source of the stimulus. Since the stimulus originates from the parasites, the simplest explanation finds it in the normal emanations of the animal, which as waste matter may be classed under the general category of toxins.

Among the physiological effects of parasitism is listed prominently the production of a condition of deterioration in the blood known as anemia, involving changes in the red blood cells and in bone marrow. In certain cases it has been possible to indicate with some definiteness the cause, as in the hook-worm anemia, already discussed. External parasitism of blood-sucking species, such as leeches or fish lice (argulids) produces anemic conditions through direct appropriation of blood, and if the parasites become numerous, enough is withdrawn to cause the death of the host.

But after the elimination of these instances there remain others in which an explanation can not be given at the present time. The most striking example of such cases is that of the broad fish tapeworm (*Dibothriocephalus latus*) frequently associated with a severe form of pernicious anemia which, indeed, is given the name of a bothriocephalus anemia. Also *Oxyurias* and *Ascaris* have been found less frequently in connection with the same pathological condition, though the connection is not satisfactorily demonstrated. Now in all these instances the amount of departure from the normal may vary from a very slight anemia to the maximum degree, while in many cases there appears to be no such effect from the presence of the parasite. The condition is also distinguishable from pernicious anemia due to inherent causes in that it disappears promptly with the expulsion of the parasites. Experiments made with an extract of the *Dibothriocephalus* injected into various animals have been successful in some cases in producing an anemia, but in other cases have failed. The reason for this anemia is not loss of blood, and equally contrary to known facts are the various hypotheses, based on the length of stay in the intestine, the predisposition of the host and the condition of the parasite. The view that it is due to a toxin seems at present least open to criticism.

The discussion which has been laid before you in this address involves many terms which are rarely used in zoological circles, and many animals which are perhaps equally unfamiliar. To the average zoologist parasitism is a *terra incognita,* if not a *terra evitata!* The biological problems it presents were among the very first to be indicated, but have not received their proportionate attention in the intervening years. Just now there seems to be awakened interest in the subject and the results of investigations in this field are most hopeful. The subject is one which really overlaps the boundaries of zoology and encroaches upon the field of physiology and of medicine also. Much fine work has been done on the medical side of the topic, but the best results there can not be reached without generous cooperation from this side also. It is eminently fitting in this place to recall the splendid researches on morbific Protozoa carried out by a zoologist on the faculty of Columbia University. There is urgent need for similar work on other groups that the medical investigator may be furnished with those morphological, physiological and biological data upon which the successful prosecution of his studies depends. The work of the zoologists, Howard and J. B. Smith, on mosquitoes made possible the scientific victories of American physicians over disease in Havana and New Orleans. The recognition of hook-worm disease as an important factor in American medicine came through the pioneer work of the zoologist, Stiles. The splendid investigation of Councilman and his confrères on smallpox was not complete without the work of a zoologist, Calkins. The triumphs of modern science are being won by cooperative efforts and these are nowhere more indispensable than in the study of animal life, so peculiarly and closely related is it to the progress of the human race. At no point, however, is the contact more intimate than here where the zoologist is called to join with the investigator in medicine in achieving the amelioration of man's physical condition and the suppression of disease.

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SCIENTIFIC BOOKS

The Wing Veins of Insects. By Professor C. W. Woodworth. University of California Publications, Technical Bulletins, College of Agriculture, Agricultural Experiment Station. Entomology, Vol. I., No. 1, pp. 1–152, September, 1906. Contributions from the Zoological Laboratory of the Museum of Comparative Zoology at Harvard College, under the direction of E. L. Mark. No. 181.

Probably no animal organs have been so minutely compared externally as have the wing veins of insects. Comparison is so easy, so unhampered by preliminary technique, and the significant characters are so tangible and withal so useful, they are universally employed in defining both major and minor groups. There are probably no animal organs that are dealt with in a specific manner by so many workers in zoology. Therefore, when there appears a pretentious work that assumes to extend the knowledge and advance the theory of venation it attracts immediate and wide-spread interest.

Such a work is this recent one by Professor Woodworth. Its purpose is "to develop a theory that will serve for the interpretation of the facts that have been so richly accumulated" (p. 3) and "to establish a rational theory of venation" (p. 143). It aims to cover the whole field, discussing, in order, the origin of wings, their relation to the body, the mechanics of flight, the basal articulation, vein structure, vein development, the genesis of venation, methods of modification, and venation types. It presents some new and interesting data on the nature of degenerate and metamorphosed veins, and more especially on the mode of articulation of the wings with the thorax; but, in the main, it is a purely theoretical discussion of certain classes of facts previously well known. Professing to be based on studies of twenty-two years, on "microscopical preparations of about two thousand species representing all the principal groups, a much larger series of insects with spread wings, and practically all the published figures of insect wings," it is singularly parsimonious of new facts.

The theory is, in brief, that insect wings have arisen from tracheal gills, that the veins are inherited from gill covers and have no connection with the tracheæ inside, but that "mechanical necessities are the dominant factor in their first production and in their subsequent development," and that the venation collectively evidences three major groups of winged insects, called by the author Neuroptera, Elytroptera and Neoptera.

Gegenbauer's theory of the origin of wings from tracheal gills is accepted, and is defended probably about as well as is possible in absence of good evidence. The gill cover of a single species of Mayfly larva of the genus *Rithrogena* is used in illustration. Unfortunately, this gill cover is one of the most highly specialized for its own peculiar functions, and is quite off the line of possible wing development. Its basal articulation is said to be like that of wings, but it is not described. The diagonal brace across its lower face, cited as 'most convincing evidence' of its relation with wings, would be much less convincing with a little more knowledge of gill covers. Still, notwithstanding that the choosing between theories in this field is a matter of balancing remote possibilities, the presence of basal articulation and musculature in gill apparatus gives the Gegenbauer theory an advantage over Müller's lateral expansion parachute theory. But it does not appear why the suggestions of Dr. Tower, published some years ago in this same series of contributions, should not have been noticed.

The author's treatment of the wing tracheæ is somewhat remarkable. He reluctantly admits that these air tubes sometimes grow in wings, but he does not allow them to appear in any of his wing figures—only in the Mayfly gill cover; and there, apparently, because not coincident in position with the brace which he thinks is like a primitive vein. He seeks by argument as labored as it is unnecessary to prove that they have no air-taking function, while quite ignoring their air-distributing function. Everybody knows that in insects air is carried to all the living tissues of the body, not in blood vessels as in vertebrates, but in tracheæ, and the equanimity with which, throughout this whole discussion, these principal visceral organs of the wings are excluded from consideration is most remarkable. A few quotations will illustrate this: "In many wings, at least, tracheation is a comparatively late and entirely secondary matter" (p. 7). Quite true; but in what wings? In none but those of a few highly specialized groups. Teeth are absent from the jaws of some mammals; but their absence is not better accounted for, nor more disturbing.

"In studying the development of veins, we need only to take into consideration the constant features of the developing wing, the hypodermis of the wing pad and the cuticle that it secretes" (p. 52).

"*Accidents* of structure of a temporary adaptive organ" (p. 62). The italics are mine.

"The venation is conceived of as receiving nothing from the precursor of the wing except veins that were developed in the same way and to meet the same needs with those of the organ after it became adapted to flight" (pp. 144).

He laments that in adopting the Müllerian hypothesis Packard was "not entirely able to divest himself of his former idea of an essential connection between tracheæ and veins" (p. 62). Alas, Packard was not able; neither was that 'prince of entomologists,' Dr. Hagen;

nor was Dr. Brauer; and, doubtless, many another in days to come, observing the individuality and persistence of the tracheæ, and the regular formation of the veins about them, will be unable to ignore them with the serenity of the author.

For, in primitive insects, tracheæ develop first, and the veins later develop about them. That is not theory, but fact; any one may easily see it for himself. The very presence of the tracheæ between the two membranes of the wing when these are fusing sufficiently accounts for the primary location of the veins. Moreover, these tracheæ in generalized wings show all the usual signs of homology: likeness in form, likeness in relations and greater likeness in earlier than in later stages. If these are not good evidence, there are no homologies of any significance. Furthermore, the homologies discovered in the tracheæ are fully corroborated by those of the adult veins, previously ascertained.

The author concludes his argument for the exclusion of the tracheæ as follows: "A large amount of very strong evidence would be needed to explain away the essential identity of structure in tracheated and non-tracheated veins; the evidence obtainable seems to indicate identity rather than difference. We must conclude, then, that the presence or absence of a trachea is an incident of structure of no special significance in comparing veins" (p. 47). Precisely. Just as the presence or absence of teeth in mammalian jaw bones is of no special importance in identifying the bones. Not the presence or absence of either teeth or tracheæ is of chief significance, but the form taken on when present.

The precedence of the tracheæ and the subsequent development of the veins about them is disposed of in this way: "It is impossible to deny that the location of the veins may have been really marked out, though unrecognizable to the eye" (p. 46). This position is, of course, unassailable. Those who can attain to equal faith may find equal security in it.

If a 'rational theory of venation' requires the elimination of the tracheæ in order to get room to grow in, let us take leave of them, as does the author, and then see how the theory thrives. Without attempting to follow in detail the hypothetical explanation of the manner in which veins arise, through the activity of a hypothetical substance secreted by the hypodermis, forming folds or wrinkles, controlled by hypothetical pressures, we find the veins at length appearing after the following hypothetical fashion: first a *marginal vein* about the entire border of the wing, and a *primary vein* along the middle of it in position somewhat like that of the ridge on the under surface of the gill cover figured. Then there appear *anterior* and *posterior veins* in the spaces at either side of the primary, outgrowing from the base of the wing. Finally, a series of *independent veins* is formed by ingrowth from the marginal vein toward the base of the wing; and with subsequent attachments and adjustments of these, the venation is completed.

In this theory of ingrowing independent veins (branches of the median vein) 'a class of veins that never had basal connections,' lies the chief novelty of the paper. It becomes at once evident why the tracheæ have had to be excluded; for the branches of the median trachea are not independent, and they grow outward from the base of the wing like the other tracheæ. It is, indeed, surprising that greater care has not been taken to establish an hypothesis intended for general application on a better basis of facts. The only evidence given to show that these veins are really ingrowing is that they are usually weaker toward their inner ends and are sometimes independent (unconnected) there. The fact that this weakening is most pronounced and that the detached condition occurs in the more specialized members of the several groups is passed by unnoticed. In generalized Lepidoptera, *Hepialus*, the Psychidæ, Cassidæ, etc., these veins have basal connections, and when free proximally, their dislocated basal rudiments within the discal cell might well have been accounted for. In the Ephemeridæ, where the author finds his series of free and primitive independents most highly developed, they are, unfortunately, most free in *Callibœtis*, *Bœtis*, *Chlœon*, etc.—a bunch of genera representing the extreme of specialization in a highly specialized order. In the Paleozoic Protephemeridæ the author meets (and frankly acknowledges) contradiction from the opposite end of the Mayfly series: "In one point, however, these early wings stand in marked contrast with those of the modern group. It is the absence of free independents. The production of free independents prior to connected ones would seem to be the natural order of evolution, but this evidence certainly does not point that way" (p. 97).

The author's account of their origin is all comprised in the following sentence: "The independents arise from the *margin of the wing,* and might possibly be considered as ingrowing branches of the marginal vein; but, since this portion of the marginal vein is commonly absent, while the independents are almost always present, this conclusion may seem to be unwarranted" (p. 68). Nevertheless, his system is built upon it.

But matters are still worse when viewed from the mechanical standpoint. This theory of veins ingrowing from the hind margin contradicts the primary principle of insect æronautics. For, as is well known, forward motion through the air is due in insects primarily to the sculling action of the wings when vibrated up and down, and that action results from the pliancy of the hind margin. The trend of specialization in the wings of all the orders is toward greater stiffness of the front margin and greater relative pliancy of the area behind it, and the obvious mechanical advantage of this is that they scull in air better. The disappearance of the marginal vein and the fading out of the base of the median, are, owing to position in the wing, the earliest and most expedient contributions to that pliancy. Girard more than half a century ago demonstrated experimentally the detriment to flight of adding weight to that portion of the wing in which these 'independents' are supposed, according to this hypothesis, to originate.

The true order of development is inverted. So it is in the case of the cross veins, whose origin is discussed on page 71. These are supposed, on this special creation theory, to grow up in the clear membrane *de novo.* Special activities of certain cells, occupying the positions that are to be those of veins and cross-veins, are made to account for the appearance of these. But for the assumption that the cells about the vein cavity show greater secretory activity, or produce more chitin, cell for cell, than those outspread in the intervening membrane, there is no proof offered: and it is not clear why the simpler and long current explanation of the differentiation veins from membrane, by accumulation of cells about the vein cavities, and the stretching of those that lie between, does not give a better basis for the application of mechanical principles. For how shall "the more rational conception that there existed in the beginning and has existed through all time to the present day a mechanical necessity in accordance with which the primitive venation was produced, and all its essential features have been maintained through all the vicissitudes of the ages" (p. 62) help us account for anything? It is merely a flow of rhetoric: not an explanation. The mechanical principle were better stated, or at least its operations detailed, with some indications of the material on which it operates. Throughout this paper controlling mechanical principles are heralded as though a new discovery in insect wings, but they never come to light. On the contrary, as we have seen, well-known mechanical principles are flatly contradicted by the theories proposed. Were it not that the principle of vein differentiation is already fairly well understood, this theory of ingrowing independent veins might possibly have made as large a contribution to the confusion of the subject as did that of Adolph, its lineal antecedent.

A reviewer of vein mechanics should have been able to see the primitive dichotomy of the branching of veins. It is a curious survey of insect venation that misses this. Dichotomy abounds in the venation of the oldest known fossils. It occurs in the generalized members of most of the groups. It occurs in the gill covers of many Mayfly larvæ and is beautifully shown in those of *Ephemerella.* It occurs in plants, also, and is the type of branching of such thallophytes as *Dictyota dichotoma* and such liverworts as *Riccia fluitans*. It occurs, apparently, wherever a branching organ extends itself unimpeded in one plane. Its wide-spread independent occurrence is evidence that it is the result of developmental dynamics, and there can be no doubt it is primitive.

The author thus 'having traced the development of these systems of veins' (p. 73) proceeds with full assurance to the application of his theory. He has no difficulty at all in identifying his marginal vein in the catch on the inner edge of the elytron of Coleoptera (p. 75). It is safe to say that no other venation theorist has ever ridden his hobby so far as this. His primary vein, 'invariably found in functional wings' (p. 63), is absent from at least one of the wings shown on p. 100. With respect to the Odonata-anisoptera, in discussing the triangle, he says: "It does not, nor does the wing of any of the Anisoptera, show any transition between a triangular and a quadrilateral cell." The ignorance of our own fauna and its literature shown by this statement is not more surprising than the readiness with which he ignores the illustrative genera that are mentioned in the legend to the figure he is criticizing. But these and other misstatements concerning the Odonata will not mislead the students of that order. Having ignored tracheation and also the likeness in venation between Plecoptera and Orthoptera, he is able to give a different interpretation of the two orders. But the great superiority of his theory appears in the treatment of the venation of the fossil Homothetidæ (p. 102), Embiidæ (p. 106) and Physopoda (p. 126). Even the last named, for whose puzzle no one has hitherto ventured a solution, is instantly resolved by the application of the 'rational theory'! It is wonderful. The only trouble with it is that it is too easy. When in doubt about a vein call it what it most resembles in the hypothetical diagram. "Class as an independent vein anything that anywhere exhibits structures characteristic of independent veins" (p. 69). Throw away the usual safeguards against misinterpretation of parallelisms: they are all superseded by the application of a mechanical theory!

And when we reach the end of it, we find that its goal is another system of vein nomenclature! This is formally compared, vein by vein, with four[1] (out of the dozens) of systems proposed in the past. It is hard to see why the author, since he identifies all of these in detail including the generally recognized branches, should have thought to advance entomology by a new batch of names for them. It is not easy to understand why, if the new terms *marginal* and *primary*, etc., may be used in an elastic sense, as provided on p. 145, the old terms *costa*, *radius*, etc., might not, if it were necessary, be so used, equally well.

Other peculiarities of this work are the ignoring the literature of the subject for nearly the last decade and of important papers on the mechanics of insect wings, much older: the misspelling of the names of authors cited, Aaron, de Selys Longchamps and McLachlan (the last in two ways), and evident misstatement of facts, such as this: "An increase in the size of a wing usually results in an increase in the number of veins" (p. 65). There is no justification in morphological experience for the statement that "increase in a wing area would do just the same things that a decrease would undo" (p. 80) and that the "direct effect of environment would be sufficient" to differentiate two groups (p. 9) is surely assuming something. The statement of p. 145 that other workers have not recognized that the same names should be applied only to homologous organs, is a fine bit of assumption. There is a mysticism about the account of the genesis of the venation that is somewhat unusual in a scientific paper: page 79 is full of it; and the statement that the primary vein was developed to be the dominant vein (p. 144) reminds one of the statement of that other narrative of

[1] Those attributed to Comstock are the names selected by Redtenbacher as most available. They were in his day and are now the ones in most common use, and to their adoption no serious objection has ever been offered.

Genesis, that the lights in the firmament of heaven were to be for signs.

It is altogether probable that entomologists, before entering upon the course here marked out for them, will demand a better statement of guiding principles, and a better disposition of the ontogenetic and phylogenetic difficulties that beset the way.

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Minerals and How They Occur. By W. G. Miller, Provincial Geologist of Ontario, formerly Professor of Geology, School of Mining, Queen's University. Toronto: The Copp, Clark Company. 1906.

In his intention of producing a book on mineralogy for 'secondary schools and prospectors' the author has succeeded admirably both because of the clearness and simplicity of his style and because of his accuracy of statement. There is room for books of this sort since, though the subject is of wide general interest, there are few sources of information which are attractive to the *beginner.* The average book offered to the beginner is not only inaccurate but lacks successful arrangement and shows poverty of facts and illustrations. Professor Miller's book contains about two hundred illustrations and these give such an idea of the subject as descriptions could not convey. They are new, well selected, and some of them are especially good (*e. g.,* Figs. 20, 47, 63, 79).

The large amount of information contained in the book is in attractive form. Upon looking at the table of contents one might be reminded of Voltaire's essay on dogs in which towards the close he says 'Speaking of dogs reminds me of cats' and proceeds to write a short dissertation on cats. One might think that the paragraphs on fossils were hardly called for a book on mineralogy. But as he becomes acquainted with the author's aim he sees that the book differs from the ordinary one which presents the science in its narrower aspects and that it has been written just as if the author were talking to interested beginners before whom he must needs start with the most obvious things—rocks, the common rocks with which his readers are familiar—and build upon them his edifice. Such work necessitates excursions into the surrounding country and the result is a building all of whose parts contribute to mineralogy.

The more involved parts of the subject are omitted or touched upon but lightly and the things which are apt to prove most attractive to beginners are presented in logical and compact manner.

A few changes might be suggested. Though crystallography is the least palatable side of the subject it is so essential as to require more attention. When the axes of the six systems are being given (Fig. 23) one should not be omitted; the orientation should be according to the almost universal method—*i. e., a* should always be the axis pointing to the observer, should always represent the short axis in the orthorhombic and triclinic systems, and the inclined axis in the monoclinic system. β should represent the acute angle made by the intersection of c and a and α by the intersection of c and b. All of the simple holohedral forms should be pictured and with the axes drawn in them. Whether combination, twinned and hemihedral forms are presented may well depend on the space at the disposal of the author. But if crystallography is to be mentioned at all the first principles should be given with clearness.

An occasional statement like the following should be modified. "During late years this theory of origin (of petroleum and natural gas) has been questioned by many workers who are inclined to believe that both materials are of inorganic origin" (p. 59). Forty years ago Berthelot suggested that petroleum might have originated from union of carbonated waters with uncombined sodium and potassium and about ten years after that Mendeleef propounded as a possible origin the union of such waters with metallic carbides. Thus the theory can hardly be called a recent one and in addition it appears to be a theory which shows possibilities rather than the facts which the study of oil fields the world over seems to establish. The actual geological conditions in oil fields necessitate the conclusion that oil and natural gas are of organic origin.

The book is full of valuable information presented in attractive form and for the beginner is one of the best books of its kind with which I am acquainted.

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SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Experimental Zoology, Volume III., No. 4 (December, 1906), contains the following papers: 'The Physiology of Regeneration,' by T. H. Morgan. Experiments on salamanders, earthworms and fish show that the rate of regeneration in a posterior direction is more rapid the further the cut surface from the original end. In other words, the more of the old part removed, the more rapid the new part regenerates. Other experiments show that this is not due to food conditions, but that the rate depends on a formative factor. It is suggested that it is the relation of tension in the old and the new part that is a controlling factor in regeneration and growth. 'Hydranth Formation and Polarity in Tubularia,' by T. H. Morgan. Experiments on tubularia show that the polarity is an expression of the direction of the gradation of the differentiated materials. The greater the differentiation in one direction the longer the road that must be traveled to produce a different kind of structure. The gradation acts as a physical factor in development, determining the tension relations in the old and new part. 'Studies on the Development of the Starfish Egg,' by D. H. Tennent and M. J. Hogue. This paper describes the parthenogenetic development of the star-fish egg following treatment with CO_2, the phenomena occurring as a result of first treating the egg with CO_2 and later fertilizing it, and the results of subjecting fertilized eggs to the influence of CO_2. 'Some Experiments on the Developing Ear Vesicle of the Tadpole with Relation to Equilibration,' by Geo. L. Streeter. A study of the normal development of the function of equilibration in the tadpole, and the variations produced by removal and transplantation of the ear vesicle during the early larval period. 'The Relation between Functional Regulation and Form Regulation,' by C. M. Child. The organism is to be regarded as primarily a dynamic or functional complex, and structure and form are visible expressions of dynamic conditions: consequently the regulation of form and structure is fundamentally a dynamic or functional regulation and only as such can its phenomena be satisfactorily interpreted. 'Study of the Spermatogenesis of Coptocycla Aurichalcea and Coptocycla Guttata, with especial reference to the Problem of Sex Determination,' by W. N. Nowlin. An investigation of two species of beetles revealed the presence of an unequal pair of chromosomes, the so-called 'idiochromosomes' of Wilson, which, we have strong evidence for believing, transmit or determine the character of sex. The small one invariably occurs in the male somatic cells and represents the recessive form of the female character; the large one in the female somatic cells and bears the male character. 'Torsion and Other Transitional Phenomena in the Regeneration of the Cheliped of the Lobster (*Homarus Americanus*),' by Victor E. Emmel. A comparison of the regenerative with the ontogenetic method of development. 'The Influences of Gases and Temperature on the Cardiac and Respiratory Movements in the Grasshopper,' by Eulalia V. Walling. The influences of gases and temperature on the respiratory and cardiac activities were found to be practically the same on segments of the isolated heart and isolated respiratory centers as in the normal grasshopper. Moreover, it was found that these activities may continue in such specimens as long as four days in an atmosphere of pure hydrogen.

DISCUSSION AND CORRESPONDENCE

NORTON'S ELEMENTS OF GEOLOGY

The review of Norton's 'Elements of Geology,' which appears in a recent number of Science, Vol. 24, p. 590, prompts one to repeat the suggestion recently made, that the legitimate function of a review in such a periodical as Science is to give to the reader an accurate impression of the general character of the work, both as to the ground which it covers, and as to the way in which it covers it.

It may be doubted whether the review in question performs this function. It leaves the impression that the book reviewed is, on the whole, a pretty poor sort of book, when it is really an excellent one. It is not beyond criticism—no book is. The reviewer indicates some of the weak points, and seems to regard as weaknesses several of the strong features. A number of the criticisms might be appropriate if the book were intended primarily as a reference work, but they hardly seem applicable to a book which is intended as a text-book for beginners. An excellent text-book is not necessarily the best book for reference. The classification of subject matter for ideal books of the two types would be, in many respects, very different. In a text-book, it is certainly no weakness that one must 'go to three or four separate parts of the book' 'to learn about sandstones,' though this might be a weakness in a book of reference. The reviewer's attitude leads one to suspect that he uses books for reference only, not as texts, and that this has influenced his point of view.

In spite of the reviewer's statement, the diagrams of the book are, on the whole, excellent and readily understood, and the notes and questions which accompany them are to be especially commended.

The criticism that the book is largely physiographic is nothing against it, and when we remember the class of pupils for whom the book is intended—high-school pupils—the absence of 'references to other books' is certainly much less serious than the reviewer seems to think.

The statement that 'the bog ore, silicious and phosphatic deposits that get a brief mention in Le Conte are not here referred to' leads one to make the further suggestion that a book should be carefully read before detailed criticism of this sort is indulged in, for bog ore is mentioned on page 53 and silicious deposits on pages 52, 178 and 261. Other similar criticisms of the reviewer might be cited. The omission of such subjects as phosphatic deposits is to be commended in a book of this type, for it must be remembered that most elementary books treat of too many, not too few, topics. In the writer's judgment the book takes rank at once among the best of the elementary text-books on geology.

H. H. Barrows

University of Chicago,
December 10, 1906

SPECIAL ARTICLES

THE SIGNIFICANCE OF THE GRASPING ANTENNÆ OF HARPACTICOID COPEPODS

The character of the secondary sexual differentiation of the first pair of antennæ of male free-swimming Copepoda and the associated manner of copulation divide these copepods into two well-marked groups: one group in which only one antenna forms a grasping organ and in which the act of copulation is relatively short; and a second group in which both antennæ are grasping organs by which the male holds the female for a long time in copula. The duration of this union is shown by two records: one of an apparently normal pair of *Harpacticus uniremis* which remained in copula at least twenty-nine and possibly thirty-eight hours; another of a pair of undetermined genus which remained in copula eight days, at the end of which both male and female died. The persistence of the male is shown by the fact that he can be torn apart, but still maintains his hold until the paralysis of death frees the female. Claus[1] observed that the males of the Peltidiæ were found in copula with females one molt from maturity and speculated upon the meaning of the phenomenon without arriving at a satisfactory conclusion.

During the spring of 1906, a large number of copulating pairs of *Harpacticus uniremis* and *Tachidius littoralis* appeared in the tow taken in Narragansett Bay and a number of pairs were separated in watch glasses for observation. We were fortunate in examining a pair of the first species just when the female was beginning to molt. The ecdysis occupied about five minutes and as the slough came away, the male, which had been holding the female by the hinder edge of the carapace,

[1] Claus, C., 'Die freilebenden Copepoden,' Leipzig, 1863, p. 71.

dropped the cast and grasped the female between the abdomen and thorax, and, moving around from the dorsal surface of the female, deposited the spermatophore in its place upon the genital segment. He then freed the female and made no attempt to renew his hold. In six days the female produced eggs which hatched in seven days.

Despite a great deal of effort, we have never again been able to observe the molt and subsequent copulation, but we have complete evidence that, in three species of two genera (*Harpacticus uniremis, H. gracilis* and *Tachidius littoralis*), the spermatophore is never attached before the female molts, and that, in every case, the male holds the female until she molts. Whenever a normal pair, left a short time before in copula, were found separated, careful examination revealed the cast of the female in the watch glass and a spermatophore attached to the female. In other words, every successful copulation must be prolonged until the female molts.

The longest period in copula observed with a successful issue was between twenty-nine and thirty-nine hours. In two cases the female died in molt, an antenna and a furcal bristle respectively being caught in the cast. In several instances the male, the female, or both died before the molt, probably because of the abnormal conditions of the experiments. In one case the male of a copulating pair was killed and, after the female had molted, another male was introduced, but no copulation took place.

These observations are by no means the first of this character, for there is a general impression among students of Crustacea that ecdysis and copulation or ovulation are closely connected processes. There is evidence that copulation follows a molt of the female in several crabs[2] and in the isopod Gnathia.[3] There is also evidence that ovulation follows the molt in some isopods and amphipods.[4] Finally, Della Valle[5] finds that the male of *Gammarus pungens* holds the female until she molts, assists in freeing her from the cast, and then deposits the sperm.

The males of Amphipoda, Isopoda, *Artemia* and related Phyllopoda, *Limulus,* and of the Copepoda of the group under discussion all have the same habit of carrying or holding the female and all have structures modified for this purpose. Hence, it is possible—it is even probable—that this habit and these structures indicate that, in these forms, the female must molt before fertilization can be accomplished.

Our attempts to find the meaning of this presumably general habit of the Crustacea mentioned have been unsuccessful, but we believe that the softened condition of the shell may be necessary for the attachment of the spermatophore or the extrusion of the eggs.

Leonard W. Williams

[2] See Herrick, F. H., 'American Lobster,' Fish, Comm. Bull., 1895, p. 39. Williamson, H. C., 'Contributions to the Life History of the Edible Crab, *Cancer pagurus,*' Rep. Fish. Board, Scotland, V., 22, 1904. Barnes, E. W., 'Natural History of the Paddler Crab, Callinectes hastatus,' 34th Report Comm. Inland Fisheries of Rhode Island, p. 69, 1904.

[3] Smith, G., 'Metamorphosis and Life History of *Gnathia maxillaris,*' *Mitt. Zool. St. Neapel,* XVI., pp. 469–471, 1904.

[4] Langenbeck, C., 'Formation of the Germ Layers in the Amphipod *Microdentopus gryllotalpa,*' *Jour. Morphology,* XIV., p. 303. See also Korschelt & Heider, 'Text-book of Embryology' (English translation), Vol. II., p. 105.

[5] Della Valle, A., 'Gammarini del Golfes von Napoli,' Fauna and flora des golfes von Neapel, 20, p. 276, 1893.

BLOWING SPRINGS AND WELLS OF GEORGIA, WITH AN EXPLANATION OF THE PHENOMENA[1]

The blowing springs and wells of Georgia may be divided, for convenience of description, into two classes, namely, those in which the air passes inward for a time and after a short period of quiescence reverses its course, and those in which the quantity of the air is constant and moves in one direction only. One of the best illustrations of the former class of springs is the Grant Blowing Spring, near Chattanooga, Tennessee, a description of which is as follows:

The Grant Blowing Spring is located at the base of Lookout Mountain near the Georgia-Tennessee state line about three miles from the corporate limits of Chattanooga. The

[1] By permission of the state geologist.

spring has long been known and is much frequented by tourists visiting Chattanooga. It may be reached by the Alton Park electric cars, or by the Chattanooga Southern Railway. The proximity of the spring to Chattanooga and its accessibility has doubtless added much to its notoriety.

In general appearance the spring is not unlike many other bold springs met with along the eastern base of Lookout Mountain. It flows from a fissure, at the base of a limestone bluff, forming a good-sized stream. The spring itself reveals but little evidence of the phenomenon for which it is noted. Nevertheless, the phenomenon can readily be detected by holding a smoldering match or lighted paper near the opening from which the water flows. The motion of the air is to be seen in its full force at an opening in the bluff above, about forty feet distant, and at an elevation of ten or fifteen feet above the spring. At this opening, which leads down to the stream supplying the spring, there is, at times, a strong current of air passing inward or outward, depending on the atmospheric conditions hereafter to be discussed. The writer was informed by Mr. W. H. Grant, the present owner of the spring, that the opening above referred to was formerly of sufficient size to admit the body of a man; and furthermore, that he, together with a civil engineer, some years ago entered the opening which led into a cave having large chambers fifteen feet or more in height. The distance to which the cave was explored by Mr. Grant and his companion was not measured, but it was estimated to be nearly a mile. The direction of the cave is said to be southward parallel with Lookout Mountain. Mr. Grant reported that they noticed no current of air in the cave. This, however, may be accounted for by their using a lantern which would not be affected except by a strong draught. The stream forming the spring was found traversing the cave as far as the exploration extended, and many stalactites and stalagmites were reported in the larger chambers.

The formation in which the cave occurs, and from which the spring flows, is one of the lower members of the Carboniferous rocks known as the Bangor limestone. It consists of a very pure heavy-bedded blue or grey limestone attaining a thickness, in the neighborhood of Chattanooga, of about 800 feet. In the immediate vicinity of the blowing spring, the formation dips at a low angle westward toward the axis of Lookout Mountain. The Bangor limestone is highly soluble in meteoric waters and frequently gives rise to limestone sinks and caves of greater or less extent.

At the writer's suggestion, Mr. Grant made a series of observations on the blowing spring in order to determine the time and direction of the air currents, together with the relative temperature of the water flowing from the spring and the outside air. The results of the observations, which extended from December 21 to December 26 inclusive, are embodied in the following table:

TABLE I.

	Time		Temperature		Direction of Current	
Date	A. M.	P. M.	Air	Water	In	Out
Dec. 21	8		46	52	Weak	
21	noon		50	55	Strong	
21		4	48	55	Strong	
22	8		36	54	Strong	
22	noon		42	56	No current	
22		4	46	56		Weak current
23	8		43	56	Strong	
23	noon		42	54	Strong	
23		4	40	56	Strong	
24			27	53	Strong	
24	8		38	54	Strong	
24	noon	4	33	55	Strong	
25			28	52	Strong	
25	8	4	38	56	Not so strong	
25		10	30	55	Strong	
26	4		26	54	Very strong	

The following barometric readings furnished by Mr. L. M. Tindell, officer in charge, U. S. Weather Bureau, Chattanooga, Tenn., show the variations of the atmosphere pressure during the time of Mr. Grant's observations.

The tables here given will be further considered at the end of this paper in the discussion of the explanation of blowing springs and wells.

TABLE II. HOURLY BAROMETRIC READINGS, U. S. WEATHER BUREAU, CHATTANOOGA, TENN., DECEMBER 21 TO 26 INCLUSIVE

Date	21	22	23	24	25	26
1 a. m.	28.85	29.30	29.09	29.35	29.42	29.45
2	.87	.30	.07	.36	.42	.45
3	.90	.30	.06	.34	.42	.45
4	.42	.30	.08	.34	.42	.45
5	.94	.33	.09	.34	.42	.45
6	.97	.33	.13	.35	.43	.45
7	.28	.33	.13	.36	.43	.44
8	29.02	.34	.15	.37	.44	.45
9	.04	.34	.20	.39	.45	.47
10	.05	.34	.2	.39	.44	.47
11	.05	.32	.24	.38	.43	.45
12 m.	.05	.28	.25	.37	.40	.41
1 p. m.	.05	.24	.24	.36	.38	.37
2	.06	.22	.24	.36	.38	.35
3	.08	.22	.26	.36	.38	.34
4	.10	.21	.26	.36	.39	.33
5	.13	.19	.28	.37	.40	.33
6	.18	.19	.29	.40	.42	.33
7	.21	.19	.30	.41	.43	.32
8	.24	.18	.32	.42	.44	.32
9	.25	.15	.33	.44	.45	.32
10	.29	.14	.34	.44	.45	.33
11	.29	.11	.33	.43	.45	.32
12 m.	.29	.06	.33	.42	.45	.32

Boston Well.—The Boston deep well belongs to the second class of blowing wells, namely, wells in which the direction of the air current is in one direction only. Boston, the town in which the well is located, is on the Atlantic Coast Line Railroad, in the southeastern part of Thomas County, twelve miles east of Thomasville. It has an elevation of 198 feet above the sea-level. The surface of the surrounding country is comparatively level, though lime sinks, produced by the subterranean stream, are occasionally met with. The prevailing rock of the region is Vicksburg-Jackson limestone overlain by sands and clays of variable thicknesses.

The well, which is six inches in diameter, has a depth of 290 feet. Water was reported at 120, 160 and 286 feet, respectively. The main water supply at present is said to come from a subterranean stream in the limestone at 120 feet. The casing extends to 110 feet. The static head of the water in the well when completed was 128 feet from the surface, or eight feet below the subterranean stream. Shortly after the completion of the well, Mr. J. Z. Brantley, the mayor of the town, discovered that there was a continuous draught of air passing down the casing, and by placing his ear near the mouth of the well he was able to detect a sound like running water. This indraught, Mr. Brantley reports, was quite strong and continued as long as the well was left open. The writer was unable to verify Mr. Brantley's statement at the time of his visit, owing to the well being connected with the pump which supplies the town with water.

The Lester Well.—This well, reported by William Miller, which is also similar to the Boston well, occurs on B. E. Lester's plantation, twenty miles south of Thomasville, near Iamonia Lake. Mr. Miller, in describing this well, says that at a depth of 154 feet he struck a stream of water running so swiftly that he could not pass a two-pound iron plumb bob attached to a fishing line through it. He reports blowing crevices in the well at 87, 124 and 144 feet. When the well was being bored the air from each of these cavities is said to have passed in in the forenoon and out in the afternoon; but after the completion of the well to the swift moving subterranean stream, the air ceased to pass outward, but was sucked in with a strong steady pull, drawing the flame and smoke of a torch down the casing when held six inches above its opening. This well is cased to 70 feet, below which point it is said to penetrate a soft white limestone.

Causes of Blowing Springs and Wells.—The two classes of blowing springs and wells above described appear to be due to two entirely different causes. Those of the first class, of which the Grant blowing spring is a good type, seem to be due entirely to the difference of atmospheric pressure of the air on the outside and on the inside of the cave.

At the time of my visit to the Grant blowing spring, I was of the opinion that the relative temperatures of the air on the outside and on the inside of the cave, the latter temperature being indicated by the water flowing therefrom, had something to do with the air currents; but the record furnished by Mr. Grant (see Table I.) shows that the direction of the currents has nothing whatever to do with these relative temperatures. That these currents are due solely to the variation of atmospheric pressure appears to be conclusively demonstrated by comparing Tables I. and II. The first of these tables shows, with only two exceptions, namely, at noon and 4 P.M. December 22, that at the time when the observations were made there was an ingoing current. Table II., which gives the barometrical readings, shows that the time of recorded ingoing currents, except at noon, December 24, was during the time of increasing atmospheric pressure; and that in the two exceptional cases, which showed outgoing or no currents, the atmospheric pressure was decreasing. In other words, the outgoing currents always take place during rising barometer, and ingoing currents during falling barometer. As the atmospheric pressure usually increases daily from 4 A.M. to 10 A.M. and decreases from 10 A.M. to 4 P.M., it follows that springs, wells and caves of this class will generally have an indraft in the forenoon and an outdraft in the afternoon. If the daily variations of atmospheric pressure were regular, the ingoing and outgoing currents would also be regular and would take place at the same time each day. However, as the daily maximum and minimum barometric readings may vary greatly from day to day, due to approaching storms or other causes, the ingoing and outgoing currents will not always act with the same energy.

In the second class of wells and springs, the constantly outgoing or the constantly ingoing current is entirely independent of atmospheric conditions. The currents, whether outward or inward, act with equal energy during high or low barometer and always move in the same direction. The Boston and the Lester deep wells are excellent examples of wells and springs of this class. The phenomenon which they exhibit seems to be due entirely to the friction of the air on a rapidly moving current of water. This phenomenon is beautifully illustrated in Richard's water air-blast, to be found in many well-equipped chemical laboratories. In the Boston well, and also in the Lester well, appear almost exactly the same conditions met with in Richard's water air blast. The well itself forms the inlet for the air, and the rapidly flowing stream in the subterranean channel below completes the conditions necessary for an ingoing air blast. As the air in the wells here named is constantly drawn in, it naturally follows that it must escape at some other point as an outgoing current, thus giving rise to continuously blowing caves or springs.

As underground streams frequently pass from one bed of rock to another in their subterranean course, they, no doubt, often form waterfalls which possess all the essential conditions necessary for producing an air blast, thus giving rise to continuously blowing caves and springs.

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CURRENT NOTES ON LAND FORMS

GLACIATION OF THE BIG HORN MOUNTAINS, WYOMING

A RECENT report on the 'Geology of the Big Horn mountains' by N. H. Darton (Prof. Paper 51, U. S. Geol. Surv., 1906; excellent plates) describes the range as a wide anticline with steeper dips on the east, eroded sufficiently to expose its granitic core over the broad arching crest, while scalloped ridges of the more resistant members lie along the flanks and in places stretch over toward the axis of the range. R. D. Salisbury presents a chapter on glaciation—in which there is to our view an insufficient recognition of the previous work of F. E. Matthes on the same district—showing that many glaciers occupied the upper valleys during the last glacial epoch. Erosion by these glaciers, working in valleys that had been previously developed by normal preglacial erosion, is held responsible for 'the development of cirques, the cleaning out of the upper parts of the valleys through which the ice passed, the rounding and widening of the valley bottoms, the polishing of the rock surfaces in the valleys and the excavation of some of the lake basins.' The cirques head in superb cliffs, which rise abruptly to the broad highland surface of the unglaciated granite; sharply serrate ridges occur where the widening of neighboring cirques and troughs has consumed the intervening highland surface; here the mountains gain a distinctly Alpine form. As to the strength of glacial erosion in excavating the valley troughs, it is said that 'not a few of the valleys may have been deepened 400 to 700 feet in their upper parts, while in some cases * * * the deepening may have been considerably more.' In view of this, explicit mention of 'valley deepening' as well of the 'rounding and widening of valley bottoms' might have been made in the list of features for which glacial erosion is held responsible, as quoted above. Here, as in so many other glaciated valleys, the depth of the glacial tarns, many of which occur in the upper reaches of the valleys, is by no means a full measure of the depth of glacial erosion—the rock basins simply indicate an excess of erosion at one point over that next down-valley, a fact which might have been more clearly presented. Hanging lateral valleys occur but are not numerous; nevertheless they certainly deserve a place in the list of 'the distinctive features of glaciated mountains,' where for some reason they are omitted.

The morainal deposits, which occur where the local glaciers ended in the descending valleys on the mountain flanks, are discussed in some detail; good examples of moraines and morainal lakes are figured. About half of the lakes of the district—all of small size—are of this origin; the other half occupy glacially excavated rock basins higher up the valleys, as already indicated. Changes in drainage due to glacial erosion are noted, an example of glacial capture previously reported by Matthes being here figured. (The titles of two figures illustrating this 'capture' have been by oversight interchanged.) A case is cited in which a glacier failed to remove all the decayed rock at a certain point, while rock surfaces not far away show severe wear. Although this is a minor feature, and certainly to be expected as a common result of glacial activity, the citation is of importance in connection with other cases of larger area where the failure of a glacier to remove decayed rock has been urged as evidence of inefficiency of glacial erosion in general. The facts, as here recorded, again emphasize the principle that glaciers, like rivers, erode vigorously in one place, feebly in another, and make extensive deposits in a third.

D. W. J.

GLACIAL EROSION IN THE HIMALAYAS

It has been frequently remarked by those who are still unconvinced of the capacity of glaciers to deepen valleys and excavate lake basins, that the absence of valley lakes in the glaciated districts of the Himalayas is strong testimony on the negative side of the problem. The reply has been made that the glaciated parts of the Himalayan valleys probably had so strong a slope in preglacial time that the overdeepening by glacial erosion did not produce profound lakes, and that such lakes as were produced have already been filled with waste by the heavily loaded rivers of those lofty and often barren mountains. But a specific example of glacial erosion in the inner Himalayan region east of the vale of Kashmir, has recently been described by Ellsworth Huntington, under the title, 'Pangong, a glacial lake in the Tibetan plateau' (*Journ. Geol.*, XIV., 1906, 599–617), from which it appears that valley glaciers have actually produced lakes there, as well as in other mountain ranges. Pangong, or Pangkong, at an altitude of 14,000 feet, about 40 miles long and 142 feet deep (as stated by Drew), is the distal member of a series of lakes of which the total length is 105 miles, with a maximum breadth of four miles. The enclosing mountains rise in steep slopes for the first 1,000 or 2,000 feet, and then at gentler slopes 3,000 or 4,000 feet more to peaks 20,000 feet in altitude. Large moraines, old enough to be much eroded, are found some twenty miles down the main valley from the end of the lake. Glaciated knobs are found along the valley floor, perched erratics occur up to 600 feet over the lake surface, and at least one lateral valley hangs distinctly above the main valley. Pangong does not overflow at present; its surface is thirty-five feet lower than a barrier, a mile farther down the valley, which Drew took to be an alluvial fan formed by a side stream, but which Huntington gives good reason for regarding as a rock sill or inequality in the deepened valley floor. The glacial origin of this string of lakes thus seems to be reasonably well attested. Postglacial climatic changes, indicated by abandoned shore lines, are discussed in some detail.

W. M. D.

POSTGLACIAL AGGRADATION OF HIMALAYAN VALLEYS

THE possibility that glacially overdeepened Himalayan valleys have lost their lakes in consequence of postglacial aggradation, as suggested at the beginning of the preceding note, is supported by the features of the Shigar valley, northeast of the vale of Kashmir, as described in an admirable essay on 'Die Thäler des nordwestlichen Himalaya,' by K. Oestreich, topographer of the Workman expedition, 1902, and now Docent in Marburg University (*Peterm. Mitt. Ergänz'hft,* 155, 1906: 36 exceptionally fine plates). This valley was invaded by huge glaciers from the lofty Mustag range on the north, where glaciers of great size still remain; and Oestreich points out the strong contrast between the broad glaciated trough of the Shigar and the narrow gorge of the Indus which the Shigar joins, the village of Skardu lying near the junction. The Indus in its northwestern intermontane course follows for 150 miles (except near Skardu, as stated below) a young gorge, eroded some 200 meters beneath the floor of an earlier, larger and more mature valley: the sides of the gorge show numerous ledges between incompletely graded slopes; the bottom has no continuous flood plain, but only local sandbanks at the base of the convex spurs; the road can not follow the river, but has to rise and fall on the spurs of the valley side. The Shigar valley, on the contrary, is described as having been widened and deepened by glacial action: it has a broad, aggraded floor, on which the river divides and wanders in a braided course: the valley sides are glaciated and carry patches of glacial deposits. The barren slopes of the desert mountains are believed to have aided in supplying waste with which to aggrade the overdeepened valley. Similar features are found also in a middle portion of the Indus valley—the Skardu basin—for some twelve miles below the entrance of the Shigar; and again, to a less degree, for some twenty miles farther up the Indus to the entrance of the Shayok, which like the Shigar brought in a great glacier from the north. Before the Skardu basin was filled with gravel, there is much probability that it contained a lake.

It is singular to note that, although Oestreich ascribes the deepening and widening of the Shigar valley to glacial erosion, he explains its continuation in the Skardu basin as the result of tectonic movements: the glaciers did not, he says, deepen the basin by erosion, but the deepening of the basin by deformation attracted the glaciers to it. The text is unfortunately not detailed enough to enable the reader to reach an independent judgment.

W. M. D.

UPLIFTED PENEPLAINS IN THE HIMALAYAS

IN a later section of the essay referred to in the foregoing note, Oestreich gives an excellent description of the highlands of Déusī—the Deosai plains of English travelers—and accounts for them as an uplifted and not yet dissected peneplain. They have a somewhat circular area, 24 kil. in diameter, and stand at an altitude of from 3,800 to 4,000 met.; their gently undulating surface, sometimes surmounted by subdued hills, shows no sympathy with the deformed structure of their mass. Mountains rise around them to 5,000 met. except on the southeast, where their streams escape. The whole surface has been glaciated, and is now clothed in summer with grass and flowers. It is pointed out that the Déusī highland is the only example of its kind on the near side of the river Indus, but that similar highlands exist farther inland, especially in Tibet.

It thus would seem that, as far as this part of the Himalaya is concerned, it falls in with a number of other mountain ranges—as recently pointed out for the Alps by Penck, for the Carpathians by Martonne, for parts of China by Willis, to say nothing of various ranges in the United States—in owing its altitude not to the deformation by which its disordered structure was caused, but to a broad uplift which took place long enough after the period of deformation for erosion to have greatly lessened or almost destroyed whatever unevenness of form the deformation produced.

W. M. D.

A MATHEMATICAL EXHIBIT OF INTEREST TO TEACHERS

For the benefit of students and teachers of mathematics who may be visiting Columbia University, the department of mathematics in Teachers College has arranged a permanent exhibit of material available for the study of the history and teaching of the subject. One feature of the exhibit is a collection of mathematical apparatus and models adapted to the needs of the various grades from the kindergarten through the high school, including games, mensuration blocks and models usable in geometry and trigonometry.

In addition to Professor Smith's library of several thousand books and pamphlets upon this subject, there is also available his collection of mathematical instruments—some dating as far back as 1450—of manuscripts, and of engravings and portrait medals of eminent mathematicians.

The early mathematical instruments exhibited include the following: an astrolabe of Arabic workmanship; one of Italian workmanship, signed by the maker, and dated 1509; another, a part dating from about 1450, and the rest, including the four plates, from the following century; and one of Paduan workmanship, signed by the maker, and dated 1557, a practically perfect specimen, with five finely engraved plates. There is also a quadrant of the sixteenth century, one of the primitive instruments of trigonometry, bearing the early names 'Umbra recta,' and 'Umbra versa,' together with several leveling instruments of the seventeenth and eighteenth centuries. There are also numerous measures of length and weight, of the seventeenth and eighteenth centuries, including the ell and some interesting sets of money changers' weights; several finely engraved protractors, diagonal scales, and similar instruments; several sector compasses and compasses of other kinds, of the Renaissance period; a collection of typical forms of dials to illustrate the application of mathematics to dialling in the Renaissance period, and several armillary spheres of the sixteenth, seventeenth and eighteenth centuries.

The material used to illustrate the development of mechanical calculation includes the following: a collection of medieval counters (jetons, reckoning pennies) of fifteenth and sixteenth century workmanship, partly French and partly German, some with the figure of the Rechenmeister seated at the abacus. Books showing the process of calculation by means of counters 'on the line' are also exhibited. There are also to be seen an Arabic abacus, a Russian tschotü, a Chinese swanpan, a Japanese saroban, a set of Napier's rods, and a set of Korean bones (the modern form of the ancient Chinese 'bamboo rods,' or the Japanese Sangi). Some Japanese books of 1698 are exhibited showing the transition from this latter form of computing to the saroban, which took place in Japan about that time. Besides these there are shown several modern calculating machines, including the Goldman and Stanley arithmometers, slide rules, and similar devices. There are also available for study, in addition to those displayed, several early treatises showing the use of counters, together with numerous works on the historical development of this phase of arithmetic. This is also extensively illustrated in a collection of stereopticon slides belonging to the department.

There are in Professor Smith's library about two thousand portraits of mathematicians. Of these it is possible to exhibit only a relatively small number. About forty are framed and can readily be examined, and visitors wishing to examine others in the collection are assisted in doing so. This part of the collection represents the work of a number of years and the repeated examination of the stocks of many European dealers. It is particularly rich in the works of early engravers, although containing a considerable number of photographs and modern process portraits. Reproductions of a number of the portraits have been made for school and college use by The Open Court Publishing Co., of Chicago.

The collection of Newtons includes all

the most important portraits of this great mathematician and physicist. An effort has also been made to acquire all the best portraits of Leibnitz, Descartes, Euler, the Bernoullis, Legendre, Monge, Cauchy and others who stand out as particularly prominent in the creation of pure mathematics. The collection also includes the portraits of many who have achieved success in the field of applied mathematics, notably of men like Laplace, Lagrange, Huyghens, Bailly and Arago.

Many of these portraits have been reproduced in stereopticon slides for the use of the department, and copies are supplied to schools at cost.

The collection of medals of mathematicians includes more than a hundred pieces. The following are among the most prominent mathematicians represented: Fr. Arago, Archimedes, Aristotle, Bailly, Betrand, Bonnet, Tycho Brahe, Cardan, Cassini, Cauchy, Cavalieri, Copernicus, d'Alembert, De Moivre, Descartes, Euler, Fermat, Galileo, Gassendi, Gauss, Grandi, Halley Hutton, Huygens, Kepler, Lacroix, Lagrange, Lalande, Laplace, LeVerrier, Lobachevsky, Maurolicus, Monge, Neudorffer, Newton (seven medals), Pascal, Pestalozzi, Poinsot, Poisson, Pythagoras, Quetelet, Stevin, Thales, Viviani, Wolf, Wren.

The complete set of mathematical portrait medallions by David d'Angers is included. In addition to the portraits there are numerous other medals of interest in the history of mathematics, including the rare Metric System piece of 1872.

Another interesting feature of the exhibit is Professor Smith's collection of autographs of mathematicians. On account of space, it is possible to exhibit only a few of the several thousand autographs in the library. The following are among the most interesting, and are shown in one of the wall cases: Newton—a two-page manuscript demonstration written for one of his students at Cambridge; Leibnitz—an autograph letter relating to a series of integrals; autograph letters of Sir William Rowan Hamilton, Euler, Johann Bernoulli, Mersenne (written about 1625), Maupertuis, Legendre, Wronski and Arago; documents signed by Gauss, Laplace, and Lagrange; autograph letters from Poncelet to Liouville, Liouville to Dirichlet, and Arago to Poncelet. Autograph letters of the following mathematicians have been taken from the files so as to be accessible, and are usually displayed: in pure mathematics—Jacobi, Cayley, Sylvester, Kronecker, Cremona, Hachette, Poincaré, Hermite, Clebsch, Cauchy, Chasles, Clifford, Binet, Bezout, Monge; in astronomy—Bode, Airy, Delambre, the three Cassinis, Maskeleyne, Flamsteed, Flammarion; in physics—Ohm, Bessel; in the history of mathematics—Montcula, Fuss, Libri, Kästner, P. Tannery, M. Cantor.

In the line of Newtoniana there are five framed portraits of Newton, as follows: Mezzotint by Simon, after Thornhill; line engraving by George Vertue, after Vanderbank; line engraving by Houbraken, after Sir G. Kneller; lithograph by G. B. Black, after Wm. Gandy; line engraving by E. Scriven, after Vanderbank. There are seven medals of Newton, representing the work of Croker (bronze and silver), Dassier, Roëttiers, and Petit (two specimens), besides one without the artist's name. The Newton manuscript was long in the library of Professor Jacoli, at Venice. It consists of a physical demonstration written by Newton at Cambridge, for an Italian student, c. 1700. The impression of Newton's Galileo seal is from the original which was recently presented to the South Kensington Museum. The bust of Newton is after the original by Roubillac. The unframed portraits, numbering over one hundred, include specimens of the work of the following engravers: Phillibrown, Zeelander, Lips, Romney, Fry, Rivers, Scott, Tardieu, Ridley, Goldar, Cars, Laderer, Le Cœur, Freeman, Seeman, Krauss, Ravenet, Guadagnini, Holl, McGahey, Conquy, Zuliani, Cooke, Le Keux, Normand, Landon, Baumann, Wedgwood, Dupin, Smith, Edwards, Desrochers, Weber and others.

There are also displayed a number of books and curios illustrating certain steps in the history or the teaching of mathematics. These include a Babylonian cylinder with cuneiform numerals, a piece of ancient Egyptian pottery with the zodiacal signs, Roman coins illustrating certain unusual forms in the ancient numeral system, some English tally sticks of 1296, two Renaissance compotus medals, and a celestial sphere of the sixteenth century.

The bibliographical curios include one of the few copies saved from the fire which destroyed most of the first edition of Libri's 'Histoire des Mathématiques' (Vol. I.), with Libri's autograph marginal notes. There are also autograph presentation copies of Laplace's 'Théorie des Probabilités' and of Halliwell's 'Rara Mathematica,' over a hundred unpublished autograph letters of Prince Boncompagni on the history of mathematics, numerous first or early editions of works by such writers as Newton, Descartes, Tartaglia, Cardan, Bombelli, Paciuolo, Euler and Barrow, a number of the earliest editions of Euclid, an unpublished French translation of Cantor's 'Mathematische Beiträge zum Kulturleben der Völker,' from the library of Chasles, and various similar works of bibliographical interest.

THE COMMITTEE OF ONE HUNDRED

At the meeting of Section I of the American Association for the Advancement of Science held in New York on December 29, 1906, Professor Irving Fisher, of Yale University, reported for the 'Committee of One Hundred' of which he is chairman. This committee was appointed in accordance with a vote of Section I at its meeting last July, in Ithaca, its purpose being to consider the best methods of securing the establishment of a national department or bureau of health. This vote was taken in consequence of a paper on this subject read at the Ithaca meeting by Professor J. P. Norton. There had been previous attempts to secure a national department of health, notably those of the American Medical Association, which for twenty years has reported favorably on the subject, but has been unable to secure a large interest in the project, outside of the medical profession. The present movement is not a medical movement, although the medical profession is fully represented in it. The movement was endorsed on December 13 by the joint conference at Washington of the legislative committee of the American Medical Association and the National Council on Medical Legislation before which the first draft of a bill was read prepared by Representative Barchfeld to establish a national department of health. The following is the list of the committee of one hundred as at present constituted:

COMMITTEE OF ONE HUNDRED OF SECTION I. OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, APPOINTED TO CONSIDER METHODS OF ESTABLISHING A NATIONAL DEPARTMENT OF HEALTH

Appointed from the A. A. A. S.

Dr. Wm. H. Welch, pres. A. A. A. S. and of State Bd. of Health of Maryland; professor of pathology, Johns Hopkins University.

L. O. Howard, secretary of the A. A. A. S.; chief bureau of entomology, U. S. Dept. of Agric.

Professor Irving Fisher, chairman of the committee of 100, and of the Economic Section of A. A. A. S.; prof. of political economy, Yale University.

J. Franklin Crowell, Sec. of Economic Section of A. A. A. S.; Editor of *Wall Street Journal*.

Professor J. P. Norton, author of paper on national health department read before the American Association for the Advancement of Science, on the basis of which the committee was appointed; prof. of political economy, Yale University.

Professor James McKeen Cattell, editor, SCIENCE, the official organ of the A. A. A. S.; prof. of psychology, Columbia University.

Appointed from the United States Government.

Dr. Robert M. O'Reilly, surgeon general, U. S. Army.

Dr. P. M. Rixey, surgeon general, U. S. Navy.

Col. William C. Gorgas, sanitary officer, Isthmian Canal.

H. W. Wiley, chief, bureau of chemistry, Dept. of Agric.

Dr. Cressy L. Wilbur, chief statistician, Vital Statistics, U. S. Census.

A. C. True, director, office of experiment stations, Dept. of Agric.

Chas. P. Neill, commissioner of labor, Dept. Commerce and Labor.

James R. Garfield, chief bureau of corporations, Dept. Commerce and Labor.

Gifford Pinchot, chief forester, Dept. of Agric.

Max J. Baehr, U. S. Consul, Cienfuegos, Cuba.

A. D. Melvin, chief of bureau of animal industry, Dept. of Agric.

Amos P. Wilder, U. S. Consul, Hong Kong, China.

Gen. Leonard Wood, Gov. of Moro Province, Philippines.

Appointed from State and Local Governments.

Dr. A. C. Abbott, health officer of Philadelphia.

Dr. Thos. Darlington, health officer of New York.

Dr. Alvah H. Doty, health officer, Dept. of Quarantine, N. Y.

Dr. John S. Fulton, sec. Maryland State Bd. of Health.

Professor Chas. Harrington, sec. Mass. State Bd. of Health; prof. hygiene, Harvard Medical School.

Dr. Charles D. Smith, State Bd. of Health, Portand, Me.; prof. physiology, Bowdoin College; supt. Maine Gen. Hospital.

Appointed from Medical and Hygienic Institutions and Associations.

Dr. Joseph Bryant, pres.-elect Amer. Med. Association.

Dr. Geo. H. Simmons, sec. Amer. Medical Association.

Dr. Chas. A. L. Reed, chairman Legislation Com. of Amer. Med. Association.

Dr. Hermann M. Biggs, pres. Nat. Assoc. for the Study and Prevention of Tuberculosis.

Professor Livingston Farrand, sec. Nat. Assoc. for Study and Prev. Tbc.

Dr. C. O. Probst, sec. Amer. Pub. Health Assoc. and Ohio State Bd. of Health.

Professor L. Emmett Holt, sec. Rockefeller Institution.

Dr. J. H. Kellogg, supt. Battle Creek Sanitarium.

Dr. E. L. Trudeau, Adirondack Cottage Sanitarium, Saranac Lake, N. Y.

Dr. Prince A. Morrow, pres. Amer. Soc. Sanitary and Moral Prophyl.

Dr. Dudley A. Sargent, pres. Boston Health League.

Dr. Luther H. Gulick, pres. Amer. Physical Education Society.

Appointed from Physicians and Hygienists.

Dr. Frank Billings, pres. Assoc. Amer. Physicians; prof. of medicine, Rush Medical College, Chicago.

Dr. Henry B. Favill, prof. of therapeutics, Rush Med. College.

Dr. P. M. Jones, ed. *Jour. California Med. Assoc.*

Professor E. O. Jordan, ed. *The Journal of Infectious Diseases*, Chicago.

Dr. Quitman Kohnke, formerly health officer, New Orleans, Covington, La.

Dr. Richard C. Newton, formerly ed. *Jour. N. J. Med. Assoc.*, Montclair, N. J.

Appointed from Other Associations and Institutions for Human Betterment.

Felix Adler, pres. Ethical Culture Assoc.

Miss Jane Addams, Hull House, Chicago, Ill.

C. Loring Brace, sec. Children's Aid Society.

Mrs. Melvil Dewey, sec. Lake Placid Conf. on Home Economics.

Professor S. M. Lindsay, sec. Nat. Child Labor Com.

E. R. L. Gould, pres. City and Suburban Homes Co.

Mrs. Ballington Booth, Volunteers of America.

Edw. T. Devine, gen. sec. Charity Organ. Soc., N. Y.

Rev. Josiah Strong, pres. Amer. Institute of Social Service.

John Graham Brooks, pres. Nat. Consumers' League.

Adna F. Weber, sec. Amer. Assoc. for Labor Legislation.

Professor Jeremiah W. Jenks, pres. Amer. Econ. Association.

Robert Treat Paine, pres. Amer. Peace Assoc.

Daniel C. Gilman, pres. Nat. Civil Service Reform League.

William R. George, 'George Junior Republic.'

Robert S. Woodward, pres. Carnegie Institution, Washington, D. C.

J. Eugene Whitney, sec. People's University Extension Soc., N. Y.

Austin G. Fox, chairman, Public Health Defense League.

Experts on Various Phases of Health Work.

Professor Francis G. Benedict, prof. of chemistry, Wesleyan University.

Dr. Jaques Loeb, prof. of physiology, Univ. of California.

Professor M. E. Jaffa, prof. of chemistry, Univ. of California.

Professor Ellen H. Richards, prof. of sanitary chemistry, Mass. Inst. of Technology.

Professor Franklin C. Robinson, prof. of chemistry, Bowdoin College.

Professor F. F. Wesbrook, prof. of pathology and bacteriology, Univ. of Minnesota.

Professor Sam'l H. Woodbridge, prof. of heating and ventilating, Mass. Inst. of Technology.

Appointed from Educational Institutions.

Pres. James B. Angell, pres. Univ. of Michigan.

Dr. J. S. Billings, librarian, Pub. Libraries, New York City.

Professor R. H. Chittenden, director, Sheff. Scientific Sch., Yale University.

Pres. Chas. W. Eliot, pres., Harvard University.

Pres. Arthur T. Hadley, pres., Yale University.

Pres. G. Stanley Hall, pres., Clark University.

Miss Hazard, pres., Wellesley College.

Booker T. Washington, supt., Tuskegee Inst.

Clergymen and Lawyers.

Rev. Lyman Abbott, ed. *The Outlook.*

Rev. W. G. Eliot, Portland, Oregon.

Rev. C. H. Fowler, M. E. Bishop, N. Y. City.

Rev. Edw. Everett Hale, Chaplain, U. S. Senate.

Rt. Rev. John Ireland, Archbishop of St. Paul.

Professor James B. Ames, Dean Harvard Law School.

Hon. Ben. B. Lindsey, Juvenile Court, Denver, Col.

Hon. John D. Long, Ex-Sec. of Navy; Ex-Gov. of Mass.

Hon. Wm. K. Townsend, Judge U. S. Circuit Court of Appeals.

Additional Members.

Professor Liberty H. Bailey, prof. of agriculture, Cornell University.

Luther Burbank, horticulturist, Santa Rosa.

Andrew Carnegie, philanthropist.

James H. Causey, health and political reform, Denver, Col.

Miss Grace H. Dodge, Working Girls' Clubs, New York City.

Thos. A. Edison, inventor.

Horace Fletcher, writer on health.

Professor Harry A. Garfield, prof. of politics, Princeton.

Professor Franklin H. Giddings, prof. sociology, Columbia University.

Professor C. R. Henderson, prof. of sociology, University of Chicago.

Mrs. Mary F. Henderson, writer on health.

John Mitchell, pres., United Mine Workers.

Melville E. Stone, gen. manager, Assoc. Press.

Talcott Williams, editor.

Michael Vincent O'Shea, prof. of education, Univ. of Wisc., Madison, Wisc.

Certainly if eminence and determination count for much, the committee as above enumerated ought to succeed. The committee consists of persons of large influence, including, as it does, such men as President Eliot, of Harvard University, Professor Welch, president of the American Association for the Advancement of Science, Mr. Andrew Carnegie, Mr. Thos. Edison, Luther Burbank, Gen. Leonard Wood, Dr. L. Emmett Holt, Dr. Trudeau, Felix Adler, Jane Addams, Lyman Abbott, Archbishop Ireland, etc. The few who have been compelled to decline membership on the committee have in almost every case expressed their approval of its objects. Thus ex-President Grover Cleveland wrote: "I hope I need not say to you that I am in complete sympathy with the aims and purposes of this organization." In somewhat the same manner Dr. Andrew D. White, former president of Cornell University and Ambassador to Germany and Russia wrote: "The paper enclosed from Dr. Norton seems to me masterly and thoroughly well adapted to its admirable purpose. I need hardly say that I am in entire sympathy with your movement."

Professor Fisher is now engaged in selecting a sub-executive committee which will draft a bill for congress and plan the future campaign. The committee of one hundred is not as yet pledged to support any particular form of organization for the proposed department or bureau of health, and its first duty will be to decide whether it is advisable to attempt to secure a department with a cabinet officer at its head or a bureau under one of the existing departments.

SCIENTIFIC NOTES AND NEWS

Professor Dimitri Ivanovitch Mendeleef, the eminent chemist, director of the Russian Bureau of Weights and Measures, died at St. Petersburg on February 2, at the age of seventy-three years.

Sir Michael Foster, professor of physiology at Cambridge from 1883 to 1903, secretary of the Royal Society from 1881 to 1903, president of the British Association in 1899, and member of parliament for London University, died on January 29, at the age of seventy-one years.

The French government has made Professor Simon Newcomb, U. S. A. (retired), commander of the Legion of Honor.

A bill has been reported in the senate promoting to be major-surgeon in the army Dr. James Carroll, curator of the Army Medical Museum and professor of pathology in George

Washington University, for his investigations on yellow fever.

Professor William James, of Harvard University, our most eminent student of philosophy and psychology, celebrated his sixty-fifth birthday on January 11, and retired on January 22 from the active work of his chair. Professor James is at present giving a course of eight lectures on 'Pragmatism: a new name for an old way of thinking,' before the departments of philosophy and psychology of Columbia University.

The Cullum medal of the American Geographical Society has been awarded to Dr. Robert Bell, F.R.S., chief geologist of the Canadian Geological Survey.

The University of Edinburgh has conferred the honorary degree of doctor of laws on the Prince of Monaco for his scientific investigations.

Mr. Alexander Agassiz has left on the yacht *Virginia* for a scientific cruise in the West Indies.

Dr. Edward L. Nichols, professor of physics in Cornell University, president of the American Association for the Advancement of Science, has left for a trip abroad. He expects to visit Algeria and Sicily and to spend several months in Europe, returning to the United States in September.

Dr. William Trelease, director of the Missouri Botanical Garden, left St. Louis on January 24 for an expedition to the West Indies which will last about two months.

Dr. Gordon will, during the present summer, carry on ethnological researches on behalf of the Peabody Museum, Yale University.

Professor Emil von Behring, of Berlin, will spend some time at Capri, for the recovery of his health.

Professor Friedrich von Esmarch, professor of surgery at Kiel, celebrated his eighty-fourth birthday on January 9.

Professor Geo. W. Jones, professor of mathematics at Cornell University, will retire at the end of the present year, after thirty years of service in the institution.

Mr. Frederick T. Gates has succeeded Mr. Robert C. Ogden as chairman of the general education board, endowed by Mr. Rockefeller with $10,000,000.

Robert M. Chapin, instructor in chemistry at Amherst College, has accepted a position as assistant chemist in the Bureau of Animal Industry.

Professor Josiah Royce, of Harvard University, is giving a series of five lectures at the University of Illinois. His general subject is 'Loyalty as an Ethical Principle.' The special topics of the five lectures are: 'The Problem of Ethics'; 'Four Ideals of Personality'; 'Loyalty as a Personal and Social Ideal'; 'Loyalty as a Factor in American Life'; 'Personality and Immortality.'

Professor R. S. Chittenden, of Yale University, will lecture at the University of Illinois during the month of May on 'The Physiology of Nutrition.'

Dr. Elmer E. Brown, U. S. Commissioner of Education, will deliver a course of lectures at the summer school of Yale University.

Professor W. Somerville, recently appointed Sibthorpian professor of rural economy at Oxford University, delivered his inaugural address on February 1, his subject being 'The Place of Rural Economy in the University Curriculum.'

Dr. Howard A. Kelly, professor of gynecology in the Johns Hopkins University, is preparing an illustrated cyclopedia of American medical biography, to be issued in several volumes. The work will include sketches of the careers of all the medical worthies of the United States and Canada from the earliest times to our own day.

It is proposed to found an invalid home for physicians and a medical library in honor of the twenty-fifth anniversary of the death of the Russian surgeon Porogoff.

Professor Wilbur Samuel Jackman, who held the chair of teaching of natural science in the School of Education of the University of Chicago and was principal of the elementary school, known for his publications on nature study, died on January 28, at the age of fifty-two years.

Dr. George B. McElroy, for many years professor of mathematics at Adrien College, died on January 29, at the age of eighty-six years.

The death is announced of Mr. Frederick Stearns, a business man of Detroit, who made archeological collections, which he presented to the University of Michigan, the Detroit Museum of Art and other institutions.

Miss Agnes Mary Clerke, known for her writings on astronomy, died in London, on January 20, aged sixty-four years.

Professor Adam F. W. Paulsen, director of the Danish Meteorological Institute, died in Copenhagen on January 11, at the age of seventy-four years.

The deaths are also announced of Dr. Michael Konowalow, professor of chemistry and director of the Polytechnic Institute at Kieff, and Dr. Ennon Jürgens, professor of mathematics at the Technical School at Aachen.

The Liverpool School of Tropical Medicine sent Professor Ronald Ross, C.B., last year to Greece to study the question of the prevalence of malaria there. As the result of that visit the school has made a strong effort to raise funds in England for combating malaria in Greece and has despatched to Sir Francis Elliot, British minister at Athens, £200, being the amount of the first donations collected.

At a meeting held at Brussels on January 29 at the residence of Minister of State Beernaert, it was resolved to organize a new Belgian Antarctic expedition.

The Peabody Museum of Yale University has received the geological and archeological collection of the Ingham Institute, which came into the possession of the University by the bequest of William Lampson.

Arrangements have been made to establish at Chemnitz a training school for aeronauts and constructors of air ships. A similar school has been in operation in Paris for a year past. A one year's course is contemplated for the present, the school to be opened in May, 1907. This course, at the outset, is limited to the construction and use of balloons. It will be enlarged so as to include aeroplanes, as soon as practical working types have been developed.

Reference was made in the issue of Science for January 25 to the establishment during convocation week of the 'American Entomological Society,' whereas it should have been the 'Entomological Society of America.' The American Entomological Society was organized February 22, 1859, as 'The Entomological Society of Philadelphia,' was incorporated under this title, under the laws of Pennsylvania, April 11, 1862, and changed its name to The American Entomological Society, February 23, 1867. Since 1876 it has been located in the Academy of Natural Sciences of Philadelphia. The thirty-second volume of its *Transactions* is now being published; the present president is Philip P. Calvert, Ph.D.

The London *Times* states that the fourth annual meeting of the Association of Economic Biologists opened at Cambridge on January 9, in the pathological department of the university. The objects of the association are to discuss new discoveries, to exchange experiences, and carefully to consider the best methods of work; to give opportunity to individual workers of announcing proposed investigations, so as to bring out suggestions and prevent unnecessary duplication of work; to suggest, when possible, certain lines of investigation upon subjects of general interest; and generally to promote and advance the science of economic biology in its agricultural, horticultural, medical and commercial aspects. The work of the association includes the various problems connected with economic botany, such as the fungoid diseases of plants and animals; those connected with economic zoology, such as the many problems in connection with insects and other animals injurious to crops, live stock, animal parasites, etc., the scientific cultivation of plants and breeding of animals, and the questions affecting the various natural history products that enter into commerce. The attendance included Professor F. V. Theobald, the outgoing president, Mr. A. E. Shipley, the new president, Mr. F. Darwin, Professor Nuttall, F.R.S., Mr. R. H. Biffen, Professor Carpenter, Dr. Macdougall, Professor Howard Marsh, Dr. Freeman, Dr. Williamson, Professor Robinson, Mr. C. Warburton, Mr. Herbert Stone, and Mr. Walter E. Collinge (hon. secretary). The report of the council noted the steady growth in the numerical strength of the association; with thirty-one members elected at this meeting there was now a membership of 112. Mr. Shipley's address was on 'Sea Fisheries.'

REFERRING to the plebiscite of eminent Frenchmen recently reported in this journal *The British Medical Journal* says: "A Paris newspaper recently invited its readers to reply to the question, who are the ten greatest Frenchmen of the nineteenth century? Fifteen million votes were recorded, with the interesting result that Pasteur was at the head of the poll with 1,300,000 votes. His majority over Victor Hugo, who was second, was 100,000. Napoleon was fourth. Among the ten men in the list were the late Professor Curie and Dr. Roux of the Pasteur Institute. The others were, with the exception of one or two men of letters, politicians such as Carnot, Thiers and Gambetta. The list is interesting as showing the high place which science holds in the popular mind of France. How different would the result of a like appeal to public opinion be in this country! The general composition of such a list of the 'greatest men' of Great Britain might easily be foretold. It would include politicians, preachers, two or three soldiers, and one or two popular novelists; science, and particularly that applied to the art of healing, would be nowhere. When the Order of Merit was created there was naturally a good deal of difference of opinion as to the names proposed. There was one name as to which disagreement could scarcely have been expected; yet in more than one of the alternative lists suggested the name of Lister was conspicuous by its absence. This particular form of stupidity scarcely exists in France. It may be, as has been suggested by M. Jules Clarétie, that the choice of Pasteur shows that the gratitude of the French people goes out towards the man who saves life rather than to 'the saviour of society' or the 'idle singer of an empty day.' We are inclined to think that there is more than this in the preeminence accorded to a man of science—that there is appreciation of the value of knowledge for its own sake, and appreciation of the work of those who add to it. But even on the assumption that Pasteur has been pronounced the greatest Frenchman of the last century by the selfish regard of his countrymen for their own well-being, is it not a striking proof of the intelligence of a people that it can perceive the worth of such a man? It is surely a disgrace to us that the name of Pasteur is probably better known in this country as a bogey of obscurantists than as one of the founders of scientific medicine."

WE learn from the London *Times* that through the generosity of Mr. W. A. Cadbury, the valuable collection of *algæ* made during the last thirty years by Mr. E. M. Holmes, F. L. S., curator of the Pharmaceutical Society's Museum in Bloomsbury-square, has been acquired for the botanical department of the University of Birmingham. The collection includes about 13,000 specimens, and has the reputation of being, apart from the national collections at the British Museum and Kew, the best collection of *algæ* in Great Britain. In certain respects it is, indeed, unique, in that the British portion of the collection, largely the outcome of Mr. Holmes's personal activity as a collector, includes three or four species which have been found but once. The foreign portion of the collection is as nearly complete as it could be made. All the specimens are well displayed and mounted, since it had been throughout the collector's aim that they should not remain in private hands, but be fitted in all ways for public purposes. Mr. W. A. Cadbury has desired, as a condition of gift, that the collection shall be accessible to algologists generally, at times and under conditions which may be convenient to the staff of the botanical department of the university.

AT the monthly general meeting of the Zoological Society of London held on January 17, the report of the council for the month of December last was read by the secretary (Dr. P. Chalmers Mitchell, F.R.S.), in which it was stated that during that month 150 additions had been made to the society's menageries—viz, 67 by presentation, 16 by purchase, two received in exchange, 61 received on deposit, and four born in the gardens. Amongst these special attention was directed to a pair of Siberian dholes (*Cuon alpinus*), from Thian Shan, received in exchange on December 2, new to the collection; to a Cape hunting-dog (*Lycaon pictus*), from South Africa, purchased on December 1; to an Addax Antelope (*Addav naso-maculatus*), from North Africa, presented by the Duke of Bedford on December 18; and to a Bubaline Hartebeest (*Alcelaphus bubalinus*) and a hybrid between Père David's deer (*Elaphurus davidianus*) and the red deer (*Cervus elaphus*), deposited on December 29. The report further stated that the number of visitors to the society's gardens during the month of December had been 15,405, showing a decrease of 4,112 visitors as compared with the corresponding month of the year 1905, caused by the inclement weather experienced during the Christmas holidays. The total number of visitors during the year 1906 had amounted to 896,423, or an increase of 180,943 as compared with the total number of visitors (715,480) during the year 1905. The report also stated that the total amount of money received for admission at the gates had amounted to £22,359 2s. 2d., against the sum of £17,469 6s. 4d. received from the same source during the year 1905. The total number of fellows elected during the year 1906 had amounted to 371, showing an increase of 62 as compared with the total number of fellows (309) elected during the year 1905.

UNIVERSITY AND EDUCATIONAL NEWS

Rensselaer Polytechnic Institute has received a gift of $1,000,000 from Mrs. Russell Sage. The money will be used for the School of Mechanical and Electrical Engineering. Mrs. Sage has also given $1,000,000 to the Emma Willard School of Troy, and $250,000 to the international committee of the Young Men's Christian Association.

The establishment and permanent endowment of Peabody College for Teachers, at Nashville, Tennessee, is now assured. The Tennessee legislature has just passed a bill donating to the college $250,000. The city of Nashville has given $200,000 and the county of Davidson $100,000, making in all from these sources $550,000. These gifts have been made in response to a proposition from the Peabody Education Board to endow the college with $1,000,000 when the above amounts were available All the conditions imposed by the Peabody Board have now been complied with and it only remains for that board to organize the institution. The college will thus have $1,550,000 in money. In addition to this, the university of Nashville has donated the campus and buildings now occupied by the college, valued at $250,000. It is understood also that gifts will be received at once from other sources amounting to about $1,000,000. Should this hope be realized, the college will start on its new life with an endowment of $2,000,000. It is the purpose of the authorities of the institution to solicit other funds for the erection of the necessary buildings.

By the will of Arthur Mills, of Brookline, Harvard University will ultimately receive $150,000.

Professor Schuster has offered to the University of Manchester during the next three or four years an annual sum of £350 as the stipend of a reader in mathematical physics.

Through the death of Mrs. John Daglish, Armstrong College, Durham, will receive a bequest of $25,000 for the establishment of a traveling fellowship in mining.

Dr. Ellis E. Lawton, instructor in Yale University, has been appointed associate professor of physics.

The University council of Liverpool University has elected Mr. Percy E. Newberry to the Brunner chair of Egyptology and Mr. John Garstang to the John Rankin chair of methods and practise of archeology, both of which chairs were recently established by Sir John Brunner, M.P., and Mr. John Rankin.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, FEBRUARY 15, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

RELATIONS OF SALARY TO TITLE IN AMERICAN UNIVERSITIES[1]

IN this paper is considered the problem of the relation existing between salary and title, under the conditions ruling in American colleges and universities. What adjustment of these relations is most favorable to the effectiveness of the institution concerned? In general, three types of adjustment are possible. In the one case, a fixed salary may be attached to the professorship and to each of the lower grades of rank. Next, each grade may have a fixed minimum salary, with a system of automatic increase with length of service, and for no other cause. The third relation is the one generally prevalent; the salaries in any grade are not definitely fixed, and increase of salary may be made at any time and for many reasons other than those connected with length of tenure.

The first of these systems aims, so far as professors are concerned, to establish a republic of letters. It would develop a condition in which a man once chosen for a chair is responsible to no one but himself, and in which he neither expects promotion nor fears its failure, because his character and work are judged by no president, no committee and no executive board. The men in minor positions are professors in waiting, to receive recognition in case of vacancy or of departmental expansion.

[1] A paper read at the Cambridge meeting of the Association of American Universities, November 23, 1906. The paper was prepared by John Maxson Stillman, professor of chemistry in Stanford University.

These again are held on an equality pending the opportunity to rise to greater responsibilities and greater remuneration. The second system is a modification of the first, with the added recognition of the fact that, with university men, the expenses of living increase with the years. The third system considers the problem from the standpoint of the efficiency of the university organism and of the actual value of the professor to his students. In it, the element of competition appears, and the greater pecuniary reward goes with the greater academic service. The first and second systems imply a static organism, a university with its form and scope fixed once for all, and the professors as incumbents of established positions. The third system is dynamic. It implies the growth of the university organism, and the value of personality as a factor in different phases of growth.

Taking universities as they are, the institution is not a republic of letters on the one hand, as there are students as well as professors to be considered. On the other hand, it does not find its homologue in a great business enterprise. It is not alone what the members of the faculty do, but the ideals they represent, which is important.

In the practical discussion of the first and second of these systems, we may assume that if promotion is impossible or automatic within the grade, the promotion from one title to another is not likewise automatic. In such cases there would be no possibility of any discrimination between men of different value except by the difficult method of dismissal of such as fail to reach the plane of efficiency desired in the highest positions.

One of the most obvious arguments in favor of like remuneration for equal grades is that it relieves the university authorities from the difficulty of attempting to assign different money values to services extremely difficult, even impossible, equitably to appraise. No university president and no board of trustees, nor indeed any other body of men, can have the intimate knowledge of the values of the services of men in a university faculty adequate for the establishment of just discriminations in salary on the basis of service rendered. Even if we assume such intimate knowledge, we are at once confronted with even greater difficulties of establishing the standards of judgment as to the value of these services. The elements entering into the value of a university teacher are many;—*e. g.*, originality, scholarly productivity, teaching capacity, industry, energy, personal influence, character, executive capacity. Different men place the emphasis very differently on the relative value of these different qualities, and agreement as to their relative importance is impossible. Such being the case, discriminations in salary between men of approximately equal standing can not be equitably administered.

When, therefore, it is attempted to establish such discrimination, there results dissatisfaction in the faculty. Faculty members criticize the discrimination in the light of their own knowledge of the men and their work and according to the emphasis they place upon their various qualifications. This engenders jealousies and gives rise to attempts to influence the president to recognize the claims of individuals, and cliques and factions are created. Stimulated by the uncertainties as well as by the possibilities of the shifting basis of salaries, political methods, personal influence, and 'wire-pulling' become prevalent. Discontent, lack of harmony among the faculty members, and between faculty and president, are the natural consequences. The president is charged with favoritism, and professors are accused of exerting undue influence on behalf of their own interests or of the interests of their friends or favored subordinates. I have endeavored to present this argument strongly, and we must admit that there is justice in the objections to the attempt to estimate closely the values of men by difference in salary.

On the other hand, if we admit that harmony would be promoted to a certain extent by the same pay to men of the same title, we must consider at what cost this harmony must be secured.

The above argument depends for its validity upon the assumption that the pay is an important element in the ambition and desires of the university teacher. It would be strange if it were not. Uniformity in pay, if it is to be a satisfactory condition, assumes at least approximate equality of value to the university. It is very evident that no such condition exists in any university faculty. In any faculty there are wide differences in the value of different men to the university, whatever criteria of value may be assumed. It is neither fair nor just to expect men of exceptional value to be satisfied with salaries paid to men of distinctly inferior academic value. There is injustice in not recognizing increasing influence, scholarship and general usefulness by commensurate increase in salary. Nor should it be necessary to pay men of mediocre value the higher remuneration which is fairly deserved only by the strongest men.

If it be argued that none but men of approximately the same ability and value should hold the same rank, it can be asserted that such a condition is practically unrealizable, as may be easily verified by considering any given faculty. It is a matter of greater difficulty for a president and trustees infallibly to select the strongest men only for professors, than it is to properly appraise their services when in the university service. Appointees do not and can not equally fulfill the hopes and expectations under which they were appointed, but once appointed they can not be summarily dismissed to make place for greater men, so long as with a fair degree of scholarship, industry and devotion they pursue their career; but there is no justice in paying such men the same as ought to be paid to those who are of distinctly higher value to the university and to scholarship. Furthermore, when a man by reason of merit attains a full professorship early in life, if he feels that thereafter with moderate attention to duty his salary is assured without hope of increase on the basis of value rendered, an important incentive is lost to him for his future progress and development. He is deprived of a stimulus to activity and ambition not without its influence upon common human nature even in academic circles.

There is also the more utilitarian idea of supply and demand which must be taken into account. No university has unlimited means at its disposal, and the problem of administration is to perform the most effective service for education and the increase of knowledge with the means at its disposal. To fulfill its responsibilities to its students and the public, it must secure and hold the most efficient men possible. If the law of supply and demand sets unusually high the value of the good men in certain lines, or the value of exceptional men in any line, it then becomes the duty of the university to pay some men salaries which it can not afford to pay to all.

It may be claimed, on the other hand, that university teachers do not and ought not to enter the career for the commercial value of the position. The world offers other opportunities for those who seek large incomes, and the university teacher who is fitted for the work looks to enlarged opportunities for study, research, and to the love for his work as a teacher for the rewards of his success rather than to financial rewards. This is unquestionably true to some extent. Nevertheless, within the limits of salaries at present existing in the universities, the fact remains that university teachers are appreciative of and desirous of such increases in their remuneration as lie within the range of present possibilities in any university. Until the range of university salaries is distinctly higher than at present, university teachers will find that the minimum salaries paid in any institution are not so large but that they are compelled to deny themselves and their families many reasonable comforts and luxuries which are greatly desired by all people of similar culture and social status. And so long as this is true, the university must face the necessity of competing with the world outside for the services of thoroughly competent and ambitious men in many lines. For the less wealthy universities particularly, the competition for the best teachers would under a fixed salary scheme render it impossible for them to hold their strongest men, if to do so they were compelled to pay an equal salary to all holding the same title.

In consideration of all these factors in the problem, is it not probable that the gain of simplicity of administration and some measure of harmony in the faculty by the system of equal pay to equal rank would be made at too great an expense of efficiency?

We must not overlook the fact that if equal pay to equal rank were the rule, discriminations would still have to be made in the matter of promotions from one grade to the next. In promotions, not only the pecuniary consideration is concerned, but a public honor is conferred. Precisely the same variety of considerations enters into the qualifications for promotion as into salary differences. The same lack of agreement as to what relative weight should be given to teaching power, productivity as a scholar, personal influence and character, etc., exists here, and the same possibilities of jealousies, suspicions of favoritism, 'wire-pulling' and personal influence. The writer is inclined not to lay great emphasis on the dangers of such influences as necessarily incident to either system under discussion.

Wherever discriminations have to be made into which enter estimates of the relative values of such services, whether by differences in pay within the same grade or by promotion, there is bound to be some dissatisfaction and discontent. A just and wise administration will reduce these evils to a minimum by inspiring the faculty generally with confidence in the fairness and general good judgment with which such discriminations are made. Entirely eliminated, dissatisfaction and discontent can never be. At best they can be confined to those members who differ with the constituted authorities as to the relative value of their services, and, perhaps, to their particular friends. Under either system these administrative difficulties will exist and remain the same in character though differing possibly somewhat in degree. These difficulties will be lessened to a great extent by avoiding the making of small differences in pay between men of the same rank. For while the reasons may be readily apparent to the university community why a considerably larger salary must be paid to certain individuals, it will be much more difficult to justify small differences in salary to men of the same rank. For small differences in general usefulness or value to the university it will be admitted are not possible of fair estimation. They are not justified either on theoretical or on economic grounds. They tend toward discontent and irritation without material saving to the university treasury, and they can not command the general approval of the university public. When certain men stand out so prominently as to deserve to be recognized by the authorities by a larger reward than their associates, the university community generally recognizes the fact, but the reasons for such recognition should be such as to be clearly recognized. Where, however, such is not the case, then equal pay to equal rank is the safer and fairer basis. The practical working out of the salary problem to my mind should be on some such lines as the following:

Each grade or title should have a minimum salary pertaining to it. This salary should be large enough to insure comfortable living with due regard for the reasonable demands of cultured taste. Much discontent arises because the minimum salary of the various grades is often so low that men can not live as the requirements of their profession and social status demand. This inadequate minimum compels the authorities to make advances of salary to meet personal necessities, which are not always justifiable on the grounds of relatively greater service rendered. Appointments to the lower ranks should be probationary and the university should be considered perfectly free to terminate such positions, to continue them, or to promote to a higher grade in due time. Within each grade certain allowances of increase in pay should be made for length of satisfactory service. Above these minimum salaries there should be the power to advance the salary of any man when it is clearly for the interest of the efficiency of university work to do so. Generally speaking, the maximum salary of one grade should be less than the minimum of the grade above, though even here it is imaginable that a departure occasionally from this rule might become a justifiable exception. In each grade the authorities should have the option of leaving a particular teacher undisturbed at the minimum of his grade and time of service, or of advancing him in recognition of extraordinary ability or unusually valuable service. They must also have the option of promoting or of passing by any individual according to his deserts or the university's needs. In the lower grades, below that of professor, while the deserving character of a member must be considered, it must also be kept in mind that the scope of the university work and the financial limits of the university may prohibit promotions even when fairly deserved. Even very good men must often look abroad for their promotions. In the higher grades it is assumed that permanency of position is reasonably assured, and this should guarantee the minimum salary of the grade and time of service without any presumption of further increase unless fairly won by unusual distinction and recognition; but the university should then be free to recognize such service freely, both for the encouragement of scholarly ambition and to be able to retain its strongest men.

Briefly summarized, we may say that the maximum efficiency of the university work and a minimum of administrative difficulty resulting from inequalities in pay in the same grade, will be attained by a minimum or normal salary for each grade large enough so that men of reasonable desires may live and do their work and maintain their families without worry and discomfort; by reasonable increases dependent upon length of efficient service, and with freedom to recognize unusual ability or distinguished service as the requirements of the case may demand. Such recognition, however, should be made for reasons, the reasonableness of which should appeal to the university faculty generally.

Upon the assignment of this topic to Stanford University, President Jordan issued a circular letter to many presidents and faculty members asking their opinions upon the question involved. At the time of writing some hundred answers have been received. The limits of time prevent the writer from obtaining permission to quote over their signatures the many interesting answers. I append, however, quotations from some of them that are typical of certain classes of answers and which will supplement the above brief discussion. Of sixteen college or university presidents, fourteen are opposed to equal pay for equal rank, one in favor, and one answer not decisive. Of eighty-one faculty members, sixty are opposed, seventeen in favor, four gave no decisive answer. Some thirty-three emphasize the value of an established minimum for each grade, increases above which may be made for good reasons, as length of efficient service, unusual ability or general usefulness.

John Maxson Stillman
Stanford University

The following quotations are from presidents of universities:

1.

I am firmly of the belief that there should be no rigid salaries payable to all men in the university bearing the same title. In other words, I believe that the university should pay what it thinks a man is worth. If a certain department is in need of a very eminent man, it would be wise for it to pay him double the salary ordinarily given. I believe that the principle should be variation, according to ability and experience, and quality of usefulness to the institution at any given time.

2.

In my judgment, while the salaries of professors in any institution will naturally gravitate toward some given figure, I see no reason why there should not be deviations therefrom, due to the greater value to the university of some men over others or the need of larger compensation to retain such men in the service of the university. In the case of assistant professors and instructors, who are appointed for a fixed period of time, I see little, if any, objection to uniformity of salary.

3.

Sitting in an easy chair, one can argue one's way with perfect satisfaction to one's self up to the conclusion that all men having the same title should have the same salary; but I have never been able to manage a university on that principle, and I have never been able to acquire such ability. Extraordinary things are always coming in to interrupt in the application of the theory. We have here assistants, instructors, assistant professors, professors, head professors, deans, president. Some of the instructors get as large salaries as some of the assistant professors; some of the assistant professors get as large salaries as some full professors. Where action is in our hands, we prefer, within reasonable limits, to increase salaries rather than titles. Sometimes, however, a man insists upon an increase of his title, and to refuse him means to lose him whenever a good offer comes from another institution. Sometimes it is possible to increase a title and to promise the increase of salary on and after a certain date when additional money is expected. Some men insist upon increase of salary more than upon increase of title, and *vice versa*. Sometimes you must increase the salary and title both.

I need not point out that the case does not occur in which an assistant gets as much salary as an assistant professor; nor a case in which an instructor gets as much salary as a full professor; but instructors and assistant professors do run together sometimes in respect to salaries; and so also assistant professors and professors in some instances.

4.

I beg leave to say, 'No.' The reason of the answer seems to me to be summed up comprehensively in the remark that men, even college professors, differ in character and efficiency. Therefore, the pecuniary recognition may fittingly vary according to their work and worth.

The individual and not the institutional method should prevail. It is much easier to administer a college on the basis of the same compensation for men of the same professorial grade; but I believe that such administration is not wise either for the individual or for the institution itself in large relations, or fitted to promote the higher interests of the whole community.

5.

I know of no solution of this problem which seems to me entirely satisfactory.

There are those who think that a fixed system or scale of some sort should be adopted and followed. That would be the easiest plan for the trustees and the president. Perhaps it is the right plan. But after the most careful consideration it does not seem so to me. I do not find any institution such as ours where any fixed system has been found practicable, though various systems have been given trial.

A fixed system of promotions and salaries is said to work well in the army—in time of peace. In time of war it is in the army as in all other callings—the system must go to make way for the most efficient service.

Aside from the differences between men, which count in every occupation and must count among teachers as well as others, there are circumstances and conditions which vary from department to department and render a fixed system a heavy handicap upon the maximum efficiency of the university.

The university can not, for example, make nothing of the fact that it is very much more difficult to get good men in certain departments than in others. This fact constitutes part of the reason for one exceptional recommendation made at this time. In such cases the university can not afford to restrict itself artificially by a system.

On the other hand the conditions and circumstances in another department may make it extremely difficult to promote every worthy man. The commonest example, in the universities generally, is that of the department whose places are already filled by a practically permanent staff of good men. If one looks at the case of a single individual, relief seems easy. But some one must look at all the individuals. Some one must look at the budget for the year. Some one must consider what the budget will be next year and in the following years when outgoes are growing and incomes are standing still. In a word some one must consider the life of the university as a whole. When one does that, the problem of advancing all the individuals who deserve it appears not simply difficult but impossible. No university is rich enough to make places and salaries for all who deserve them. No university has a right to make place or salary for any man unless this is justified by the interests of the institution.

In fine, an automatic system is easy and peaceful. It relieves the trustees and the president from their most trying responsibility. But in my judgment it must again and again pay for this peace and pleasant irresponsibility by the sacrifice of essential university interests.

I believe that the trustees and the president must choose a harder and more troubled course. They must accept the responsibility of doing what they believe to be best and must abide the consequences to themselves and to the university.

I do not forget, as I write, the first-rate importance of a good spirit within the faculty. The maintenance of that spirit requires that the administration of affairs should be reasonable and disinterested. But if a reasonable and disinterested administration (which must often bring disappointments to individuals and which must sometimes make mistakes) does not develop a good spirit within the faculty, the whole problem seems to be hopeless.

6.

While there are diplomatic reasons for giving the same salary to men holding the same title in a college, there is no justice in it, as it is impossible to secure men of equal effectiveness in their respective positions. The question of administration is doubtless easier if professors are placed on equal salary, but there seems no sufficient reason to my mind why the laws of supply and demand should not be applied to college professors as well as in other walks of life.

7.

There is no reason why the same salary should be paid to men bearing the same academic title in other than the subordinate grades, such as assistant, tutor and instructor. I think that assistant or adjunct professors, and more particularly professors, should be compensated as individuals and not as members of a group, the amount of compensation to vary in accordance with particular circumstances affecting the nature, the quality, and the amount of their academic service.

The following quotations are from faculty members:

8.

For a categorical answer to the question I should say, *no*.

The dilemma which the question involves is in some respects similar to that which appears in the question of salaries for public school teachers, and is somewhat more remotely analogous to that which appears in the whole problem of appointment, promotion and salaries in the civil service. On the one hand, a mechanical uniformity is easy of administration and shuts out the dangers of favoritism, wire pulling and the whole set of evils that are commonly described as political. On the other hand, such a system makes it impossible to adjust the external rewards of service to differences of experience, capacity and life-needs among those to whom the schedule of salaries applies. It may be taken for granted that there will be an upper limit of salary which will be low enough to prevent the position of instructor from becoming an object of covetous competition, and there will be also a lower limit which is not too low to enable a self-respecting man to live respectably. I can well understand that even within these limits there is danger to the scientific and spiritual interests of a university in a sliding scale which may seem to emphasize purely what may be called the market value of a man; but it should be remembered, on the other side, that a thousand differences of personal and family need, of general make-up and disposition, not seriously affecting a man's scholastic efficiency, and other differences too numerous to mention, are present and must be considered and ought to be considered when a man in a given institution is offered a higher salary in another institution, or in some other occupation for which his talents may fit him. Without entering into any unseemly competition on purely financial grounds, an institution may consider, and I think ought to consider, such differences, in adjusting the salaries of instructors within such limits as are suggested above.

There is not much danger that an instructor will work for money chiefly, or will get rich even if he does, but even in a university a man who renders services of extraordinary value should have a fair opportunity of receiving a larger income than another instructor of the same scholastic grade whose services are notably inferior to his. The difference in salary will not pay for the difference in service and can not be made to pay for it; but it may render the more useful man a little more free to make the most of his useful life by travel, by acquisition of the means of culture and research, and by the various other ways which are within the purchasing power of money; and it may render him better able to help his family and friends and those who have a right to look to him for help.

9.

1. I do not think that 'the same salary should be paid to men bearing the same title.' But this under the following provisos:

2. There should be a minimum salary for each rank, no less than which each appointee should receive upon his advancement to the rank.

3. There should also be a maximum salary for each rank, attainable by those members of the rank whose abilities and performance show that they deserve it.

4. Promotions within each rank should be upon proved merit, judged in the light of the quality of departmental work, and without reference to 'university politics,' or 'work' in the manipulation of committees—in short, the study or the courting of 'influence,' whether with authorities or with students. The rate, or rapidity, of promotion should correspond to the proved value of research and teaching service.

5. Advances in salary, as in rank, should be made in sole conformity with the advice of the president, and this advice should result from consultation with the department-head under whom the candidate serves, and should, unless there is clear adverse reason, follow the head's advice.

10.

My feeling is decidedly in favor of *equal* salaries, as tending to greater unanimity of feeling among professors, and so to greater loyalty to the university. Equal salaries seem to eliminate, so far as possible, the whole element of *favor*—the *personal* equation—and this sets every one free to do his best, according to his light, for the university. Equality, too, removes the possibility of bargaining, of jewing up or jewing down a salary, according to the exigencies of the moment. This policy, too, seems to me to be, in the long run, the more dignified for the university. A man comes to it, not because he is bought at a high price, but because the university *as a whole* suits him. He takes his place in the equal brotherhood of professors, feeling that his fortunes are bound up with theirs, and so with the fortunes of the whole university. * * *

Of course equality of salaries will occasionally prevent a university from securing an able man who might have been secured by 'subsidizing' him—and it will result in some men being paid more, and some less, than the market will bear. * * *

Of course this preference for equality does not preclude an advance for years of service, so long as the advance, as at Harvard, is automatic, so to speak, and not a thing to be bargained for, or begged for. (I have heard of one interior university where advance of salary must even be 'toadied' for.) * * *

And yet in the long run I am convinced that equality of salaries tends to contentment, to fraternity of feeling, to loyalty, and to the minimizing of the force of the mercantile spirit in a faculty. * * *

My argument is intended to apply chiefly to full professors, the 'peers of the realm.' There is no objection to a money stimulus to the men who are still on probation, with their spurs to win.

11.

1. It is necessary, in my opinion, to separate the college of arts and sciences from the technical colleges in the discussion.

2. If there were sufficient men who were *called* to teach in the sense that the old apostles were called for their work, then I believe that there would be no question of diversity of salary. All that would be needed would be barely enough to keep soul and body together.

3. While there are plenty who would like the call of a university there are not enough with '*the call*' to fill college positions. This puts the colleges in the field of competition with the practical world for the all-round, capable and forceful men who are not satisfied, on the one hand, with the dead level of communism or, on the other, with things as they are and have been. Progress and experiment to attain that progress is their motto. Now to get these men to put their courage and force to the service of a college, the college must offer them something like the chance they would have in the great world, that is, a chance to receive the reward to which their force and courage entitle them.

4. To obtain the men with the force and courage which shall make the university a real and living part of the civilization of the time, the university must—in a measure at least—appreciate the reward which is offered by the world for the kind of service it desires. There is no doubt that money at the present time is taken as the measuring stick of men. Money certainly makes it possible for men to attain much which is most desired by them and their families.

After men have once caught the divine fire which comes to the college teacher, money questions would not so much affect them; what I am contending for is that the university get the forceful characters into its faculty by offering rewards which will attract them. It can not afford to be manned by those who can not get a living so easily any other way.

5. *Technical Schools.*—The same general statements may be made as with reference to the college of arts, but here the university comes into more direct competition with the practical world. If the technical school is to be an integral part in directing the progress of the civilization of our time the men who form its faculties must be among the chosen—men with strong character, clear heads and the courage and foresight to make the necessary advances. Then the schools could hope to be leaders instead of mere trailers.

Now to get a sprinkling at least of the real leaders there must be provision in both salary and rank. And the salary in the technical school must average considerably higher than in the college of arts.

6. Just the method to pursue to attract into the teaching profession the all-round, forceful men so much needed in colleges may perhaps be answered by creating special positions with corresponding salary—such as head professor, dean, director, etc. This might leave the rank and file in a group with uniform salary and therefore without one element of discord.

7. In closing then I would say that in the modern university there must be diversity of salary and rank in each college and a distinction between the college of arts and the technical schools.

While it may not be germane, I would like to put in a plea for appreciation of the teacher who is really *called.* He, after all, is as much needed as the one who can do magnificently anything he puts his brain and hand to. Money is usually much less prized by him than opportunity for study, for investigation in his chosen field.

12.

Apparently it is not practicable to pay the same salary to all men holding the same rank in a faculty, and yet a wide departure from this policy seems to work great injustice in many cases. I once believed that a university president should be free to pay whatever salaries he found necessary to secure the men he wanted, and that salaries within the faculty should be based entirely upon efficiency. I have now come to regard the other extreme, with an absolutely fixed scale, as preferable to this method.

No man is omniscient, nor can any man know with a higher degree of approximation the relative efficiencies of the men in a moderately large faculty. Even the men in the same department or in closely related departments differ widely in their estimates of any particular man. One lays the stress on one qualification and one on another. Hence, while no individual is competent to pass upon the salary scale of a whole faculty, no group of men will come to any agreement upon such a scale.

Most men are so constituted that their opinion of the qualifications of others depends largely upon their personal likes and dislikes (I know that I am personally very strongly influenced by such considerations), others are so constituted that they yield to persistent pressure on the part of one who is working for an increase of salary, and still others may be influenced by the cliques which are always formed in a faculty for the purpose of boosting their members. Hence, whenever a man receives a higher salary than others whom the general consensus of opinion would rank in the same class, there are apt to be charges of favoritism or 'pull.' In this way, the influence of the president who has the fixing of salaries is bound to be greatly weakened. * * *

Again, assuming that a man of absolutely impartial mind and of wide information could be given the authority to fix salaries, there is no general agreement as to the grounds upon which distinctions should be based. One man is an excellent teacher and exerts a great influence upon the lives of his students; another is a great investigator and does not know the names of his students. Which should receive the greater salary? Most men are neither great teachers nor great investigators, but one excels in some particular and another in something else. How shall we say which is entitled to the greater salary?

I am aware that absolute uniformity is impossible, even if it were desirable. Certain departments are compelled to pay more for men of equal ability and preparation than other departments. At any one time, there are living in the world only a few first-class men in any profession and a university which is able to afford the luxury of such a man should be compelled to pay for it. However, the total number of such men in the world is not large enough to make it necessary to take them into consideration in deciding upon the general policy of a university. So I believe that university salaries should, as far as possible, be fixed upon an arbitrary basis, taking into consideration the rank and time of service of the men, and that departures from this fixed scale should be made only for weighty reasons, reasons which would be recognized by the faculty as a whole.

13.

I may now say very briefly that I think university teachers of the same title should not receive the same remuneration. With such a plan I believe we would have too serious an interference with the great law of supply and demand. In our academic guild there are already too many impediments to the free working of the law of the 'struggle for existence and the survival of the fittest.' The principle of the 'redemption of the unfit,' which our too pious altruistic brethren are introducing as a counter law to the law of evolution, is being overworked in these times.

Everywhere in life, a man should be paid what he is worth. I know that many of my colleagues say that this would introduce commercialism into university life. It is often said that university men can not do their best work if they have to be continually thinking about their salaries. This is in one sense profoundly true, but I believe that it is often the argument under which idlers take refuge.

14.

Fundamentally, a sharp distinction must be recognized between the academic and the administrative requirements of the institution. This underlies the question at issue. The university is required to exist in a commercial world, and meet financial conditions on a commercial basis, while, at the same time, it must meet its obligations to the ideals for which it stands. The university thus is forced to maintain its standards at variant costs, and direct its progress by paying prices that it does not itself control, when they are disproportionate to the true academic returns. This is because of the outside standards of value, which do not accord with the necessary ones within. The discrepancy holds true for every item of expenditure, and among the other items are included the salaries.

There then arise two classes of considerations for which salaries are paid. One is that for services which perhaps have little or no commercial value, rendered by persons who are thoroughly dependent upon college interests and college standards. The other is for services purposely attracted away from commercial competition by liberal payment. The two classes, or purposes, are separate and must be regarded separately; for they are made necessary by different causes, they administer to different needs, and they bring about different results. One is strictly academic, and the other is thoroughly commercial; and each is a factor in practically every salary.

The true academic purpose is steady, continuous, uninfluenced directly by commercial considerations, and ultimately is desirable. Such services are easy to grade. Salary in that case conforms closely to title; and acting with title it becomes an expression of university approval and merit.

Commercial considerations, however, can not be bounded so. The commercial purpose is fluctuating, insincere in the college field, and rests upon a commercial basis and outside standards. It is always subject to influence acting on the outside, and these break into any desirable order of things within.

Therefore, when necessary, commercial influences should be met according to their demands, not restricting or limiting the power of the university to do this, but observing its best interests while protecting its standards. The salary is the means that makes the fulfilment in that case possible and is the only compensation the college can afford to offer commercialism. It could be shown, on the whole, to be unwise for a university to go far into extreme commercial competition for men, and deal in such commercial margins upon men as have accrued from popularity, or from success in some one line of commercial activity.

Thus salary, in part, must be held under a commercial standard, while title always must be considered under an academic one. The two standards do not conform, and no effort could draw them naturally together in defiance of commercial law and custom. A uniform scale of salaries, graded according to title, or defined by the title, is an ideal to be approached as closely as outside conditions will permit. Such uniformity, however, is not sufficiently supple for working purposes; and for practical reasons, the best results are obtained by a moderate departure from it in different ways, limited not by defined bounds, but by conservative administration, retaining always the ideal in view.

15.

Let me say in answer to both questions that I think the same salary should not be paid to men bearing the same title. There should be an identical minimum salary to be paid to all men bearing the same title with such additions in individual cases as the university may deem wise, in order to recognize the moderate natural increase, up to a certain limit, which ought to be given to all members of the faculty who do faithful service, and the exceptional increase which ought to be granted to men of exceptional value.

16.

The question strikes me as scholastic.

University organization can not be modeled upon the army. The best interest of university service prohibits that it should be enterable only at the bottom. University professorships high and low should be open to competition. Professors do not need the shelter of benevolence nor to be entrenched behind the contract esteem of benefactors. Universities should be free to get the best men their resources will command. Hence no rigid connection between title and salary roll is advisable.

17.

I beg to offer as my opinion:

That the same salary should not be paid to men bearing the same title. I feel that in the lower grades of the instructing force different sums should be paid according to the character and amount of work. In the grades of instructors, assistant professors and associate professors I think there should be a minimum and a maximum limit—that in general, advancements or appointments should be made at the minimum figure, and there should be a regular automatic increase in those salaries until the maximum limit is reached; then it may be desirable to retain the person at that salary either permanently or for some time, until he has shown his qualifications to be raised to the next grade.

When it comes to the full professorships, I think again there should be a minimum salary, and that the advancement to what might be termed the 'regular' salary of full professor should be, as in the lower grades, automatic and regular; that above the regular salary there should be exceptions made upon the sole consideration of the value of the individual to the university. That means discrimination, and I believe in discriminating between the good, the mediocre, and the bad.

18.

It seems to me the fairest method is that a certain minimum salary should be attached to each title and that the appointment to such a place would necessarily carry with it this minimum salary, but there should be possibilities of individual increase over this minimum. The factors which should determine this differential are various. The success of the professor in his particular field, either as a teacher or as an investigator, or as a leader of public thought, or interest in university activities should be followed by some recognition in the way of increased salary. I am afraid that, if promotion were simply a matter of time or routine, there would be a distinct lowering of effectual effort for advance. The more highly one prizes an academic grade, the greater should be the possibilities of this differentiation.

While academic life is not strictly comparative with business life, they should have certain analogous elements. It is easy to get one-thousand-dollar men in business, much more difficult to get five-thousand-dollar men, and almost impossible to get ten-thousand-dollar men, but a ten-thousand-dollar man is worthy of his hire. So in the university, the ten-thousand-dollar man should have his corresponding reward. Of course, the elements which make for success in an academic career are not the same as in a business career. The standards are different, the aim is different, but what I want to bring out is that the value of men is so different that they can not be fairly classified by the ordinary academic grades, and, while in the university the money reward is not the sole object of the professor's work, it should form a certain element of it.

19.

In my opinion professorships within the same university should unquestionably be placed upon a like financial basis.

I find the most emphatic argument for this in the evils that almost inevitably accompany any other disposition. Those evils, indeed, seem to me a most serious menace to the amelioration—so much needed—of the professor's standing. In the absence of such a system or practise, the individual professor is likely to spend serious efforts in enforcing his claims to securing such advances in salary as he can effect. Most directly and most frequently he encourages offers of affiliation with other institutions. He particularly suggests, when such inquiries come, the necessity of additional financial inducements to secure his transfer; at other times the university aspiring to secure his services at once holds out the lure of additional income. If he accepts such an offer, he is likely to find in the new environment that he has been engaged at a salary denied to many of his colleagues of longer service, of greater adaptation to the needs of that institution, of equal reputation and attainments. Such a position should be more generally embarrassing than it seems to be. If he declines the overture, he is likely to yield to the temptation to demand of his present authorities that they compensate him for the loss he has incurred by declining the 'call.' The commercial standards that thus enter degrade the proper appreciation of academic standards and prevent the emphasis upon the essential factors of academic compensation. There are to-day many men of first-rate character and value receiving most inadequate salaries, while in the same faculty are a few men with far better incomes whose greater freedom from care is due merely to the fact that they entered the institution at a later period of its history and have not to their credit years of self-sacrificing service. Such a university actually punishes those who have aided to build it up. It may be replied that this difficulty could be avoided by increasing salaries from within as generally as from without. I reply that the spirit of this method is against such procedure; and that a complete adjustment would amount to nothing less than an equality of salary.

I shall say little of the feeling of personal injustice, of jealousies small and great, proper and improper, that arise under the system that allows each man to fight for himself alone. I mention the fact that, struggle against it as we will, men will be rated by the salaries they receive. Academic democracy is hampered in its expression, and men are judged by false standards. It is but an exaggerated expression of this attitude—something that hangs in the air and contaminates—that induced more than one graduate student in a certain but nameless institution to look up in the proper report the salaries of the several professors under whom study was contemplated, and to choose those with the largest figures to their credit. They wanted their 'majors' only under at least '$3,000' men. This is the rating that figures in the Sunday issues of our great and representative dailies.

The fact that the only practicable mode of avoiding the inevitable difficulties, injustices, inequalities, and pernicious influences of a system that leads each man to struggle for himself, is to adopt the system of equality: this alone seems to me an adequate reason for the system I advocate. Yet it seems to me that equally with the avoidance of evils is there in the 'equality' system the greatest good, alike in principle and in practise. The very freedom from care and unrest and uncertainty, and the consequent emphasis placed upon the incumbent's devoting himself to his proper interests, is a great step in itself. Nor can I see why any president or board should desire to complicate matters by attempting to differentiate among equally, or nearly equally, worthy men by a financial standard. It is sometimes said that the business of a president or of a board is to translate academic utility into money values, a task for which a composite of

Solomon, Job and Socrates would be inadequate. The thing is an obvious impossibility, and, as said before, a crude attempt to force the distinction places a most undesirable emphasis upon a distinction that fundamentally has no place in the academic life. The very fact that a president is willing to prove to his board that Professor A. is worth $2,600 to the university, while Professor B. is worth $2,700, seems to suggest rather forcibly that the result might better be left to the throw of a die.

We must also remember that under present circumstances these discriminations may mean all the vital difference between finding a debit or a credit in the year's accounting. If professors were paid upon an adequate basis, the problem would shift in importance, though the relative value of principle would be the same. A salary should secure a reasonable, comfortable living. The salary is intended to permit one to live and pay one's bills; those bills are largely determined by the standard of living. The butcher and the baker—unlike the graduate student—do not consult the salary list before making out their bills. These come in to the fortunate and the unfortunate alike.

Nor can I see any useful purpose that a differentiation of salary serves. I have never heard any defence thereof that at all aims to set forth its utility. It is generally set forth as a practical necessity. A certain man can be had only by offering him a certain salary. Trustees are influenced by these superficially business-like considerations; and so the specious argument with its attendant evils returns and grows in force. Yet in the long run, the university that strains its maximum efforts to pay adequate salaries will reap the benefits of its worthier policy. Indeed that is the case to-day. To anticipate the occasion of a summons elsewhere, to place the emphasis upon academic privileges, to make it clear that the best the university can do is already done, and is not withheld until a 'hold-up' forces the situation, is more likely to attract and retain the proper kind of man than any shrewd juggling with the translation of academic deserts into dollars and cents.

I do not address myself to the practical problem—related yet different—of providing a system for the proper advancement of men from subordinate to the higher positions. I believe the issue in such cases is properly that of determining by academic standards when and whether the candidate is to be advanced to a higher rank. Some should be advanced more rapidly than others. Such differentiation is part of the selection that is as necessary in the academic as in any other career. But once selected, the further differentiation of salary should be affected by no other consideration than time of service, and such other regularly provided conditions as belong to every man's career.

20.

As an ideal, the payment of uniform salaries to all who may bear the same title would seem highly desirable, inasmuch as it would in effect recognize the equal value or importance of one department with another.

As a matter of practical university administration, it will always be difficult to realize any such ideal, and chiefly for these reasons:

1. While all professors should be equally eminent in their respective professions, such will not be the case in any actual faculty. Certain individuals may tend to stagnate, others to draw forward and in various ways differences are sure to exist.

2. As a result of these differences, either in professional or in collateral qualifications, it will result that in a real sense, certain individuals may become of more value than others, no matter on what basis 'value' may be estimated. Such differences may not improperly be made the basis of a difference in salary.

3. Due to one cause or another, the services of different members of the same grade of the teaching staff may have different market values. Certain individuals may be more or less prominently before the public, or may receive calls from other institutions, and if it is desired to retain their services, an advance in salary must be made. * * *

To summarize: if universities could be provided with ideal faculties and administered under ideal conditions, doubtless equality of salary in the same grade should and would prevail. Under actual conditions and as a practical administrative problem it does not seem likely that it ever will.

21.

I am strongly inclined to think that men bearing the same title in a given institution should receive the same salary, or what would be better, if practicable, that there should be a fixed scale of advancement by years so that new appointees could start in at the bottom of the scale and receive a regular increase of salary until a certain maximum is reached. It might be necessary to recognize services in other institutions at similar grade in adjusting the pay of men changing from one full professorship to another. To give satisfaction such a sliding scale would have to be administered uniformly and the practise of withholding the expected advance in some cases and in making it in others strictly debarred, otherwise members of a faculty would feel themselves perpetually under trial and while this might stimulate a certain feverish activity, it would not promote the efficiency of the body as a whole. I believe that the healthiest system and the one which would lead to the finest *esprit de corps* to be that of uniformity of pay; the salary in the case of each institution to be of public knowledge. A sliding scale of annual increase which could be relied upon would be satisfactory to the younger members of the staff and peculiarly advantageous to those who remain in service for a long period of years.

Such a system would do away altogether with the present very objectionable practise of trying to obtain offers from other institutions, not for the purpose of accepting the same, but to force an increase of salary in the position which one already holds. At present this is in many institutions the only successful way of improving one's condition as regards pay.

Such a system as that outlined, however, should be so arranged as not to altogether discourage migration; for, aside from mere monetary considerations, it is doubtless better for the universities and their professors to have a certain reasonable amount of change of personnel, on the one hand, and of environment, on the other.

It might possibly be found necessary to pay higher salaries in departments which come into touch with practical life, such as the chairs in engineering, architecture, medicine and law, than in the case of purely academic positions, because of the demand outside of the universities for men fitted to fill these positions, but such a distinction is unfortunate and should be avoided if possible. If a man conversant with practical affairs selects a university career he does so necessarily at a pecuniary sacrifice and it is perhaps not too much to ask him to be content with the same pay as his colleagues in other departments.

22.

As regards instructors in different schools, or perhaps even in different departments of the same school, some variation seems necessary. Experts in law, medicine or engineering, even though of no greater relative prominence in their specialties than teachers in the so-called academic departments of the university, must ordinarily be paid higher salaries than other teachers, simply because a sufficient number of first-class men can not be gotten away from the competition of active practice in these subjects for salaries that will procure good teachers in the academic departments; otherwise, indeed, a university proposes to pay the academic teachers on the basis necessary for the professional ones, which is not likely to be the case. I assume, therefore, that the practical question for discussion is whether there should be variations in the salaries paid to men of the same rank in the same school or similar departments. Ordinarily, I should think it preferable to pay the same salary to men bearing the same title and doing work similar in amount and general character. Where a university's funds are reasonably adequate to the work it is attempting to do, this seems the best way of preventing jealousies and dissatisfactions which are very natural where there are marked distinctions in salary among men of the same rank. At least this is true among professors and associate professors, though the reasons for it are not nearly so strong in the case of temporary appointees like instructors and assistant professors. A variation of salary within certain limits may here often be proper and useful in enabling the university to retain a good man whom it can not immediately promote to the higher ranks. In any case, however, I should think it proper for a university in rare instances to pay special salaries to men of extraordinary abilities. The salary of a professor is not large at best, and a university teacher with an opportunity to earn a much larger sum outside of the university might occasionally have duties that he felt obliged to discharge which necessitated a larger income. If his value to the university were very great, I should think it proper for the university to retain him by special arrangement, but such cases would be very few in number.

Institutions whose income does not enable them to employ the usual number of full professors at salaries paid for good men by the larger universities must necessarily choose between having all the men in a department below the first rank, or trying to have at least one first-rate man in each department who shall be paid a substantially higher salary than the others. I should think it better for an institution to pursue the latter policy; but it would be difficult for such an institution to retain its better men, even of the second grade, if it never advanced any of them to the rank of professor except the heads of departments. The title of professor is looked upon by a considerable number of excellent men as a substantial promotion even though unaccompanied by much increase in salary; and I should think it proper for a university with a moderate income to act upon this known fact of human nature. I am pretty sure it would be able to retain better teachers, in the long run, this way, and that is the principal object to be gained by salary rules. Perhaps even such a variation as that just suggested could be made the subject of regulation by constituting heads of departments a higher class than full professors.

23.

As there are many different degrees of worth, or value to the university, among the men of its faculty, and as but a limited number of these degrees is represented in the usual scale of titles composing the faculty organization, I should say, decidedly, that the same salary should not necessarily be paid to men bearing the same title. The differences in salary should correspond as nearly as possible and expedient to the differences in degree of worth of the men to the university. The factors determining this worth are several and various. Some of them appeal primarily to our ideal professional qualifications; others appeal more to our recognition of the practical necessities of university administration. Theoretically, the ideal qualifications should be the preferred and most rewarded ones: actually both categories of qualifications must be taken into account. But there is no scale of degrees of worth determined either on the basis of ideal qualifications alone, practical necessities alone or (as is inevitably the real basis) of a combination of these two, that does not include in its series more degrees of gradations of importance than are represented by the conventional scale of faculty titles or positions. These degrees should be recognized and rewarded by differences in salary, even though they can not be by differences in title.

24.

I believe there should be a minimum standard of salary for a given title, but that the maximum should be varied to suit the class of men engaged in such work. *Many valuable men deserve an advance in salary before they deserve promotion in rank;* in fact, some exceedingly helpful men may never deserve a high rank as to title, but become increasingly useful as members of the teaching faculty. It is my feeling that a professorship should not be awarded simply and solely because of scholarly attainments or ability as an investigator; the title has a greater content than these qualifications imply.

Under the present economic condition the salaries now being offered to college workers are so meager as to offer no incentive to young men of ability to enter the profession. Of course, many young men of ability are entering it, but they are doing so with no hope of any financial reward and many of them are not conscious of the difficulties that await them. It is easy to say that the best men are willing to make sacrifices, but it is not so easy to see that the sacrifices which they are called upon to make are many times serious detriments to their advancement. For example, comparatively few men in college work, relying wholly on their salaries, can afford to hire a stenographer or reader to do certain amounts of detailed and more or less mechanical work. This is not as it should be. Much more time could be given to investigation, and they would have more desire to investigate and devote their energies to essentials, if they could be relieved of the purely mechanical work. At present, as I say, relying wholly upon their salaries for support, this is practically impossible.

In conclusion: people of the country are, in general, of the opinion that college professors are poorly paid, hence there would be no serious objection on the part of the public to a change for the better.

25.

The titles do not represent ranks of men in military alignment, but a group of runners spaced out yonder on the track. Our hope is to space them out more widely still by evoking from each best one his utmost effort and speed. A prize—of some sort—is what human nature demands in all such cases. 'To him that hath shall be given' is never truer nor more just than here. Now the title itself is a prize. But the title of itself affords no further inducement to him who has already won it; and for those of the highest academic rank no further prize of that sort is possible. But this last is precisely the group that most needs such stimulus.

To rely upon the desire of fame alone to furnish the needed spur seems to me not quite all we may rightly do. Fame depends upon too many accidents, and generally comes too late to avail the individual for further effort. It seems, moreover, not quite fair that the world generally should be left to pay the debts of the university for exceptional service rendered first of all to the university itself. The evils which are feared in this connection—bitterness, jealousy and the charge of favoritism—are already here, and will inevitably attend any attempt to recognize excellence. The way to deal with them is neither to efface distinctions already established, nor to refuse to carry them further as may be needed; but rather to make sure that every such recognition of excellence shall carry with it the conviction of its essential justice and desert. I see, therefore, no reason why the principle of special reward for special service, operative everywhere else, should be made inoperative within the charmed field of a certain academic rank. To make it so suggests at once the methods of the 'union,' and its results—loss of incentive towards excellence because mediocrity will answer just as well!

And I make no question of recognition of a pecuniary sort. Added pecuniary resource here is not so much *pay* as it is a necessary condition of further and continued effort; relieving the scholar's mind from carking cares, and his body from profitless fatigue, and setting free his energy to do its proper work. Nor should it be necessary for a man of proved value and ability to work for a position elsewhere, in order to come into the enjoyment of what he has deserved at home.

A graded use of salary during the two or three years of one's novitiate in the professorship—if the man has it in him to rise at all—seems almost the only proper thing—and works well in practise. The associate professorship seems not so distinctly to need an augmentation, since it should lead betimes to the next stage. But in the last stage the principle of recognition of exceptional quality, I think, should have large sway.

26.

In order to answer your question, two things must first be clear, viz.: (1) The basis on which salaries are fixed; (2) what is implied by the same title?

As to the first I may express the fairly obvious belief that salaries should be the pecuniary compensation for services rendered. In practise, however, the salary question is frequently complicated by the introduction of other matter. In fixing salaries the only condition should be 'value received.'

Assuming this, if the same title truly indicates equal usefulness among the men who hold it, I believe that equal salary, equal compensation, is both logical and just. But unless men equal in rank are also equal in usefulness, I do not see that equal compensation is justified.

I may therefore state my opinion regarding your question in these terms:

1st. Salaries should be compensation for services rendered;

2d. They should be fixed only according to services rendered;

3d. The salary of an individual should therefore be determined solely by his usefulness;

4th. Unequal usefulness of men in the same or in any rank should necessarily imply unequal salary.

27.

Equality of salaries would presuppose at least approximate equality in scholarship; but there are very few universities in this country that have reached that stage. In other cases a uniform salary scale would seriously hamper the appointing power in its efforts to secure good men, especially in universities that are at a disadvantage in consequence of their geographical location or for any other reason; unless indeed the salaries be placed at the maximum figure obtaining anywhere, and that is obviously impracticable.

28.

In general, I think that it is not just to pay the same salary to all bearing the same title. It seems to me that there are at least four reasonable grounds for such discrimination:

1. Relative success in teaching;
2. Relative success in research or in otherwise contributing to knowledge;
3. Various personal qualities not easily defined, but going to make up character and culture, and manifested in the teacher's influence for good on the student;
4. Length of experience. If the teacher does not grow, he should not remain in the university. If he does develop from year to year in such qualities as those referred to above, it is right that he should receive better pay.

I most emphatically do *not* believe in salary discrimination based on a 'commercial' valuation obtained from the demand for the teacher by other institutions—a condition which already exists to too large an extent. When the laws of supply and demand govern the salaries paid our teachers the influence of our universities for good must certainly suffer. We should see such results within the faculties as now exist on the campus—an able teacher of Latin, let us say, of high character and long training, and successful as a teacher, getting perhaps $1,000 or $1,200, while a recent graduate, of doubtful character, can command $5,000 for six weeks' work as football coach.

29.

I am distinctly of the opinion that we are too careful in differentiating officers of the university according to age and title rather than to ability, activity and general usefulness.

Universities are lax in encouraging young men to leave the university when they are not fitted, and at the same time American universities seem to be equally lax and short-sighted in disposing of older men who in their prime were a great help and honor to the university. The modern trust is much more humane in these matters and, it seems to me, takes better care of its good men.

Salaries should not be paid in amounts proportionate to arbitrary gradations of title. When a man has proved his usefulness to the university, the university should be willing to pay him any reasonable salary to retain his services.

30.

A member of a university faculty is likely to put more life into his work, if from time to time he should receive some definite recognition of excellence to which he may attain, whether in teaching, research, executive work, or length of honorable and efficient service. This recognition may come to him in a variety of ways, one of which is increased remuneration. It is desirable that such encouragement should be given him more often than he could be promoted from title to title, for a complex system of titles would be unwieldy. Therefore, a minimum and maximum salary should be established for each title, a man being advanced, now within his title, now from one title to another, as he shall merit.

In reading this President Jordan made the following comment:

I may add a word of my own to Professor Stillman's discussion of this topic.

The problem is rendered more complex through the existence of the assistant professor, on whom in great part the work of the American college now falls. Historically, the assistant professor is a recent development and his position has no analogue in the universities of England or Germany.

The fact that the American universities are teaching institutions, as distinguished from those whose primary function is that of an examining board, has had a large influence in shaping our university organization.

In England and Germany, in general, there is an established standard of excellence of erudition or of culture to which the student aspires. In framing this standard, no consideration is paid to the powers or the tastes of the individual standard. It is a standard set by society or by academic tradition, and only in late years has the number of such ideals or goals of effort been multiplied.

In Europe generally, the professor teaches what he pleases, but the student uses what he teaches only as an aid to a predetermined end. The teachers of minor grade find their opportunity when the professor fails to make his work useful as a preparation for examinations. If a Privatdocent can make his work attractive and practical, the students will pay for it. Otherwise he may starve. The university has no responsibility for him, no interest in his fate. Neither does the university feel any obligation that the subjects demanded in examination shall be well taught to each individual candidate.

This is especially true of the older universities of England, and to this day the chief university function which is unquestioned is that of examining for degrees. One and all, they are primarily examining and not teaching universities.

In an examining institution, fees are charged. These fees mostly go to the professor, and very unequally, but that is his own concern. If he is interested in them, he should choose a remunerative field. The professor needs only assistants of an inferior order. These he may pay himself, and their status does not concern the university. If professor and assistants fail to cover the ground, the private tutor covers the rest, and for him again the university has no responsibility.

In a teaching university, every student must be reached. The classes taught must fairly represent the subject and the numbers in each class must not be greater than the teachers can properly handle.

In the small college under the old régime, this work was divided among a group of professors. The elective system demands many more teachers and better ones, so far as class-room work is concerned, than the English system. It is, in fact, the element of choice, whether between fixed courses, or between courses, which is responsible for the great extension of the American college system, which is now at its height.

Needing many more teachers, without the means of making them all professors, and with the opportunity of trying them out before promotion, has called into being the great army of assistant professors and others of intermediate grade, who do most of the actual work with students in the American colleges and universities to-day.

It is manifest that no system of automatic promotion by which each of these can ever be assured of a professorship in his own institution, is possible. There will never be professorships enough to go around, and even the best men must often look for promotion elsewhere. Besides this, only a small percentage of these men show that combination of personality, character, scholarship, productiveness and force which should make them worthy of first-class professorships anywhere. In the promotion of these men, the interests of the university or college as a teaching body, in other words, the interests of the students, constitute almost the sole consideration.

It is a matter of wise administration to allow a reasonable minimum in each grade, enough to enable a man to live decently. It is well to make a small automatic addition to this from year to year. It is well that this addition should cease when further promotion is not in the university's own interest. It is almost as injurious to overpay a mediocre man as to give a good man too little. The only justification for either is found in the limitations of financial ability and in the absence of means for exact valuation of the achievements and the possibilities of the various instructors. The rapid promotion of exceptional men is, under our system, a necessity. Equal pay under equal conditions considers the position, not the man, as the unit, and it is only possible under static conditions. Applied to the American university of to-day it would leave to the institution only the dregs of the faculty, unless its equal pay was held level with the maximum in other institutions. Except in two or three of our strongest universities, that course is at present impossible.

Because the university has assumed responsibility for all the necessary teaching, and this with the exaction of low fees, and in some parts of the country none at all, the assistant professor is part of the system as much as the professor. At the same time, the assistant professorship has risen through necessity, not through the voluntary choice of university authorities. We are not certain what he ought to be paid, how rapidly he should grow, or what should be his status in academic matters. These matters are mostly determined for us by necessity. We have not yet reached agreement as to whether he should have an equal voice or any voice in academic matters. University legislation usually tends to give him a nearly equal voice, regarding the academic executive as merely first among equals. University custom tends to hold the executive responsible for his associates, after the fashion of business corporations. There is justice in both points of view, and it is well for the universities that the two tendencies should continue to strive with each other. But the final outcome will be that the president of the university will be the executive representative or spokesman, not the ruler of the faculty, and the department head will stand in similar relations to his fellows. Meanwhile the title is an academic honor, the salary a practical means to an end, and so long as our universities are in process of formation, the two will not bear any automatic or static relation to each other.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

MEETING OF SECTION G (BOTANY)

Section G held three independent sessions for reading of papers, December 28 and 31, 1906, in the rooms of the botanical department, Schermerhorn Hall, Columbia University. The attendance ranged from about thirty at the last session, when the Botanical Society was holding a simultaneous meeting, to about one hundred and twenty-five. On December 28 a joint session with Section F (Zoology) was held at Teachers College, at which over three hundred persons were present. Owing to the absence, on account of illness, of the retiring vice-president, Dr. Erwin F. Smith, the hour assigned for his address was used for other papers.

During the past year there has been effected a union of three affiliated societies, viz., the Botanical Society of America, the American Mycological Society and the Society for Plant Morphology and Physiology, the combination bearing the name of the oldest and first mentioned of the three. This has simplified somewhat the relations of other botanical interests with Section G. The new Botanical Society held two sessions in which members of the section were largely present, the attendance then being over one hundred; one of these sessions was held at the New York Botanical Garden, Bronx Park, after which all the visiting botanists were entertained at luncheon by the garden. The society held two sessions on December 31, simultaneously with Section G.

Forty titles were submitted for the program of Section G, from which about thirty papers were actually presented. The first six of the following were read before the joint session with Section F.

Elementary Species and Hybrids of Bursa: George H. Shull, Station for Experimental Evolution, Cold Spring Harbor, New York. (To be published in Science.)

Mendel's Law as a Tracer of Lost Parents —I. The American Carnation: J. B. Norton, U. S. Department of Agriculture.

In this paper it will be shown that the common greenhouse carnation is a hybrid type whose two parents are a single form and a very double form. These parental types have been extracted by ordinary breeding methods and recombined to produce a uniform hybrid first generation agreeing with the standard commercial types. This is the first experiment, so far as the author knows, that shows a commercial application of Mendel's law of heredity.

Preliminary Note on Pollen Development in Œnothera lata De Vries and its Hybrids: R. R. Gates, University of Chicago.

Œnothera lata is one of the mutants which does not mature its pollen, and hence must be pollinated from another species, producing a hybrid in the next generation. The plants studied were from a cross between *O. lata* and *O. Lamarckiana,* which is a Mendelian hybrid, showing in the next generation, according to De Vries, an average of 15 per cent. *O. lata* plants, the remainder being *O. Lamarckiana,* the pollen parent.

It is found that pollen development in *O. lata* may proceed to the formation of tetrads. The later stages are very irregular and result in complete degeneration of the pollen cells, usually long before maturity. This degeneration may begin in some cases as early as the synapsis stage. Irregularities in the distribution of the chromosomes in the reduction divisions give rise to small extra nuclei in the pollen tetrads, and other conditions similar to those found by Juel in *Hemerocallis fulva* and *Syringa Rothonagensis* (a hybrid), by Tischler in hybrids of *Ribes,* by Guyer in hybrid pigeons, and others. The prevalence of the condition, especially among sterile hybrids, suggests that it may prove to be a distinguishing characteristic of certain classes of hybrids.

The sporophyte number of chromosomes in *O. lata,* as determined in the prophase of the heterotypic mitosis in the pollen mother cell, is 14; while the sporophyte count for the *O. Lamarckiana* side of the cross is at least 20. The conclusion from this is that the pure *O. Lamarckiana* itself must have over 20 chromosomes. It is believed that differences among the chromosomes of *O. Lamarckiana* and its mutants may furnish a cytological basis for discontinuous variation, and hence a count of the chromosomes in *O. Lamarckiana* and other mutants is to be made at once to settle this important point.

In both sides of this cross the heterotypic mitosis in the pollen mother cell shows one, or more commonly two, ring-shaped chromosomes with a peculiar history. They may be designated heterochromosomes. In *O. lata* their origin has been traced. They arise during synapsis, apparently by the cutting off of a loop of the spireme, for in earlier stages of synapsis the whole spireme is continuous. One or two of these bodies may be formed as large rings or closed loops some time before the rest of the spireme breaks into chromosomes. Later they are found, much condensed in size, on the spindle; but in metaphase they have generally wandered towards the poles or out into the cytoplasm, where they probably disintegrate. Their possible significance in connection with the phenomena of mutation will not be discussed now. The full paper will appear shortly.

On the Behavior of the Seedlings of Certain Hybrids of Viola: Ezra Brainerd, Middlebury College, Vermont.

Many violet hybrids are found in the second generation to revert in some characters to one of the parent species, and in other characters to the other parent species. Illustrations are given of this in four particulars: (1) in leaf-outline; (2) in pubescence; (3) in size; (4) in color of capsule and of seeds.

The tendency of certain individuals to recover from impairment of fertility is shown.

Illustrations are given of the way in which species may arise by the attainment in the more marked hybrid forms of fertility and stability in reproduction.

Origination of Species by Hybrids among Wild Plants: D. T. MacDougal, Carnegie Institution. (To be published in the *Botanical Gazette.*)

An Instance of Natural Hybridization: W. W. Rowlee, Cornell University.

In 1896, the writer in conjunction with Dr. Wiegand, published an article in the *Bulletin of the Torrey Botanical Club,* describing as hybrids certain willows that grow in a small peat bog near Ithaca.

The writer was impressed at the time

(and the impression has grown since) with the insecurity of diagnoses of wild hybrids. Inferential evidence must be accepted that the plant is a hybrid in the first place and in the second place the parentage must to a considerable extent be assumed.

The case which came under our observation was perhaps clearer than some others because one of the supposed parents, *Salix candida,* was confined to this particular locality. No other station is known for the plant within a radius of forty miles. The other parent might have been any one of several willows growing near by. The morphological characters of the hybrid pointed strongly toward *S. cordata* as the other parent, and it was so decided in the published paper.

After the publication we undertook to hybridize artificially *S. cordata* and *S. candida,* with the result that duplicates of our wild hybrids were produced. It became at once interesting to inquire into the conditions under which the wild hybrids were produced. As stated above, one of the parents was local. The area covered by the bog is less than an acre. It is completely surrounded with hard ground that was covered in its primeval condition with a heavy growth of trees. After the country was cleared up the owner of this swamp (before 1870) made an attempt to convert it into agricultural land by drainage and tillage, and succeeded to the extent of lowering the water level so that he could clean the surface and in a dry season plow it. The ground proved to be full of copious springs, and in spite of the complete system of ditching, it remained too wet to be manageable. During the period of cultivation *Salix candida* was nearly exterminated. It, however, maintained itself sparsely along the ditches. When the farmer abandoned his attempt at reclamation *Salix cordata,* which grew in abundance in the vicinity, invaded the swamp. A fertile and unoccupied seed-bed was afforded, and plants that found it congenial took possession. Of the shrubs, *Salix candida* and its hybrids apparently succeeded best, and the result is a plantation of very interesting willows, a complete series of which have been taken. It seems highly improbable that these forms appeared in the swamp prior to 1880, as Professor Dudley and other students had studied the flora of this particular place carefully and repeatedly.

The conditions existing in this peat bog have suggested to the writer that the clearing of the country may have given similar opportunities to other species. The numerous forms of *Cratægus* and other types recently described, readily adapting themselves to open conditions, may have had their rise under conditions similar to those in which our hybrids of *Salix candida* developed.

A Natural System of the Discomycetes: F. E. CLEMENTS, University of Nebraska.

Spore Forms of Spegazzinia ornata Saccardo: ERNST A. BESSEY, Subtropical Laboratory, Miami, Florida.

Two spore forms are known for this fungus, one long-stalked with spiny processes and one short-stalked and smooth. According to Saccardo the latter are borne on the spines of the former. Cultures have demonstrated that the forms are independent of one another and arise directly from the fungus mycelium.

Accelerated Blossoming due to Defoliation by Storm: ERNST A. BESSEY.

On October 18, Miami, Florida, was visited by a hurricane of great intensity, lasting about nine hours. The wind attained a velocity of probably seventy-five to eighty miles an hour and possibly still higher for certain gusts. To a considerable extent both native and exotic trees were defoliated, the loss of leaves varying from that of those at the extremities of the branches to the total defoliation of the tree, often accompanied by loss of branches. Among the trees thus affected were live-oak (*Quercus virginiana*) and mulberry (*Morus rubra*). The former does not shed its leaves normally until the new foliage is fully developed in the spring, thus being evergreen, but the latter is normally without leaves in December and January.

About three to four weeks after the storm, defoliated trees of both species pushed out into flower, new foliage buds beginning to unfold also. The normal time of blossoming is about February 1, so that the time of blossoming was accelerated eight to ten weeks.

It is interesting in this connection to recall the newspaper reports of the Mobile hurricane according to which peaches and other fruit-trees came out into bloom two or three weeks after that storm.

This phenomenon of blooming being hastened by defoliation is not rare. In Paris, in August, 1904, the writer saw horse-chestnut trees that had been defoliated by some insect, pushing out into bloom again. It is by no means unknown in America. Following the freeze at Miami, at Christmas, a pomelo tree was observed which had lost the leaves on one of its large lower branches. This branch was in full flower three weeks after the freeze.

The explanation of the appearance of the blossoms when the trees start to reform their leaves lies in the fact that the flower-anlagen are laid down in the summer so that when the leaves expand the flowers also appear. The fact that the root and trunk cells had not reached the dormant condition is probably the explanation for the fact that the trees defoliated in October produced new leaves and flowers so soon. Possibly if the weather had remained cold for several weeks after the storm the plants might have assumed their winter dormancy.

An Outbreak of the European Currant Rust, Cronartium ribicola Dietr.: F. C. Stewart, Geneva, New York.

Cronartium ribicola is an heterœcious rust having its uredo and teleuto stages on the leaves of various species of *Ribes* and *its æcidium stage* (= *Peridermium strobi* Kleb.) on *Pinus* spp., particularly *P. strobus*. In Europe, where it has been known for fifty years, it is widely distributed and in some regions causes serious injury to *Pinus strobus*. With one possible exception, it has never been collected in America prior to 1906. Accordingly, it is interesting to note that in September, 1906, the writer found it abundant in a currant plantation on the grounds of the New York Agricultural Experiment Station at Geneva, N. Y. In this plantation three species (*Ribes nigrum, R. rubrum* and *R. aureum*) were represented by fifty-four varieties, forty-eight of which were affected. In many cases every leaf on the bush was affected. *R. nigrum* suffered most and *R. aureum* least. Near-by specimens of *R. grossularia*, also, were slightly attacked. The source of infection is unknown. The only specimens of *Pinus strobus* nearby are two apparently healthy trees planted eight years ago 112 meters west of the infested currant plantation. All *Ribes* plants on the station grounds have been destroyed in an attempt to stamp out the disease.

The Origin of the Hymenium in Some Geoglossaceæ: E. J. Durand, Cornell University.

The hymenium of the Geoglossaceæ has been supposed to be free or exposed from the first. Schroeter made this character the distinguishing character of the Helvellineæ. Dietrich called attention to the fact that in *Leotia lubrica* and *Mitrula phal-*

loides the hymenium was at first covered by a delicate membrane or veil, which it breaks through. The writer's observations on several species confirm those of Dietrich. It is probable that in all members of the group the hymenium is at first covered.

Notes on the Embryology of Rhizophora Mangle: Mel. T. Cook, Santiago de las Vegas, Cuba.

Presents many difficulties in technique, owing to the presence of tannin, etc. Only one of the four ovules develops. Integuments develop early and increase the difficulties. Embryo-sac apparently follows the normal course. Embryo has a long filamentous suspensor, the synergid end of which degenerates very early, but the opposite end persists very late. Endosperm very abundant and the outer cells grade almost imperceptibly into the nucellus cells. The cotyledons develop very early at the expense of the endosperm cells and have the appearance of being very much cramped. After the development of the cotyledons is well advanced the root tip begins to grow more rapidly. At the same time the surface cells of the cotyledons become very much modified and very protoplasmic, evidently for absorption. Cells through the entire embryo show evidence of great activity, the vascular system becomes more prominent and peculiar ingrowths from the surface of the cotyledons connect with it. Endosperm almost entirely disappears and the embryo is fed by the mother plant.

The Embryology of Rhytidophyllum Crenulatum and R. Tomentosum: Mel. T. Cook.

Of interest because the family is tropical and subtropical and unworked morphologically. Ovule anatropous. Archesporium a single subepidermal cell which usually does not divide but elongates and produces the normal eight-nucleate sac. Synergids small and staining deeply. Antipodals small and disintegrating early. The polar nuclei unite near the antipodals. Micropyle and pollen tube conspicuous. Entrance of pollen tube obliterates the synergids. Endosperm undergoes primary division before the first division of the proembryo, and the two daughter nuclei are separated by a cross wall which soon disappears. Both nuclei divide rapidly. Micropylar endosperm disintegrates very early. Embryo at first filamentous, followed by a division of the apical cell. Suspensor elongates and appears to function as a haustorium, then disintegrates. Differentiation of tissues does not follow the exact order of *Capsella bursa-pastoris* and other dicotyledonous plants.

Radioactivity a Factor in Plant Environment: C. Stuart Gager, New York Botanical Garden.

The hitherto recognized factors of plant environment may be classified as molar (including living organisms), molecular and undulatory. Radioactivity is an expression of atomic disintegration, and is accompanied by a relatively enormous release of energy. At least four different factors may be included under the term radioactivity, for the breaking down of the atom of a radioactive substance is accompanied by (1) a stream of negatively charged ions or 'corpuscles,' each about one thousandth the size of a hydrogen atom, and moving with about 95 per cent. the velocity of light. Streams of corpuscles constitute the *alpha* rays; (2) a stream of positively charged ions, of nearly twice the size of a hydrogen atom, and moving more slowly than the corpuscles. Streams of positive ions constitute the *beta* rays; (3) an electro-magnetic pulse, analogous to the X-ray, and caused by the starting or stopping of ions. These rays, given off from radioactive substances, are termed *gamma* rays; (4) the emanation, an inert gas of the argon family, which is radioactive, and whose atoms are, therefore, undergoing a disintegration.

Recent researches lead to the conclusion that the phenomenon of radioactivity is not confined to the so-called radioactive elements, radium, uranium, thorium and others, but is a general property of matter. Not only are the heavier metals, such as lead, zinc, silver, but various other substances, namely aluminum, copper, tin-foil, air from soil, spray at the foot of waterfalls, mud, tap water, petroleum, freshly fallen rain and snow, metal surfaces on which light waves of short wave length impinge, and flames, have all been found to give off a penetrating radiation. It seems probable that radioactivity is a general property of matter. Results already obtained demonstrate conclusively that it is a factor in the normal environment of living things, and its effect on the life processes of plants becomes, therefore, a matter of considerable interest and importance.

Some Effects of Radioactivity on Plants: C. Stuart Gager, New York Botanical Garden.

In ascertaining the effects of radioactivity on plants, it has not been possible satisfactorily to screen out the various kinds of rays, and the emanation, so as to study separately the effects peculiar to each. The results obtained, therefore, must be attributed, almost without exception, to the sum total of all three kinds of rays involved in the radioactivity of radioactive substances. The present paper concerns chiefly the results obtained with radium bromide, of various strengths of radioactivity, used either in sealed glass tubes or in the form of Lieber's radium coating on rods and cylinders.

Experiments lead to the conclusion that the rays of radium act as a stimulus to the physiological processes of plants, accelerating, retarding, or inhibiting, either germination, growth, respiration, fermentation, cell division, starch formation, sensitiveness to gravity, according to the strength of the radium salt employed, the duration and distance of exposure, the intervention of screens, the nature of the tissue and, possibly, the species of the plant.

When the pollen or the ovary is exposed before pollination, or the ovary after fertilization, the resulting seeds produce plants profoundly different in the first generation from that normal for the species. If these modifications shall prove heritable in subsequent generations the ability of radium rays to induce mutative changes will have been demonstrated.

While there is seemingly a very crude analogy between radioactivity (atomic disintegration) and the molecular disintegration involved in destructive metabolism, there is nothing in the former that may be rightly compared, in any way, to constructive metabolic processes. The results of continuous experiments, covering a period of over two years, do not give the slightest support to any theory that radioactivity is, in any real sense, comparable to metabolism, or that the rays of radioactive substances are capable, independently, of elevating inorganic compounds into the condition of living organisms.

The Pathology of the Rice Plant: Haven Metcalf, U. S. Department of Agriculture.

The literature on rice diseases is scanty, and mostly in the Italian, Japanese, Dutch, Russian and Portuguese languages. Fifteen diseases have been described on rice; further research may be expected to demonstrate the identity of several of these, now supposed to be distinct. Eight rice diseases occur in America, of which six were described by the writer in Bulletin 121 of the South Carolina Experiment Station. Of these, *blast*, the most serious, is caused by a fungus of the genus *Piricularia*, as the writer has demonstrated by repeated successful inoculations from pure cultures. It is not impossible that further researches will show this disease to be a form of the Italian *Brusone*. Two diseases are increasingly prevalent, and may have been recently introduced into the country: a 'smut,' caused by *Ustilaginoidea virens* (Che.) Tak.; and a stem rot under water, due to *Sclerotium Oryzæ* Catt.

The Production of Toxic Soil Conditions by the Roots of Plants: HOWARD S. REED, Bureau of Soils, U. S. Department of Agriculture.

The author reported the results of experiments demonstrating the excretion of deleterious substances by roots. Roots growing in non-nutrient agar rendered it unfit for further growth. The excretions from wheat were decidedly toxic to a second crop of wheat. The excretions from corn or cowpeas were scarcely, if at all, toxic to wheat. The excretions from oats were quite toxic to wheat but less toxic than those of the wheat itself. Apparently the excretions from the roots of a given plant, or of its near relatives, are more toxic to that species than the excretions from plants belonging to more distantly related species.

The Rôle of Certain Elements in the Physiology of the Plant Cell: HOWARD S. REED.

The author reported the results of experiments made at the University of Missouri. The vegetative bodies of a number of lower plants were cultivated in solutions lacking one of the essential elements. Most of the elements have more or less specific functions to perform, and, within narrow limits, there is no substitution possible. The view was held that the essential elements, in addition to building up tissues, serve as sources of energy to the cell.

Some Mutual Effects of Tree-roots and Grasses on Soils: CHAS. A. JENSEN, Bureau of Soils, U. S. Department of Agriculture.

Seedling trees of maple, dogwood, cherry, pine and tulip were planted in paraffined wire pots and the pots planted to wheat. The pots were of such size as to make a close physical relation of the roots of the two kinds of plants necessary. The pots containing the trees in every instance gave less yields of wheat than the controls, especially during summer months when the trees were physiologically active. Towards the autumn, when the trees were entering upon their seasonal rest, the wheat yields increased, sometimes slightly exceeding those of the controls. Different species of trees also had different effects on the wheat yields. As the experiment was carried on under control of external conditions, it is believed that the bad effect of the tree seedlings on the wheat growth is due to toxic substances excreted by the tree roots.

The Botanical History, Classification, and some Uses of Sorghum: CARLETON R. BALL, U. S. Department of Agriculture.

Andropogon sorghum (L.) Brot. comprises a very large number of domesticated forms. None of them is known to exist in the wild state. The cultivation of sorghum dates from remote antiquity. India and the eastern and central portions of Africa are to-day the two great sorghum centers, both in rich abundance of forms and in the economic use of them. Facts point to the independent origin of numerous cultivated forms in India and in Africa. *A. halepensis*, supposed to be the parent of all cultivated sorghums, is widely distributed over most of these two regions. Introduction into Europe took place in the early days of the Christian era. The practise of all early botanists was to regard all forms in cultivation as distinct species. Between thirty and forty specific names have been applied to the plant and more than one hundred trinomials in recent years. Identity of the important species and the natural method of classifying and naming forms of a cultivated species. Use of sorghum for human food, forage, syrup, sugar, liquor, building material, firewood, etc.

Periodicity of the Sexual Cells of Dictyota dichotoma: W. D. Hoyt, Johns Hopkins University.

On the coasts of Wales and England *Dictyota dichotoma,* according to Mr. J. Lloyd Williams, bears its sexual cells in regular fortnightly crops. The times at which these crops are produced bear a definite relation to the tides, and the observed behavior is best explained by regarding the increased illumination obtained during the low water of spring tides as the factor which determines the time of fruiting.

Observations made at the laboratory of the Bureau of Fisheries, at Beaufort, N. C., during the past summer show that at this place also *Dictyota dichotoma* produces its sexual cells in regular crops at periods bearing a definite relation to the tides. These crops, however, are borne at monthly, instead of fortnightly, intervals, the time of their production is not influenced by differences in the height of different spring tides, and it seems that light is not the factor that determines the time of fruiting.

Evidences of Sexual Reproduction in the Slime-molds: Edgar W. Olive, University of Wisconsin. (Read by C. A. King.)

Some recent work on the exosporous myxomycete, *Ceratiomyxa,* has shown that a simple form of sexuality exists in this form. Toward the close of the more or less quiescent cleavage stage, the nuclei of the plasmodium fuse in pairs, so that the uninucleate protospores which result from cleavage each contain a large fusion nucleus. The chromatin of this large nucleus soon appears to become shrunken, or rounded up in a dense ball, at one side of the large nuclear cavity, thus resembling strikingly the condition which has been termed synapsis. That this condition is really a true synapsis, and not an artifact, is suggested by the two rapidly recurring nuclear divisions which, after a short period of rest, follow. These facts appear to indicate that a reduction phenomenon takes place at this time, similar to that which takes place in the spore mother-cells of higher plants. As a result of the double division, each mature spore of the final fructification comes to contain four nuclei.

We thus note that in the life history of this form, we have presented the three morphological stages, which, as pointed out by Blackman and others, occur in connection with the complete sexual cycle, viz., (1) *cell fusions* (when the many myxamœbæ unite to form the plasmodium); (2) *nuclear fusions* (in pairs, near the close of the development of the fructification); followed immediately by the third stage, *chromatin fusion* (associated with synapsis and the subsequent reduction division). As has been pointed out, more or less wide gaps may separate these successive stages in different plants. In *Ceratiomyxa,* we obviously have a condition comparable to that in the rusts, since cell fusion is far separated from the final nuclear and chromatin fusions.

As Blackman has further pointed out, the late nuclear fusion in the rusts has to do with the reduction phenomema which immediately follow, and he holds the opinion that the stimulus to development—fertilization proper—is imparted by the earlier cell fusions. In *Ceratiomyxa,* the conditions appear essentially similar.

Although the numerous nuclei in the plasmodium do not apparently maintain the paired relation seen in the rusts, at least until just before spore formation, yet notwithstanding this fact, the plasmodium may be regarded as similar to the binucleate condition in the rusts, and therefore as a sporophyte stage. The 4-nucleate spores, together with the several generations of swarm-spores and myxamœbæ, would, according to this view, constitute the gametophyte development.

Localization of Plants in the Finger Lake Region and the Adjacent Ontario Lowlands of Central New York: W. W. ROWLEE, Cornell University.

The author gives a description of the physiographic features of central New York, pointing out that Onondaga, Oneida and numerous small lakes on the Ontario lowlands are strikingly different from those of the lakes of the Finger Lake Region proper.

The subject discussed in the paper deals principally with localized or rare plants in these two regions. A list of species is taken up and discussed under the three headings: (*a*) plants of recent introduction, (*b*) plants requiring peculiar conditions, (*c*) plants with no apparent cause for limited range. The paper is devoted particularly to plants of the last category, and it is pointed out that one and the same species is scarcely ever localized in both regions. Either a localized species of the uplands does not occur in the lowlands, and *vice versa;* or a species localized in the highlands will be relatively abundant in the lowlands, and *vice versa.* Again it is noted that the localized species of the highland region occur with few exceptions in wooded uplands, while those of the lowlands are confined mainly to the lakes and watercourses and their immediate vicinity.

The author concludes that in the Ontario lowland region the waterways have directly or indirectly been responsible for the introduction of the localized species, and the fact that almost all of them are at their extreme northern range indicates that they are an element of the south Atlantic coast flora at its extreme limit of endurance.

Agrostological Field Notes for 1906: A. S. HITCHCOCK, U. S. Department of Agriculture.

Outlines of two field trips: (1) Southern States, March–April, (2) Rocky Mountains, July–August. Methods of study of grasses in the field. Kind of notes necessary to take in studying the variation of a species. Results as applied especially in the genera *Panicum* and *Poa.*

By visiting type localities many doubtful species of *Panicum* of the southern states were satisfactorily worked out.

Classification of the Paniceæ: Mrs. AGNES CHASE, United States Department of Agriculture. (Read by title.)

The Sub-aerial Absorption of Water; a Function of the Ligule and Stipulaceous Tissue of the Grasses: F. L. STEWART, Murrysville, Pa.

This paper gives the results of experiments extending over a series of years; disclosing the existence of an adjustment of the external to the internal structure of the grasses whereby the moisture of the atmosphere by a system of condensation, conduction, temporary storage and absorption enters into the circulation of those plants, contributing to their nutrition and thus supplementing the supply derived from the roots.

'G' Trees: S. M. TRACY, Biloxi, Miss.

Describing a peculiar form of *Pinus Tæda L.* which occurs frequently in southern Mississippi, though very rare in other sections. Paper accompanied by photographs.

Parasitism of Buckleya distichophylla (Nutt.) Torr: Samuel M. Bain, University of Tennessee.

This shrub, known from only a few localities in the Tennessee and North Carolina mountains, is parasitic on the roots of other trees. Like its Japanese congener, *B. Quadriala,* it has several hosts, and was found growing on *Fagus americana, Pinus virginiana* and *Tsuga canadensis,* and doubtless occurs on other species. Specimens growing on *Tsuga* appeared more robust than those growing on other hosts.

The Plant Disease Survey of the United States: W. A. Orton, Bureau of Plant Industry, U. S. Department of Agriculture.

The department has for many years collected information and statistics in regard to the distribution and prevalence of plant diseases in the United States. Beginning in 1899, an annual summary has been published in the appendix to the year book. In 1905 the scope of the survey was extended by the adoption of a plan for securing the active assistance of plant pathologists in some of the experiment stations, who were made collaborators in the department. It is expected that the work will be further extended as circumstances permit.

The object of the survey is to gather facts that will serve as a basis for the study of the science of the geographical distribution of plant diseases. It is desired, first, to learn and record the distribution of plant diseases throughout the country; second, to record the varying prevalence of these diseases from year to year, and the losses caused by them; third, to study the conditions governing the development and spread of plant diseases, including the relation of weather conditions, association of crops, the invasion of the host by its parasite and the natural resistance of varieties; fourth, to bring together a collection of specimens representing the distribution of plant diseases in the United States, which will assist in settling questions that may arise in the future respecting these matters; fifth, to summarize the additions to our knowledge of plant pathology, and the progress made in the treatment of plant diseases, and to make all this information available to workers everywhere through the files and collections open for reference in Washington, the publication of an annual summary in the year book, and through more complete reports in bulletin form to be published at intervals of a few years.

Such a project can be carried out only by a national organization like the Department of Agriculture. The underlying purpose in carrying on this work is to assist the collaborators by bringing them into closer touch with farmers and others in their states, as well as with the department, and to aid them in the investigations of their special problems.

There is a growing interest in the geographical distribution of plants and their relation to each other and to their environments; it is hoped that similar interest can be aroused in studies of the distribution of plant diseases and the important scientific and economic problems growing out of this line of work. The cooperation of many observers is required for the successful accomplishment of such a survey. It is the purpose of this paper to request the aid of all interested persons.

Fasciation in the Œnotheras: Alice Adelaide Knox, Carnegie Institution.

Fasciation in the *Œnotheras* is caused by mechanical injury due to the attacks of insects in the meristems of the growing tips. In the material studied most of the injuries came from moths, species of *Mompha,* which live parasitically upon the primroses through part of their development. This relates both to ring-shaped and to simple fasciations.

Experiments upon the heredity of fasciation prove that in these forms the seed of normal plants produces as many flat stems as the seed of banded plants. In the *Œnotheras* the phenomena leading to fasciation are those of traumatism.

A Recording Evaporimeter: BURTON EDWARD LIVINGSTON, Desert Botanical Laboratory.

The author describes and exhibits a newly devised instrument for studying the daily march of the evaporation rate. It consists of a porous clay cup filled with distilled water and connected with a reservoir by a tube in which is placed an electric valve. A U-tube mercury contact is provided, by which an electric current flows to open the valve when a cubic centimeter of water has been evaporated from the porous cup and thus removed from the instrument. The same column of mercury makes a second contact and causes the valve to close when a cubic centimeter of water has flowed into the apparatus from the reservoir. An electric pen, drawing a continuous horizontal line upon a paper slip carried on a daily rotating drum, is raised slightly each time the valve is opened, and thus a short vertical line is drawn upon the record for each cubic centimeter of water evaporated. The spacing of these lines of course constitutes a record of the varying time intervals required to evaporate that volume. This instrument is available for recording the rate of loss of water, or of any liquid which can be used with a glass stop-cock, from any given container. For example, it can be used to produce a recording potometer for experiments upon plant transpiration.

The following papers were read by title owing to the absence of the authors when called for:

A Composite Lycopod Type from the Devonian: DAVID WHITE, U. S. Geological Survey.

This paper describes the remarkable fossil tree trunk illustrated in the New York Geological exhibit at the American Museum of Natural History. The fossil, which is extraordinary for its size and completeness, is very important systematically, since it combines certain features that serve to distinguish the later Lepidophytic groups. Most conspicuous of these are the *Sigillaria* form of leaf cushion in the lower part and the *Lepidodendron* form in the upper part. The fossil is one of the more highly developed representatives of an ancestral type *Archæosigillaria.*

Hybridization a Factor in Migration and Competition: E. N. TRANSEAU, Alma College, Michigan.

The increasing list of plants which form Mendelian hybrids by cross pollination with closely allied species and varieties necessitates the consideration of pollen dissemination in estimating the mobility of such species. The ease with which varieties of corn may be dispersed through pollination is a familiar example. Such hybrids are uniform in the first generation, but in the second (F_2) are resolved and produce some pure individuals of each parent type. A newly arisen variety of a widely dispersed species may by this means have its mobility greatly increased beyond that due to seed and vegetative propagation alone.

Competition in nature may involve several generations. In such cases factors other than the purely physical must be taken into account. The ability of a newly arisen variety to form Mendelian hybrids carries with it the chance of being carried over unfavorable seasons in the heterozygote condition, which may determine the failure or success of the plant in establishing itself in a given habitat. In the case of competition between a mutant and its parent form, the chances of persistence of the former may be increased or decreased through hybridization, according as they Mendelize or produce constant hybrids.

Species which hybridize readily afford a method for the quantitative study of cross pollination.

The Morphology of Lemna trisulca: Otis W. Caldwell, State Normal School, Charleston, Ill.

The sporophyte body is regarded as composed of stem, root and leaf, not as an 'undifferentiated shoot.' Floral structures, by their relative position and age indicate that there is instead of a single flower an inflorescence consisting of one carpellate and two staminate flowers all borne on a greatly reduced spadix. Carpellate and ovular structures indicate relationship with the more complex Araceæ.

The tapetal cells may grow into the loculus, become multinucleate and almost join cells from the opposite side. Spore mother cells may act in the same way. Spore mother cells and spores may disorganize at any time.

No case of indirect division in the germination of the microspore was found, the divisions observed being amitotic.

The female gametophyte rarely matures, the ovular structures disorganizing at any time. Vegetative reproduction is depended upon almost exclusively. Seed production is seldom attempted and almost completely unsuccessful.

The Structure and Wound-Behavior of the Cedar of Lebanon: Edward C. Jeffrey, Harvard University.

The Abietineæ are divisible on anatomical and reproductive characters into two distinct subgroups, the Pineæ and the Abieteæ. The former are characterized by deciduous cones with non-deciduous scales, by the resin canals of their secondary wood, and by the peripheral resin canals of the primary xylem of the root. The latter by non-deciduous cone axes with deciduous scales, by the absence of resin canals in the secondary wood and by the median resin canal of the primary xylem of the root. The cedar is distinguished among the Abieteæ by the fact that it forms, as a result of injury, resin canals in the secondary wood both in the vertical and in the horizontal planes, which are continuous with the resin canals outside the woody cylinder. Further in the primary wood of the root, although the median single resin canal, which is characteristic for the Abieteæ, is present it is often accompanied by apparently vestigial peripheral marginal canals, similar to those found in the Pineæ. These facts together with the great geological age of *Cedrus* (older than any others of the Abieteæ) tend to show that it is the most primitive of the Abieteæ and has come from the same parental stock as the Pineæ.

Tyloses in the Tracheids of Conifers: Mintin A. Chrysler, Harvard University.

Tyloses have been regarded as normally absent in the tracheids of conifers. An examination of the heart wood of the root shows that in various species of *Pinus* true tyloses occur. These are in the form of vesicular outgrowths from adjacent medullary ray cells, and are found in all stages of projection into the tracheids. They are quite distinct from the tyloses which have long been known to be present in the resin ducts, and among the conifers seem to be confined to the genus *Pinus,* where they occur in all the species yet examined.

The Homologies of the Medulla in the Filicales: J. Horace Faull, University of Toronto.

Two views are held in regard to the medulla in the stems of the Filicales, one that it is of stelar, the other that it is of extrastelar origin. These two conceptions lead to two divergent views of the stele—to the two leading stelar hypotheses.

The author of this paper holds to the view that the medulla is of extrastelar origin—excepting the parenchymatous tissue that may appear in the center of the stele at the base of seedlings in certain cases. The following facts seem to favor the correctness of this view: (1) The similarity of cortical and medullary tissues. (2) Their continuity through foliar and ramular gaps. (3) The 'intrusion' of the cortex above the origin of leaf traces near base of many filicinean seedlings. (4) The actual enclosing of a portion of the cortex to constitute a pith at the base of adventitious shoots, where a protostele passes into the siphonostelic condition—here described for the first time, and apparently a direct demonstration of the point in question. (5) It has been shown recently that the meristematic areas at the growing point do not correspond to certain exact areas in the mature portion of the stem—thereby one of the arguments in support of opposing view is removed.

Polystely in the Orchidaceæ: J. H. WHITE, University of Toronto. (By invitation.)

Van Tieghem affirmed that the cauline steles of many plants repeatedly bifurcated, resulting in the phenomenon of polystely. He further affirmed polystely does not occur in roots, and that multistelic roots of many orchids result from the concrescence of monostelic roots.

His first statement has been disproved by Leclerc du Sablon, Jeffrey, etc.

The author of this paper takes up his second affirmation and shows that true polystely does exist in orchid roots, and that concrescence is not the correct explanation of the phenomena obtaining in them.

A large number of forms have been examined and conclusions have been based on the following facts: (1) The root apex is always covered by a single root cap, and at the tip a single primary dermatogen, periblem and plerome are to be found. (2) In some orchids the lateral roots are always monostelic. (3) In others they are monostelic at their base. (4) In the most complicated cases there are fewer steles at the base than farther out from the stem bifurcation of the steles is the rule. (5) The root initials consist of a single meristematic mass for each root, no indication of the concrescence of initials. (6) The ground tissue in mature roots show no signs of concrescence. (7) The roots in the youngest generations tend to be monostelic. An increasing complexity noted in succeeding generations. Certain forms examined lend support to the view that the medulla in some cases is of extrastelar homologies.

The Araucarieæ—A 'Proto-Siphonogamic' Method of Fertilization: ROBERT BOYD THOMSON, University of Toronto.

In my study of the megaspore membrane of the gymnosperms, supernumerary nuclei in the pollen tube of the Araucarieæ are referred to and the isolated position of the subgroup indicated. The ancient geological and the widely separated geographical distribution, the large microsporangiate cones in comparison with the megasporangiate, the evident transition between sporophylls and foliage leaves (points referred to recently by Professor Seward) are indications of an interesting and probably primitive group. The morphology and anatomy of the micro- and megasporophylls indicate that they are homologous structures, functionally differentiated. The microsporangium is fern-like. The microspore multicellular, and the pollen tubes multinucleate (forty have been observed) as they grow in numbers over the sporophyll from their place of deposition, sometimes 3 cm. from the micropyle. Some penetrate the integument instead of entering the micropyle. There is no differentiation of a tube nucleus and only a single generative cell is present. For these reasons the Araucarieæ are considered by the author as proto-siphonogamic. There are indications also of a non-specialized embryogeny, intermediate in character between that of the cycads and of *Ginkgo*.

The Flowering Period of a Hybrid Opuntia: F. E. Lloyd, Desert Botanical Laboratory.

A Study of the Leaf-tip Blight of Dracæna fragrans: John L. Sheldon, West Virginia Agricultural Experiment Station. (Read by request before the Botanical Society of America.)

Tracy E. Hazen,
Secretary pro tempore

SCIENTIFIC BOOKS

Recherches expérimentales sur la Sexualité des spores chez les Mousses dioïques. Par El. et Em. Marchal. Mém. couronnés Cl. Sc. Ac. roy. Belgique, 2, I., 1906.

"A considerable number of plants are known in which a single egg gives rise by division to more than a single individual. Experimental investigations are demanded to determine if these individuals are always necessarily of the same sex." In response to this subject, thus recently proposed by the royal academy of Belgium, the Marchals have published the results of an interesting series of experiments on three diœcious mosses, *Barbula unguiculata, Bryum argenteum* and *Ceratodon purpureus.* Sowings were made from individual sporangia of these three species and, in the mixed growth resulting, both male and female plants were obtained. The conclusion thus reached that the capsule in these species contains both male and female spores was confirmed by sowings from single spores. Of five spores from a single capsule of *Bryum argenteum,* three gave rise to protonemata producing male and two to those producing female plants. Similarly, of seven single-spore sowings from a capsule of *Barbula unguiculata,* three spores produced male and four produced female plants exclusively.

Secondary protonemata, obtained by regeneration from stem, leaf or pieces of protonemata, as well as by germination of gemmæ, were of the same sex as the plants from which they were derived.

Attempts to influence the sex of protonemata by subjecting them to different external conditions were entirely unsuccessful. The effects of the following factors were tested: (1) Illumination. Cultures in strong and in weak, diffuse daylight and under red and under orange-colored glass. (2) Temperature. Three grades of temperature from 10 to 27′ C. (3) Humidity. Cultures in saturated atmosphere and in an atmosphere as dry as consistent with growth. (4) Nutrition. A clayey sandy soil watered with solutions of different chemical composition. The nutrition experiments would have been more satisfactory if a substratum had been used which alone was not capable of producing an abundant moss vegetation as was the sandy clay employed. The experiments are sufficient, however, to show that the gametophytes of the mosses tested are strictly diœcious. Writers on the mosses have claimed that in diœcious species both sexes arise from the same protonema, but heretofore no careful study of the sexual differentiation in the mosses has been undertaken. Marchal's important experiments illustrate the value and necessity of the cultural method of investigation.

These three species of mosses studied by Marchal and the liverwort *Marchantia polymorpha* investigated by the reviewer (*Bot. Gaz.,* XLII., 171, Sept., 1906) are the only diœcious bryophytes for which the sexual character of the sporophyte is known. That these forms which are diœcious in the gametophyte are all hermaphroditic in sporophyte (heterothallic and homophytic according to a more precise terminology [*Bot. Gaz., l. c.*]) does not prove that this is the universal type for diœcious Bryophytes. There is no *a priori* reason why in certain forms the sex of the whole sporophyte may not be determined in the zygote as is the case in the diœcious flowering plants and in *Mucor Mucedo*. The determination of the sex of the spores in a capsule apparently takes place at or but slightly before their formation. An attempt to suppress the formation of spores of one sex in the capsule would appear, therefore, more promising than attempts to change the sex of a spore or of a protonema already formed.

A. F. BLAKESLEE
CRYPTOGAMIC LABORATORY,
HARVARD UNIVERSITY

Le Mallatie Crittogamiche delle Piante Coltivate. DOTT. VITTORIO PEGLION, Professore die Biologia agraria nella R. Università di Bologna. 2a edizione, 1906. Casale Monferrato. 12mo., pp. viii + 323.

It is difficult for Americans to realize the great progress that is being made in Italy in the applied sciences. In botany, at least, few investigators take pains to acquire a working knowledge of the Italian language, and in consequence Italian work is best known to us through German and French reviews. The above work is a case in point: although issued over a year ago, no mention of it appears to have been made in any publication in the English language.

The book is one of a series now numbering somewhat over fifty—the Biblioteca Agraria Ottavi—all of which deal with some phase of technical agriculture, and in general represent the best Italian thought along their particular lines. No attempt has been made in this book to exhaust the subject of plant pathology, but the information contained is condensed and up to date: in many cases forming the best summary of knowledge on the given subject that the reviewer has seen in any language. But little attention is paid to mycology; the taxonomic position of the parasite does not determine the order of discussion; in other words, the book is written from the standpoint of the disease itself—making it almost unique among works on plant pathology. There is no index, but a very full table of contents; there are practically no illustrations. The book is printed in large, clear type, generally free from errors. In one place a curious transposition of paragraphs has occurred, which the author, *in litteris*, corrects as follows: the matter from and including the title 'La Peronospora del Frumento' on p. 96, to the beginning of the last paragraph on p. 97, should be interpolated between the second and third paragraphs of p. 93 following the words '* * * come suol farsi.' Also the matter beginning at the bottom of p. 97 with the words 'Quando si approssima * * *' and ending on p. 99 with '* * * contestata da diversi studiosi.' should be interpolated on p. 102 before the words 'Quando una uredospora, od una * * *.'

The book opens with a chapter of generalities, after which two chapters are devoted to fungicides and the methods of applying them. A much greater number and variety of fungicides appear to be in common use than have been found practicable in America. It is interesting to note how, owing to field conditions, the knapsack type of sprayer is almost exclusively used. With cheap and efficient labor the results of such spraying should be, and apparently are, much better than those obtained here by the wholesale use of power and other large sprayers.

The remaining chapters are devoted to specific diseases of the following plants: wheat, oats, barley, corn, rye, rice, alfalfa, beans, peas, potatoes, beet, cabbage, turnip and other cruciferæ, hemp, flax, tobacco, watermelon, strawberry, onion, tomato, grape, pear, apple, peach, plum, olive, citrous fruits, mulberry. Three chapters are devoted to diseases attacking plants in the seed-bed, to the treatment of wounds, and to the various forms of root-rot of trees. It is unfortunate that the author has not seen fit to include some bibliographical matter under these various heads; or at least to bring Voglino's bibliography (1895) down to date.

This book is of especial interest to American plant pathologists at this time on account of its discussion of the diseases of the semi-tropical plants, largely untreated in other general works. It amply merits translation into English and other languages.

Haven Metcalf
U. S. Department of Agriculture

SCIENTIFIC JOURNALS AND ARTICLES

The American Naturalist for January contains the following articles: 'Note on the Habits of Fierasfer,' by Edwin Linton, describing the manner in which the fish enters tail first the body of a holothurian. 'Records of Pennsylvania Fishes,' by Henry Fowler, giving definite localities for many species. 'Specific Name of *Necturus maculosus*,' by F. C. Waite. Tetradactylus is unavailable as a specific name, not because it refers to a generic character, but because it was not applied as a scientific name; what Lacépède wrote was *Protéetétradactyle.* Under 'Volvox for Laboratory Use,' Bertram G. Smith tells how it may be kept and J. A. Cushman records seven species of 'Ostracoda from Southeastern Massachusetts.'

The Museum's Journal of Great Britain has for its leading articles 'How to Promote Interest in Museum Collections,' by H. Conwentz, and 'Children and the Cult of the Beautiful,' by Beatrice V. Vernon. The first applies particularly to what may be termed local museums, and we doubt if many of the suggestions would prove to be practicable in a large institution. Miss Vernon's article deals largely with art museums and Mr. Harlan I. Smith will find in it methods akin to his interrogative label.

The Zoological Society Bulletin for January is an unusually good number. We can only note among other articles those on 'The Goat Herd,' 'An Almost Extinct Bird' (the California Vulture), the 'African Vipers,' 'The Frigate Birds' and 'Collecting for the Aquarium.' It is announced that the last of the large buildings are expected to be completed by the end of 1908, and that the attendance for the year was 1,300,000. A green turtle received at the aquarium weighed 540 pounds; the length of the upper shell was 4 feet 6 inches. It may be seen from this how large a thousand-pound turtle would be. The attendance at the aquarium during 1906 was something over 2,000,000.

The Museum News, of the Brooklyn Institute for February announces the installation of a group of Atlantic walrus. The principal article in the Children's Museum section is on the muskrat and states that in the Hackensack marshes the muskrats seem to live in houses throughout the year, the summer houses being more loosely built than the winter home.

The Fortnightly Review for January contains an article by E. Ray Lankester describing in some detail the work carried on at the British Museum during the past eight years under his immediate supervision.

SOCIETIES AND ACADEMIES

THE ST. LOUIS CHEMICAL SOCIETY

At the regular meeting of the society on January 14, Mr. A. H. Kelling presented a paper entitled 'Sewage Purification.' Pollution of water supplies, and the danger of infection resulting from the habits of the house fly, were dwelt on as showing the importance of the subject. The three methods by precipitation, by means of sewage farms, and by means of septic tanks and filtration were then treated in considerable detail. After the discussion, which followed the presentation of this paper, Dr. Andrews presented a paper, which favorably discussed the probable appearance of some official interpretations of certain regulations contained in the last issue of the U. S. Pharmacopœia, with reference to the Pure Food and Drugs Act.

C. J. Borgmeyer,
Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE

GENETIC LOGIC

To the Editor of Science: Dr. Tawney's criticism of my book on 'Genetic Logic,' in your issue of February 1, calls for a word or two of comment.

He is mistaken in supposing the 'dualism of control' as I develop it represents my own view of the nature of reality.

Being the 'knower's logic' that I am developing—a matter made so explicit that the phrase is coined to express it—it is strictly the *knower's point of view* from which such a dualism is depicted. I aim to trace the natural history of the naïve dualism of knowledge inside the consciousness that has it. My motive in saying this here is not so much to meet this criticism as the wish to explain an ambiguous phrase I have employed in the book. I speak of 'foreign control' as of something 'foreign to the process itself' (of knowledge). What I mean is '*seeming to the process itself to be foreign*,' not '*seeming to the writer to be foreign to or apart from the process*.' There is a real ambiguity in the phrase, and I am herewith calling attention to it. Others may be misled by it.

Dr. Tawney also says that many of the 'objects' I distinguish are not such from the knower's point of view; implying that by using that phrase I limit the 'knower's logic' to process having a self-conscious knower, that is, to consciousness of *the self-knowing or reflective type*. On the contrary, I use 'knower' for any process that has knowledge, as is customary; the dog, the worm, the mollusc is a 'knower' so far as it knows anything; indeed, I have gone to excessive pains to say that I treat of cognitive meanings from the point of view of *the process that has them—the psychic point of view*. How and with what *psychic meaning* there arise objects of knowledge in a progressive series, it is the main problem of my book to discuss. It begs the whole question to assume that there is no knowledge except that which *knows the self*. With the conclusions one may differ, but the doctrine should be clearly expounded.

Finally, a word in ethics. It is a writer's duty sometimes to help the critic understand his views; for a real embarrassment may arise to one who fears to criticize, lest the criticism, though possibly due to misinterpretation, may yet seem to misrepresent and so to have upon it the taint of intellectual dishonesty. I myself have sometimes felt in studying a book that if I knew the writer's own real mind to be or not to be what I take it to be, I should feel more free in criticizing him; for, of course, if a critic does know better, misrepresentation is dishonest. So though in itself a point be hardly important enough to require attention, yet the author may have the duty of aiding those who take interest in his work.

J. MARK BALDWIN
JOHNS HOPKINS UNIVERSITY,
February 3, 1907

SPECIAL ARTICLES

SYMMETRY IN BIG CLAWS OF THE LOBSTER

THE lobster is probably the most widely known of living crustacea, as it is one of the largest, and most eagerly sought for food. Few who have seen it have failed to notice that the great 'forceps' or big claws are unlike, the larger, which weighs from one quarter to one half as much as the entire animal, having crushing tubercles, while the smaller and slenderer is armed with tooth-like spines. The toothed, or 'quick,' claw, which is a 'lock-forceps' with serrate jaws, is used for seizing and holding, and the larger crushing, or 'club,' claw for rending and crushing the prey.

The large claws occur about as frequently upon the right as upon the left side of the body, and without distinction of sex, but as I have shown to be the case with the shrimp *Alpheus*, in which the asymmetry and inequality of the great chelæ are even more marked, this condition is probably one of direct inheritance, all members of a brood being either right-handed or left-handed. That is to say, the normal position of the toothed or crushing claw is not haphazard, but is predetermined in the egg.

In 1895 I described a variation in the American lobster[1] (*Homarus americanus*) in which both the big chelæ were similar, and of the toothed type. This variation was exceedingly rare, only three cases having been found in a collection of 2,430 lobsters made by Mr. Vinal N. Edwards, the naturalist and collector of the U. S. Fisheries' laboratory at Woods Hole, Massachusetts.

Since that time several papers have appeared upon this subject, notably by Stahr,[2]

[1] 'The American Lobster,' Bull. U. S. Fish Comm., 1895, p. 143.

[2] *Jenaischen Zeitschrift f. Naturwiss.*, 33 Bd., 1898.

who found this variation of similar toothed claws much more common in the European lobster. The history of development proves, as Stahr upon theoretical grounds maintained, that the toothed claw represents the more primitive, and the crushing claw the more modified, type. It, therefore, seemed natural to infer, as he did, that the anomalous symmetry in these weapons had been brought about by loss of a crushing claw, and subsequent reversion to the primitive toothed condition in the regenerated member which took its place. This would give us a lobster with symmetrical toothed claws like the variation described.

The converse of this, or the production of a new crushing claw in place of a toothed 'forceps' could not occur upon Stahr's theory of regeneration, and hence he inferred that my report of a case of *similar crushing* claws in a lobster was an error. Herr Stahr is not to be blamed, for this report was based upon the statement of a fisherman. Yet, however great the unreliability of this class of men in biological matters, I have yet to find a 'lobsterman' who could not tell a 'club' from a 'quick' claw. It now seems that the maligned fisherman was right, and he should get his dues, while Herr Stahr's theory will have to be revised, for Dr. W. T. Calman, of the British Museum, has recently described[3] a case of symmetrical crushing claws in the British lobster (*Homarus gammarus*), and his account is accompanied by an excellent photograph. In all other respects this animal was a perfectly normal male. It was caught near Stromness, Orkney, and its living weight was four pounds ten ounces.

In a letter regarding this unique specimen Dr. Calman says:

> The correspondence between the two chelæ as regards arrangement and size of the crushing tubercles is even closer than appears on the photograph, where slight differences of color have a little obscured the shape in one or two points. The differences are no greater than one would expect to find between the two sides of a normally symmetrical animal. In other respects the chelipeds are practically alike in size and shape, except that, as seen on the figure, the dactylus of the left is shorter than that of the right. The basal segments of the limbs show no trace of asymmetry which is often associated with regeneration.

[3] In *Proceedings of the Zoological Society of London* for 1906.

The shrimp *Alpheus* carries a huge 'hammer,' or snapping, claw, which in some species is as large as the entire body of the animal, and a diminutive claw of more primitive form on the opposite side. Moreover, in the common *Alpheus heterochelis* of the southern coast the small chela presents an interesting sexual variation of which I have drawings and notes made in 1893, but about which nothing had been published until the appearance of Wilson's interesting studies on the 'Reversal of Asymmetry in *Alpheus*' in 1903. The small chela of the male resembles the 'hammer' more closely than does the corresponding simpler and more primitive claw of the female.

A remarkable example of heteromorphic regeneration or reversal of asymmetry is seen when the *Alpheus* 'shoots' its 'hammer,' or for any cause loses its big claw, as was discovered by Prizibram in 1891. The big claw seems to hold the little one in check, for no sooner is it lost than the smaller one grows apace, and becomes differentiated into a 'hammer' or a 'snapper,' while in compensation for this change a diminutive chela of the primitive type replaces the great claw lost from the opposite side. Wilson found that in both sexes the small claw, which was regenerated from the stump of the large one, was always of the simpler female type, and, moreover, that the small chela of the male was more rapidly changed into the big 'pistol' or 'hammer' claw because it was already farther advanced on this line of development than that of the female. When the smaller claw is amputated, or when the 'forceps' are removed from both sides of the body at once, there is no reversal, a new cutting chela or hammer claw taking the place of the corresponding member lost. Many additional facts have been brought to light through the experimental studies of Wilson, Brues and Zeleny.

In the lobster no reversal or compensatory regulation normally or usually attends the regeneration of any of its appendages. The crushing or the toothed forceps, when severed at the 'breaking plane,' are replaced by their like in due time after one or more molts. How, then, are we to explain the anomaly of similar claws? It seems highly probable that the reversal, which regularly takes place in *Alpheus* when its great 'hammer' claw is cut off, does actually occur, though but rarely, in the lobster, or rather that it involves one side only, there being no immediate compensatory change to restore equilibrium of the system of which the great claws form a part. Thus, when a 'club' claw is 'shot' or amputated by the experimenter, a chela of similar crushing type is usually regenerated in its stead, but rarely a toothed claw appears. There is a reversal of the appendage, bringing about an abnormal condition of symmetry, but the process stops here, and we have as the result lobsters with similar toothed claws, like the specimen illustrated in my earlier work referred to above.

In like fashion the toothed claw of the lobster is usually replaced in regeneration by a limb of similar type, as is the rule with *Alpheus*, but in rare cases a reversal occurs here also; a 'club' claw appears, and we get a lobster with symmetrical crushing chelæ, like the specimen described by Dr. Calman. As this case is, for the present, essentially unique in the literature of the subject, we may be sure that it is much rarer than reversal from crushing to toothed claws. There is the possibility that these abnormal conditions of symmetry may be upset by a compensatory change in the appendage of the opposite side, but there is no evidence at present that this ever takes place.

In the first case the reversal from crushing to toothed claw happens to reproduce the primitive form of limb, but we see no reason for regarding this as a case of reversion to an ancestral stage, in the sense in which Stahr uses the phrase.

The explanation just offered is based on the assumption that regeneration, following loss, actually occurs in these cases. If there has been no regeneration, we must then fall back upon the view that as asymmetry in the great forceps is normally produced by changes which take place in the egg, so the rare condition of symmetry in these appendages may be casually brought about in the same way.

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CURRENT NOTES ON METEOROLOGY

THE WEATHER OF SAXONY

THAT portion of the *Deutsches Meteorologisches Jahrbuch* for 1902 which relates to the kingdom of Saxony (Dresden, 1906) contains a discussion of the special observations made at the meteorological stations in that section with a view to furnishing information regarding the general weather conditions, for use in forecasting. In addition to the regular observations made thrice daily, the observers note the prevailing type of weather during the morning, afternoon and evening, according to a scale of ten. These types differ somewhat in summer and in winter. They are simple; are noted by means of symbols, and have been found to serve very well in giving the forecaster an excellent general view of the prevailing weather over the kingdom. Such a scheme might well be adopted among the volunteer observers in this country and elsewhere, and would often serve to give a clearer idea of the weather conditions than do the regular meteorological observations taken twice a day. A second paper, on the dependence of weather in Saxony on the prevailing weather conditions of Europe, will be found useful in detailed studies of European climatology.

RAILROAD BUILDING IN ARID REGIONS

THE climatic difficulties now being met with in the construction of the new railroad between Damascus and Mecca are naturally similar to those previously encountered in other arid regions. The main line is being built across a desert highland where for hundreds of miles there are no permanent human settlements within thirty to fifty miles of the track. The country from Damascus to Medina is inhabited only by small bands of nomadic Bedouins. The scarcity of water is giving serious trouble. Some new artesian wells have been sunk, but all water for drinking and cooking purposes, and for preparing mortar, is carried in water cars. The expense of construction is thus greatly increased. All fuel has to be imported, but owing to the high temperatures little is required except for the locomotives and the repair shops.

METEOROLOGY IN EGYPT

Captain H. G. Lyons, Director General of the Survey Department of Egypt, gives an encouraging account of the present status of meteorological work in Egypt in his 'Report on the Work of the Survey Department in 1905' (Cairo, 1906). There are in Egypt ten stations and in the Sudan sixteen stations. The locations are shown on a map. There are also a number of river gauge stations. The central meteorological station at Helwan, of which a picture is given, is an attractive building, lighted by electricity, and equipped with standard instruments. Captain Lyons has already published several reports of unusual value on the meteorology of the Nile Basin, especially in connection with the Nile floods.

RAINFALL AND ALTITUDE

Hann calls attention (*Met. Zeitschr.*, Nov., 1906) to the results of rainfall measurements made in connection with the water supply of the city of Edinburgh, in the district of the upper tributaries of the Tweed. Under similar conditions of exposure, but at different altitudes, the rain-gauges show on the average an increase of 2.5 inches of rainfall in each 100 feet of increase of elevation.

THE 'SCOTIA' RESULTS

Mention has several times been made in these notes of the meteorological results of the *Scotia* Antarctic expedition, these notes being based on short reports which have appeared in scientific journals. There has lately been published 'The Voyage of the *Scotia*,' by three of the staff (Edinburgh, Blackwood, 1906), which gives a popular account of the expedition, as well as a summary of the scientific results obtained.

R. DeC. Ward

MR. ROCKEFELLER'S GIFT TO THE GENERAL EDUCATION BOARD

Mr. John D. Rockefeller has announced his intention to give, not later than April 1, securities valued at about $32,000,000, to the General Education Board, which he had previously endowed with $11,000,000. The letter announcing this gift, read at a meeting of the board on February 7, is as follows:

New York, Feb. 6, 1907.

General Education Board, 54 William Street,
New York City.

Gentlemen: My father authorizes me to say that on or before April 1, 1907, he will give to the General Education Board income-bearing securities, the present market value of which is about thirty-two million dollars ($32,000,000), one third to be added to the permanent endowment of the board, two thirds to be applied to such specific objects within the corporate purposes of the board as either he or I may, from time to time, direct; any remainder not so designated at the death of the survivor to be added also to the permanent endowment of the board.

Very truly,
John D. Rockefeller, Jr.

The board has acknowledged this great gift in the following terms:

The General Education Board acknowledges the receipt of the communication of February 6, 1907, from Mr. John D. Rockefeller, Jr., a member of this body, announcing your decision to give to the board for the purpose of its organization, securities of the current value of $32,000,000. The General Education Board accepts this gift with a deep sense of gratitude to you and of responsibility to society. This sum, added to the $11,000,000 which you have formerly given to this board, makes the General Education Board the guardian and administrator of a total trust fund of $43,000,000.

This is the largest sum ever given by a man in the history of the race for any social or philanthropic purpose. The board congratulates you upon the high and wise impulse which has moved you to this deed, and desires to thank you, in behalf of all educational interests whose developments it will advance, in behalf of our country whose civilization for all time it should be made to strengthen and elevate, and in behalf of mankind everywhere, in whose interests it has been given and for whose use it is dedicated.

The administration of this fund entails upon the General Education Board the most far-reaching responsibilities ever placed upon any educational organization in the world. As members of the board, we accept this responsibility, conscious alike of its difficulties and its opportunities.

We will use our best wisdom to transmute your gift into intellect and moral power, accounting it a supreme privilege to dedicate whatever strength we have to its just use in the service of men.

It is said that Mr. Rockefeller's gifts to education now amount to about $80,000,000.

The General Education Board has so far made appropriations, amounting to more than $1,000,000, to eighteen colleges on condition that three times the amount appropriated should be obtained by the institutions from other sources. At the meeting of the board, on February 7, the following appropriations, as we understand on the same conditions, were made: Beloit College, Beloit, Wis., $50,000; Morningside College, Sioux City, Iowa, $50,000; Lafayette College, Easton, Pa., $50,000; Wabash College, Crawfordsville, Ind., $125,000, and the University of Wooster, Wooster, O., $125,000.

SCIENTIFIC NOTES AND NEWS

PROFESSOR ERNEST W. BROWN, who this year goes from Haverford College to Yale University, has been awarded the J. C. Adams prize of Cambridge University, for his work on the motion of the moon.

PROFESSOR WILHELM VON BEZOLD, professor of physics and meteorology at Berlin, and Professor Theodor Boveri, professor of zoology and comparative anatomy at Würzburg, have been elected corresponding members of the St. Petersburg Academy of Sciences.

PROFESSOR W. M. DAVIS, of Harvard University, has been elected foreign correspondent of the Geological Society of London.

IN celebration of the twenty-fifth anniversary of the coming to Lehigh University of Professor Joseph F. Klein a reception was given him in South Bethlehem on the evening of January 22.

AT the annual general meeting of the Royal Meteorological Society, Mr. Richard Bentley, the president, on behalf of the members of the council, presented an illuminated address to Mr. William Marriott, in recognition of his services as lecturer for the society.

THE following committee has been appointed by the president of the Washington Academy of Sciences to take charge of the arrangements for the entertainment of the International Zoological Congress during its visit to Washington after the adjournment of the Boston meeting in August, 1907: L. O. Howard, chairman, Frank Baker, W. H. Dall, H. G. Dyar, B. W. Evermann, Theodore N. Gill, H. W. Henshaw, C. L. Marlatt, A. D. Melvin, C. Hart Merriam, T. S. Palmer, B. H. Hanson, Richard Rathbun, Robert Ridgway, H. M. Smith, Leonhard Stejneger, C. W. Stiles, F. W. True, T. Wayland Vaughan, C. D. Walcott.

SECRETARY TAFT will, it is said, appoint a committee, consisting of Charles F. McKim, the architect; Frederick L. Olmstead, Jr., of Boston, landscape gardener, and Frank D. Millett, the artist, to go to Niagara and gather material for a report looking toward harmonizing the commercial buildings there, particularly the power plants, with the natural scenery.

PROFESSOR J. C. ARTHUR and Mr. F. D. Kern, both of Purdue University, Lafayette, Indiana, continued their researches on plant rusts in the New York Botanical Garden herbarium during the month of January. Dr. M. T. Cook has been awarded a research scholarship at the garden for three months, beginning on January 1.

A CABLEGRAM to the daily papers from Calcutta states that news from Dr. Sven Hedin, the Swedish traveler, who last year started from Chinese Turkestan on a journey of exploration to Tibet, has been received. Dr. Hedin, who, when he sent off his courier, had already explored 840 miles of unknown country, said he hoped to reach the monastic city of Shigatse, 130 miles from Lhasa, at the end of February.

The eighth lecture in the Harvey Society course was given by Professor George Huntington, of Columbia University, at the New York Academy of Medicine on Saturday evening, February 9, on 'The Genetic Interpretation of the Variations in the Genito-Urinary Tract.'

On January 15, Professor W. E. Castle delivered an address before the American Breeders' Association, at Columbus, Ohio, on the subject, 'The Production and Fixation of New Breeds.'

Professor R. DeC. Ward, of Harvard University, gave an illustrated lecture on 'Clouds: their Formation, Classification and Value as Weather Prognostics,' before the Contemporary Club of St. Louis on February 1; before the Society of Pedagogy of St. Louis on February 2, and before the pupils of the St. Louis Central High School on February 4. He also gave an informal talk before the Science Section of the Wednesday Club of St. Louis on February 4.

Dr. Charles E. Garman, professor of philosophy at Amherst College, died on February 9, at the age of fifty-seven years.

Dr. P. J. Möbius, the author of books on various pathological, psychological and sociological subjects, has died at Leipzig at the age of fifty-three years.

It is expected that there will be a vacant fellowship, and perhaps two vacant fellowships, at the Lick Observatory, University of California, for the academic year beginning July 1, 1907. If the holder of a fellowship desires to pursue studies leading to the degree of doctor of philosophy, he will be expected to reside on Mount Hamilton during eight months of the year and in Berkeley during four months of the year. If the appointee has already taken his doctor's degree, he will probably reside on Mount Hamilton during the entire year. The stipend connected with each fellowship is $600 per annum. Applicants should address The Director, Mount Hamilton, California.

Professor Charles Lane Poor and others appeared before the legislative committee on February 5 to argue in favor of Assemblyman Young's bill to incorporate the New York Observatory and Nautical Museum, and to empower the Board of Estimate and Apportionment in its discretion to enter into an agreement with the corporation for the erection by the city in one of the public parks of suitable buildings, which the corporation is to equip with instruments and collections of nautical science for public exhibition and research.

UNIVERSITY AND EDUCATIONAL NEWS

The seventy-fifth anniversary of the foundation of Lafayette College will be celebrated from June 16 to 19 of the present year. An effort is being made to celebrate the anniversary by the collection of an endowment fund of $500,000. Of this fund more than $325,000 has been secured, and Mr. Andrew Carnegie, who has given $50,000, has also promised to give the last $50,000 of the full sum of half a million.

Mr. Andrew Carnegie has given $40,000 to Yale University for a swimming pool.

The trustees of the University of Illinois will ask the legislature for the sum of $1,600,000, in round numbers, for the running expenses of the university during the coming biennium and for $62,000 for increase in the plant. It has also decided to ask for a special appropriation of $1,000,000 for the erection of much needed buildings upon the campus in Urbana. It has, furthermore, decided to ask for $385,000 for the erection or purchase of buildings to house adequately the medical department in the city of Chicago.

At Columbia University Dr. Dickinson S. Miller, now lecturer in philosophy, has been made professor of philosophy, and Dr. Gary N. Calkins, now professor of invertebrate zoology, has been made professor of proto-zoology.

Dr. Donald MacAlister, fellow and director of medical studies of St. Johns College, Cambridge, has been appointed principal of Glasgow University.

Dr. Georg Klebs, of Heidelberg, has been called to the chair of botany at Halle, vacant by the death of Professor E. Pfitzer.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, FEBRUARY 22, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE BOTANICAL SOCIETY OF AMERICA

A UNION OF THE BOTANICAL SOCIETY OF AMERICA, THE SOCIETY FOR PLANT MORPHOLOGY AND PHYSIOLOGY, AND THE AMERICAN MYCOLOGICAL SOCIETY

THE first meeting of the united societies (the thirteenth annual meeting of the Botanical Society of America) was opened at Columbia University, New York City, December 27, 1906, under the presidency of Professor F. S. Earle. About sixty members were in attendance at the meeting, which was a most interesting one. The success of the meeting was in no small degree due to the thoughtful hospitality of the officers of the New York Botanical Garden, the Botanical Department of Columbia University and the Torrey Botanical Club.

The officers elected were:

President—Professor G. F. Atkinson, Cornell University.

Vice-President—Director N. L. Britton, New York Botanical Garden.

Treasurer—Dr. Arthur Hallock, New York Botanical Garden.

Secretary—Professor D. S. Johnson, Johns Hopkins University.

Member of Council—Professor H. M. Richards, Columbia University.

Seventeen associate members were elected and the total membership was increased to 135.

The society voted to hold its next annual meeting in conjunction with the American Association for the Advancement of Science.

Sessions for the reading of papers were

held at Columbia University on December 27 and 31, and at the museum of the New York Botanical Garden on the morning of December 29. At the latter meeting the presidential address—'The Organization of Certain Cœnobic Plants'—was delivered by Past President R. A. Harper. After the meeting a luncheon was served in the laboratories of the garden to the members of the society and Section G of the American Association for the Advancement of Science.

Abstracts of the technical papers read follow:

Figures Produced by Protoplasmic Streaming in Fungi and Slime Moulds: Professor R. A. Harper, University of Wisconsin.

The Origin of Air-Chambers in Liverworts: Professor C. R. Barnes and Dr. W. J. G. Land, University of Chicago.

Intercellular spaces are formed in higher plants by the splitting of cell walls and the separation of the cells by their unequal growth and turgor. The air-chambers of liverworts, especially of Marchiantiales, are so peculiar in form that some peculiar mode of origin was looked for. Hofmeister ascribed their formation in *Marchantia* (1862) to the detachment of the epidermis and its lifting by the plates of cells which form the side walls. This view made the air-chamber only a special form of intercellular space. But Leitgeb controverted this interpretation (1880), and his view has been universally accepted for a quarter of a century. He ascribes the origin to a slower growth of the cells at the point where four to six lateral walls meet, and the consequent formation of a pit, which becomes overgrown by adjacent cells as it deepens. He emphasizes the statement that the lowest point of the pit is the original surface, and homologizes the air chambers strictly with the pits, nearly closed at mouth, in which many sex organs are sunk.

The authors show that Hofmeister probably did not see the real origin of the air-chambers, but only later stages in their development; that Leitgeb's observations, correct as far as his descriptions and figures show, were misinterpreted by him, and do not account for the course of development described; and further, his few observations of the real origin were actually distorted (according to his own confession) to fit his theory of the homology of the air chambers with sex-organ pit.

Evidence can now be adduced, by means of the superior technique available, which shows beyond doubt that the air chamber appears first as an internal intercellular space, usually within two segments from the apical cell. This space increases in size, breaks out to the surface (rarely being met by inward splitting from the surface), and sometimes deepens by further splitting inward from the original point of origin. The remainder of the development is due to growth in all dimensions, but chiefly parallel to the surface. Thus the floor of the air-chamber, instead of arising from the original surface and being roofed over by adjacent cells, as is universally taught, is an internal surface, formed, as in other plants, by splitting due to unequal turgor and growth. Instead of being homologous with the sex-organ pits the air-chambers are strictly homologous with intercellular spaces generally.

Fertilization and Embryogeny in Ephedra Trifurca: Dr. W. J. G. Land, University of Chicago.

In *Ephedra trifurca* Torr. the pollen chamber extends to the female gametophyte, and the necks of the archegonia are freely exposed. The pollen grains rest in the bottom of the pollen chamber in contact with the female gametophyte.

Cultures of the male gametophyte in saccharose solutions gave the following results: The exine is ruptured and its contents completely freed. The body cell gives rise to two elliptical male cells equal in size and optical appearance. Later the pollen tube appears and continues to elongate for about twenty-four hours. Fertilization is possible within ten hours after pollination. Four days having elapsed between the collecting of material at Mesilla, N. M., and its fixation at the University of Chicago, it was not possible to observe stages in the progress of the pollen tube to the egg.

The fertilized egg gives rise to eight free nuclei more or less unequal in size. The number of cells which reach the suspensor stage varies from two to five. Immediately after the egg is fertilized the walls of the jacket cells disappear and their contents are mixed with the cytoplasm of the egg.

The chromatin of the second male cell and also that of the jacket cells seems to be responsible for a mass of minute ephemeral cells. Possibly this mass of small cells may be looked upon as at least suggesting how the endosperm of angiosperms may have originated.

The functioning embryonal cells become spherical. Two free nuclei are formed in each embryonal cell and soon afterward the suspensor appears. After the suspensor reaches a considerable length one of the free nuclei passes into it and downward to the tip. Immediately above the latter nucleus a ring of cleavage appears in the cytoplasm, beginning at the suspensor wall and gradually continuing inward until the cytoplasm of the embryo is separated from that of the suspensor. A wall is then laid down and the separation is complete. The suspensor nucleus passes into the suspensor and takes a position near the embryo, where it soon disintegrates. The walls of the embryo are laid down in the usual manner on the cell plate.

Under favorable conditions the development of the embryo is continuous and the young plant breaks out of the seed before the strobilus is shed. From the time the strobili can be recognized until the appearance of the young plant about six months elapses, being the shortest time yet reported for any gymnosperm.

Dioon and Ceratozamia: Dr. Chas. J. Chamberlain, University of Chicago.

An account was given of a second trip made to Mexico for the study of *Dioon* and *Ceratozamia*, by the aid of a grant from the Botanical Society of America.

Fruiting specimens of *Dioon spinulosum* were not found during the trip, but material has been secured. There are frequently more than two ovules on a sporophyll, sometimes as many as five; the seeds are larger than those of any known cycad except *Cycas* itself. The ovulate cones are reported to be lateral.

Ceratozamia was found in considerable abundance. The motile sperms were studied in the living condition and material has been secured for a study of most phases in the life history. The seeds are small and are shed soon after fertilization, so that there is a continuous development, without any resting period, from fertilization up to the leafy plant.

A series of photographs were obtained, of which lantern slides were shown.

The Genus Pleurococcus Culturally Considered: Dr. G. T. Moore, Bureau of Plant Industry.

The Early Growth of Monostroma and Enteromorpha: Dr. Tracy E. Hazen, Barnard College.

Our knowledge of early stages of growth in the family Ulvaceæ is comparatively meager. In the genus *Monostroma*, the only complete account is that of Reinke on *M. bullosum,* a fresh-water species showing the nearest approach to *Tetraspora,* and not infrequently included in the latter genus. More extended investigation of the marine species is desirable.

In a *Monostroma* which is rather abundant on rocks and woodwork in harbors about New York (probably a small form of *M. crepidinum* Farlow) a fairly complete series of young plants has been obtained. The earliest stage is not a hollow sphere as described by Reinke for *M. bullosum,* but a short, erect filament somewhat resembling that of *Ulothrix.* Soon, by longitudinal divisions in its upper cells, this filament becomes club-shaped, and by further divisions, followed by separation of the cells, balloon-shaped. The lower cells divide little, but elongate to form the characteristic rhizoidal cells found in most Ulvaceæ. The balloon-shaped portion splits open at the top by irregular rents, to form the lobed, expanded thallus characteristic of the mature plants.

In two species of *Enteromorpha* found with the *Monostroma,* I have discovered a very similar early growth; beginning with the formation of a shorter filament, the thallus takes on a long cylindrical, club-shaped form, which becomes tubular directly, and not after the formation and subsequent splitting of a flat, two-layered sheet, as usually described for this genus.

The early growth of *Ulva* is also said to be filamentous. This similarity of young stages shown by these three genera appears to indicate a rather close relationship to the Ulotrichaceæ, so that the separation by recent writers of the Ulvaceæ to a distinct order may not be sufficiently warranted.

Spore Formation in Derbesia: Dr. B. M. Davis, Cambridge, Mass.

Derbesia, whose general morphology and cytology is that of the Siphonales, is unlike the other members of this group in having very large zoospores, very unlike the small biciliate zoospores and motile gametes characteristic of this group of algæ, since each is provided with a large circle of cilia. The zoospores are developed in relatively small numbers (50–200), in sporangia which contain many thousands of nuclei when first formed upon the parent filaments.

A process of nuclear differentiation sets in shortly after the sporangia are developed. Some of the nuclei increase to four to six times their original size and finally become the nuclei of the zoospores. All others degenerate, decreasing in size, losing their chromatin content and finally breaking down in the cytoplasm. The large surviving nuclei are rather uniformly distributed throughout the cytoplasm, but are not associated with any cytoplasmic centers such as cœnocentra.

The spores are formed by cleavage furrows which enter the protoplasm from the periphery and by branching in different planes cut out the protoplasm into approximately equal masses around the large nuclei. The nuclei at this stage lie in the centers of the spore origins and from them many delicate fibrils radiate into the cytoplasm among the plastids. These radiating cytoplasmic fibrils have small granules at their bases lying against the nuclear membrane.

The nucleus of each spore origin comes to lie near the periphery, the radiating fibrils on that side (about one third of the total number) becoming connected with the plasma membrane of the spore. The fibrils also take a funnel-shaped arrangement from the nucleus outward. The granules at the bases of these fibrils pass along them to the periphery and finally lie in a circle about twice the size of the nucleus, just beneath the plasma membrane.

The granules then fuse together to form a firm, deeply staining, homogeneous ring which is the blepharoplast, and lies very close to the plasma membrane but is not a part of it. The fibrils which connect the blepharoplast with the nucleus disappear and the nucleus somewhat later passes back to the center of the spore. The blepharoplast now splits into two rings, one directly below the other. Cilia grow out from the lower ring forming a circle around the zoospore, which is about one half the diameter of the latter.

Sexuality in the Mucors: Dr. A. F. Blakeslee, Harvard University.

The *Mucors* are divided into two groups termed respectively homothallic and heterothallic. In the homothallic group, zygospores are developed from the same thallus and can be obtained from the sowing of a single spore. In the heterothallic group, which comprises a large majority of the species, zygospores are developed from branches which necessarily belong to thalli diverse in character and can never be obtained from the sowing of a single spore. Every heterothallic species is therefore an aggregate of two distinct sexual strains through the interaction of which zygospore production is brought about.

In the heterothallic species *Mucor Mucedo,* the segregation of sex is completed at or before the germination of the zygospore and all the spores in a given germ sporangium are of the same strain, either male or female.

In the germination of the zygospores of the heterothallic species *Phycomyces,* a segregation of sex takes place at the formation of spores in the germ sporangium which contains both male and female spores. The liverwort *Marchantia* polymorpha has been found to correspond to this latter type of zygote germination and male and female spores are contained in a single capsule.

Cultures were exhibited showing dark lines of zygospores between male and female strains of the same species and white lines of imperfect hybrids between male and female strains of different species. Microscopic preparations of zygospores of various species were also exhibited.

The Teaching of the Subject of Respiration: Professor Charles H. Shaw, Ursinus College.

The word respiration is used in several different senses. A confusion of ideas also exists. Except in connection with highly differentiated animals, the term respiration must signify either a gaseous interchange, or a metabolic process of energy release. The former definition lends itself to clear statement, is readily developed from experiments, and refers to a process which is really a non vital one. The latter, though not so easy to teach, refers to the essential process. For several reasons it is thought that the latter will stand. At all events the process of energy release must occupy the central place in teaching.

It is sometimes said that in respiration $CO_2/O_2 = 1$, and also that all protoplasm must obtain oxygen somehow all the time. Such formal ideas will not square either with the published facts upon the subject nor with class-room experiments.

The subject is worthy of a more adequate treatment in general courses. Eudiometers over mercury with the seeds held up by glass wool are one favorable form of apparatus for demonstrating.

Relative Transpiration in Cacti: Dr. Burton Edward Livingston, Desert Botanical Laboratory.

After a preliminary discussion of the meaning of relative transpiration, *i. e.,* the ratio of the increment of water-loss from the plant for any time interval to the corresponding increment of loss from an evaporimeter for the same interval, the paper proceeds to present a new fact in regard to the regulation of transpiration. While ordinary leafy plants in some way retard water-loss during the hours of darkness and remove the retarding influence during those of light, the cacti, at least as far as the study has gone, act in a manner exactly opposite, applying the retarding influence during the daylight hours and removing it during those of darkness. Thus, for a given transpiring surface, leafy plants lose water in the daytime more nearly at the rate of the same area of free water surface than they do at night, and cacti more nearly approach the evaporation rate from a water surface during the night than in the daytime. Data as to the nature of the mechanism by which either group of plants accomplish their regulation of water-loss is as yet entirely lacking, since Lloyd, in a paper about to be published, has thrown great doubt on the usually accepted idea that this regulation is mainly accomplished through stomatal movements.

The Water-Storing Tubers of Nephrolepis cordifolia: Professor J. W. Harshberger, University of Pennsylvania.

Nephrolepis cordifolia is a fern occasionally met in cultivation. When grown in the open it forms tubers the size of a walnut. These are developed at the end of lateral underground branches covered with flat, scale-like ramentæ which extend also to the surface of the tuber. The tubers do not store starch and other reserve foods, as an external examination of the tubers might lead one to expect, but the large, rounded, parenchyma cells are turgescent with a clear watery fluid, evidently stored against the time of drought, as the fern is usually epiphytic in habit. When these tubers are dried, they dry until they almost entirely shrivel up.

A New Native Host for Pearblight: M. B. Waite, Bureau of Plant Industry.

The pearblight bacillus, *B. amylovorus*, is undoubtedly a native parasite on the American indigenous species of Pomaceæ; it occurs nowhere else in the world and is quite commonly found on the wild crab-apples and hawthorns of the eastern United States.

When pears and apples and other pomaceous fruits were introduced into this country it promptly attacked them. It is quite easy to find new hosts on cultivated species of the Pomaceæ, as almost everything belonging to this family when grown within the territory affected is likely to be attacked. Some of these Pomaceæ look very unlike our ordinary pears and apples, but, nevertheless, may be subject to this disease, *e. g.*, the evergreen *Eriobotrys Japonica* is attacked by this disease very commonly in Florida and Georgia, and recently has been found affected in California. The arid plains and deserts and the Rocky Mountain region appear to have formed an insurmountable barrier, determining the western limits of the pearblight germ. Within the last few years, however, doubtless through human agencies, the pearblight bacillus has jumped, first to Colorado, Utah, Idaho, etc., and finally over the deserts and the Sierras into California. It is now attacking with unusual virulence the pear orchards of that state.

Few native Pomaceæ occur in California in the vicinity of the pear orchards. The beautiful, red-berried, California holly, *Heteromeles arbutifolia* Roem., is, however, quite common in the foot-hills of the Sierras, in the coast ranges, and comes down into the fruit regions. This shrub with its thick evergreen leaves looks very much unlike a pomaceous fruit, but was carefully watched for a year as a host for pearblight. After having practically given this up, however, in March, 1906, I found a striking specimen, badly attacked, at Vacaville. It had been affected on the blossoms the summer before and several of the twigs contained the living bacilli, they having been carried over the winter. It has since been found at Colusa and several other points in California.

The Causes of Dwarfing in Alpine Plants: Professor FREDERIC E. CLEMENTS, University of Nebraska.

Much attention has been given during the past eight years to the determination and measurement of the factors that determine alpine dwarfing in the Pike's Peak region of the Rocky Mountains. The work was begun with Bonnier's conclusions that the factors in dwarfing are stronger light, drier air and lower temperature, as working hypotheses. The behavior of many alpine polydemics which showed dwarf and normal forms at the same altitude, sometimes within a few feet of each other, indicated that light plays little or no part. Repeated and usually simultaneous measurements of light intensity were made in 1903, 1904, 1905 and 1906 at 1,900 m., 2,600 m. and 3,800 m. Midday readings at the three altitudes gave a value of 1 (comparative standard): in a few cases only, the intensity at 3,800 m. was 1.1 and 1.2 It is a well-known fact that the relative humidity increases with the altitude. As a rule, the relative humidity is 5–10 per cent. higher at 3,800 m. than at 2,600 m. and 15–20 per cent. higher than at 1,900 m. The variation is sometimes great, however, both simple and automatic readings giving now and then a lower humidity at the highest altitude. While humidity is not a factor in dwarfing, the reduced air pressure leads to increased transpiration, as demonstrated both by batteries of potometers and by water surfaces. The thermograph records of several years all agree in showing a great and regular decrease in temperature as the altitude increases. The decrease is about 1 1/3° for each 1,000 m., or an average difference of 25° F. between 1,900 m. and 3,800 m. The difference in the length of the season is correspondingly marked. The season is four and one half to five months at Manitou (1,900 m.) and two months on Mount Garfield (3,800 m.).

Of the factors stated by Bonnier, stronger light and drier air are not true of the region studied, and of course can play no part in dwarfing. Water content is the most important and universal factor, though its action is not at all restricted to alpine regions. Low temperature and shortness of season together stand next in importance, and even the third factor, reduced pressure, has a pronounced influence.

The Origin of New Forms by Adaptation: Professor FREDERIC E. CLEMENTS, University of Nebraska.

For purposes of experimental evolution, a careful census has been kept at Minnehaha of species that are undergoing modification. This not only gave much insight into the methods to be employed in producing new forms experimentally, but, for the region studied at least, it gave decisive evidence upon the relative importance of the four methods of origin, namely, variation, adaptation, mutation and hybridation. During seven years, but one genus, *Machæranthera,* showed sufficient variation to suggest that new forms might be arising from it in the manner assumed by Darwin. More than one hundred species have been recorded and studied in which new forms are arising by adaptation to new or changed habitats. Many of these ecads have been described by systematic botanists as new species. With the exception of an occasional monstrous growth, the only mutants observed have been the albino forms of red- and blue-flowered species. Of these nearly thirty have been found. In the lack of experiment, no certain hybrids have been found. With the exception of the oak and the willow, however, no plants occur which furnish any suggestion of hybridation. In the region studied, accordingly, adaptation is by far the most frequent method of origin, mutation stands next, hybridation is rare if present, while origin by variation, *i. e.,* the indefinite variability of Darwin, is extremely uncertain.

A Study of Disease Resistance in Watermelons: W. A. ORTON, U. S. Department of Agriculture.

A paper presenting the results of work on watermelon wilt (*Neocosmospora vasinfecta* var. *nivea* Erw. Sm.). A disease widely distributed from Maryland to Florida and Alabama, and occurring also in Iowa, Oklahoma, California and Oregon. The fungus enters through the small roots and plugs the vascular system, causing the sudden wilting and death of the plant. It can pass its whole existence as a saprophyte, and does remain in soil and in farmyard manure piles for ten years or more, yet it is an active parasite of the watermelon, attacking plants growing under most favorable conditions. It is not a damping-off fungus, nor a wound parasite, but is highly specialized as to host plants, attacking only watermelons, while other forms morphologically indistinguishable occur in the same area as specialized parasites of cotton and cowpea.

It is believed that this specialization accounts for the successful production of resistant strains, and that similar results would be more difficult of attainment in the case of diseases caused by fungi capable of attacking several hosts.

None of more than a hundred American and Russian varieties of melons tested proved resistant. The inedible citron or stock melon appeared to be immune, and was crossed with the watermelon in the hope of obtaining a resistant hybrid. A resistant strain of good quality developed in the third generation from this cross, and has become practically fixed after three years cultivation in isolated fields.

The Problems of Vegetable Teratology: Dr. J. A. HARRIS, Missouri Botanical Garden.

The Significance of Latency: Dr. GEORGE H. SHULL, Carnegie Institution.

Paper to be published in full in SCIENCE.

The Organization of the Ecological Investigation of the Physiological Life Histories of Plants: Professor W. F. GANONG, Smith College.

The paper calls attention to the changing conception of ecology, which is ceasing to be a search for utilities and is becoming an analysis of meanings. While in broad, general, or generic features, adaptation in the old causative or historical sense does exist, in details of structure and habit it is rare if not wanting, and most so-called adaptation is simply coincidence or toleration. In physiognomic ecology, therefore, the best working hypothesis is the assumption that the plant is an aggregate of physical needs which match or overlap the physical conditions presented by the environment, while the completeness of the overlapping determines the perfection of the 'adaptation.' The study of the physics of environments has made much greater progress than the study of the physical demands of plants, and the latter now offers the most important and attractive field for ecological investigation. The paper then discusses the methods of such study, and a classification of the physiological processes for purposes of investigation, through the four critical periods of the plant's life history, germination, orientation of seedling, spread of adult, and fruiting or sporification.

The Vegetation of the Blue Mountains of Jamaica: Dr. Forrest Shreve, Woman's College, Baltimore.

The Blue Mountains of Jamaica above 5,000 feet altitude in the neighborhood of Cinchona, the Tropical Station of the New York Botanical Garden, are covered by evergreen broad-leaved forest. Floristically the area is related to the surrounding tropical lowlands and to Eastern North America. In vegetative characteristics the forest likewise shows a blending of tropical and temperate features. The climate is one of much rainfall and high humidity. Alpine influences are but weakly operative at the highest altitudes. Local differences in the vegetation can be correlated with the topography and its determination of several of the physical factors. Bryophytes and Pteridophytes, as well as epiphytes, are abundant. There is a marked difference in humidity, light, wind and other factors between the forest floor and the forest canopy, with a corresponding contrast between the hygrophilous character of the terrestrial herbaceous vegetation and lower epiphytes, and the xerophilous character of the higher epiphytes and the foliage of the trees. The winter is relatively a season of rest in leafing and flowering, the spring and summer the seasons of greatest activity. Growth of leaves and shoots is extremely slow even in the most rapid growing forms. Transpiration is high under favorable conditions, but shows a high degree of sensitiveness to changes in temperature and humidity, and under the climatic conditions commonly prevailing is very low.

Cultures of Uredineæ in 1906: Professor J. C. Arthur, Purdue University.

The experimental study in the life history of various species of plant-rusts, of which this paper is a report, has been in progress during the last eight years. It embraces rusts of economic importance, and others as well. The most notable result of the present year was with flax rust, which is very destructive in the flax fields of the northwest. It was found that this rust, unlike most of its near relatives, produces all its stages upon the flax plant, and that infection comes from the old straw and stubbles that have laid out of doors through the winter. The common rust on *Juncus tenuis* was found to be connected with the *Æcidium* on *Silphium*, and the *Uromyces* on *Scirpus fluviatilis*, with the *Æcidium* on *Cicuta maculata*. Considerable advance was made in separating the *Carex* rusts; and a number of *Leptopuccinia* were also grown. The work was aided by a grant from the society, and was in charge of Dr. E. W. Olive.

Peridermium acicolum the Æcial Stage of Coleosporium Solidaginis: Dr. G. P. Clinton, Connecticut Agricultural Experiment Station.

Peridermium acicolum was found abundant on *Pinus rigida* at South Manchester, Conn., during the spring of 1906. It has been found in four different places in Connecticut—but has been reported only four or five times outside of the state, its distribution so far being confined to a small area extending from Massachusetts to New Jersey. After considering the synonomy in detail, the writer follows Arthur and Kern in calling the rust *Peridermium acicolum* (Und. & Earle). From observations made at South Manchester, verified by an infection experiment, this fungus was shown to be the æcial stage of *Coleosporium Solidaginis* (Schw.) Thm., which is common throughout the United States on goldenrod and asters. This conclusion is based upon the following points: (1) No other suspicious rust followed the attack of the *Peridermium.* (2) The *Coleosporium* (both II. and III.) occurred abundantly on *Solidago rugosa* under trees having the *Peridermium* but not under trees free from it and slightly removed. Instances were found where infected pine leaves interlocked with plants of *Solidago* and in these cases the leaves of the *Solidago* were badly peppered with the uredinial sori of the *Coleosporium.* (3) The time sequence of the two rusts was just as it should be if they were related. (4) An indoor infection experiment with spores of the *Peridermium* on a plant of *Solidago rugosa* was successful in producing the *Coleosporium.*

Culture Studies on the Polymorphism of Basidiomycetes: Dr. Geo. R. Lyman, Dartmouth College.

The life histories of about seventy-five species of Thelephoraceæ, Hydnaceæ and Polyporaceæ were studied in pure cultures with especial reference to the occurrence of secondary methods of reproduction. About 40 per cent. of the species studied showed polymorphism of some form.

Oidia were found in fully one half of the species of Polyporaceæ studied, but not in the other two families. Chlamydospores of the ordinary type were found upon the mycelia in all three families and in about one fifth of the species. Secondary methods of reproduction of a higher order were found in six species as follows:

1. *Michenera Artocreas* B. & C. was shown to be the imperfect form of *Corticium subgiganteum* Berk. The spores of *Michenera* are highly specialized chlamydospores which form a definite fructification of their own with a well-defined hymenium.

2. *Corticium alutaceum* (Schrad.) Bres., has two methods of secondary reproduction: (*a*) conidia of a simple oidium-like nature produced on the young mycelium; and, (*b*) red-brown spore-balls or bulbils of the *Helicosporangium* type produced in great profusion on the mature mycelium.

3. *Peniophora candida,* n. sp., is the perfect form of the well-known *Ægerita candida* Pers., and commonly occurs with it.

4. *Corticium roseo-pallens* Burt, produces conidia in great abundance on the mycelium. The conidia closely resemble the basidiospores and are produced successively until groups of two to ten are formed on low elevations on the sides of the hyphæ.

5. *Corticium effuscatum* C. & E., produces mycelial conidia of the *Œdocephalum* type in all cultures, thus recalling Brefeld's *Heterobasidion annosum.* The mycelium also produces abundant chlamydospores.

6. *Lentodium squamulosum* Morg., the only fleshy fungus cultivated, bears helicoid conidia upon long attenuated hairs arising from the young veil and from the margin of the developing pileus. The principal interest attaching to this species, however, lies in the structure and method of development of the basidiosporic fructification. The stipe and pileus are those of an agaric, but the hymenial region is occupied by a thick layer of irregular tubes and chambers whose external openings are more or less completely closed by a white flocculent veil. Diffusely spreading hyphæ arising from the trama form this veil, and by influencing the direction of growth of the hymenial plates, cause the porose-cellular character of the hymenial layer. The writer believes that Lentodium is not a monstrosity, as has frequently been held, but is an autonomous species whose sys-

tematic position is between the Agaricaceæ and the Polyporaceæ.

Ascigerous Forms of Glœosporium and Colletotrichum: C. L. Shear and Anna K. Wood, Bureau of Plant Industry.

Stoneman, Clinton, Spaulding, von Schrenk and Sheldon have already given the life histories of a number of forms. Klebahn has also reported ascigerous stages of two species which have been referred to *Glœosporium,* but which are evidently not congeneric with the organisms studied by the other authors mentioned, and by the present writers.

Forms from eight different hosts have been grown in pure cultures and both conidial and ascigerous perithecia produced. The forms studied occurred upon the following hosts: grape (*Vitis sp.*), apple (*Pyrus malum*), cranberry (*Vaccinium macrocarpum*), rubber plant (*Ficus elastica*), locust (*Gleditschia triacanthus*), *Ginkgo biloba,* cotton (*Gossypium sp.*) and bean (*Phaseolus vulgaris*).

The form on the apple has been grown several times before. In none of the other cases mentioned has the ascigerous form been heretofore produced, as far as known. The forms investigated can not be specifically segregated by morphological characters, and for the present are regarded as varieties of a single species. The ascigerous form has been found upon its host under natural conditions in only two cases, viz., on the apple and *Ficus elastica.* The presence of paraphyses has been reported by Sheldon. Organs sometimes occur which, if not aborted or mal-formed asci, may be called periphyses. The factors which determine the production of the ascigerous fructifications are still doubtful. Having once obtained a race or individual which produces asci, it can be successfully grown on various media and under various conditions. The mycelium, having entered the tissues of its host, has the power of remaining dormant for an indefinite period.

A New Chrysanthemum Disease—The Ray Blight: Professor F. L. Stevens, North Carolina College of Agriculture and Mechanic Arts.

Specimens of this disease were first received from Fayetteville, N. C. It was later found in Raleigh. Its most conspicuous appearance is as a blight of the ray flowers of the head. It also occurs in the stems. Examination showed a fungus constantly present in the diseased part. This was repeatedly isolated by plate culture; its culture characters were studied on various media; and its temperature and acid relation were determined. Inoculations were then made upon chrysanthemums under various conditions, producing typical cases of disease. The organism was recovered from these artificially inoculated blossoms and proved identical with that found in cases of natural infection. The fungus belongs to the genus *Ascochyta,* apparently a new species for which a technical description is provided.

A Potato Leaf-blotch Fungus New to America: Professor L. R. Jones, University of Vermont.

The fungus in question is *Cercospora concors,* first described by Dr. Robert Caspary in 1854, from collections made in the vicinity of Berlin. Since that date it has been observed with increasing frequency in Europe, proving most serious in the northern sections. The author has found it in Vermont at three well-separated stations and in three seasons, the first collection having been made in 1902. It has occurred in each case in old gardens and the indications are that it is a well-established parasite on the cultivated potato and probably widely distributed in the longer settled parts of the northeastern states and

Canada. Its attacks are confined to the leaves. It develops at the same season and under generally similar conditions to the well-known early and late blight fungi (*Alternaria solani* and *Phytophthora infestans*) and bears so close a resemblance in gross appearance to these that it has probably been commonly overlooked or confused with these diseases. Its cultural characters have been studied in detail. On artificial media it produces only a resting form of chlamydospore, similar to that produced in dead potato leaves. It is believed that the usual spraying methods will hold it in check. Certain varieties of potatoes show well marked resistance to this fungus. A detailed account of these studies will appear in the forthcoming (1906) report of the Vermont Experiment Station.

A Bibliography of North American Lichenology: Professor Bruce Fink, Miami University.

The paper attempts to give all titles pertaining to North American lichenology, from the first certain statement regarding our lichens, about the beginning of the eighteenth century, to the present time, completing a preliminary announcement concerning the bibliography made in 'Two Centuries of North American Lichenology,' *Proc. Ia. Acad. Sci.*, 1–38, 1904. With each title appear explanatory notes as to contents.

American Fossil Mosses, with Description of a New Species from Florissant, Colo.: Elizabeth G. Britton and Dr. Arthur Hollick, New York Botanical Garden.

During the summer of 1906 Professor Theo. D. A. Cockerell and his wife made extensive collections of fossil plants in the well-known Tertiary beds at Florissant, Colorado. From among the specimens collected a beautifully preserved fossil moss, in fruit, was kindly transmitted to us for examination. It will shortly be described and published under the name *Glyphomitrium Cockerelleæ.* The specimen is here for examination.

Thousands of specimens of fossil plants have been obtained from this locality by other collectors from time to time, but only three which have been regarded as mosses have been heretofore brought to light, and none of these is in fruit.

This class of plants is exceedingly rare in the fossil state and all of the hitherto described American species are sterile, the generic determinations having been based entirely upon leaf characteristics, so that such determinations were neither conclusive nor satisfactory and in some instances even their reference to the mosses is questionable. The specimen before us may therefore be said to be the first one from America in which a positive identification has been possible.

Some Changes in Wood Fiber immersed in Water: Dr. H. von Schrenk, Missouri Botanical Garden.

Recent Identifications of Cretaceous Gymnosperms from Kreischerville, N. Y.: Dr. Arthur Hollick, N. Y. Botanical Garden, and Professor Edward C. Jeffrey, Harvard University.

One of the best founded adverse criticisms in paleobotanical work is that determinations of generic and family relationships of fossil plants are necessarily based almost exclusively upon external characters. This has undoubtedly been the case in regard to the determination of fossil leaves, both of angiosperms and of gymnosperms, and it will continue to be inevitable as long as only the impressions of the leaves are available for study. If, however, certain of the leaves or leaf impressions are found associated or actually connected with twigs, or branches, or cones, or other parts in which the plant tissue is sufficiently well preserved for sectioning and microscopic examination, the ordinal, or family, or even exact generic relationships may be accurately determined.

Such conditions of preservation have been found in the Cretaceous deposits at Kreischerville, on Staten Island, and a large amount of material from this locality has been collected and subjected to critical examination. Some of the preliminary results thus obtained form the basis of this communication.

The lantern slides show microscopic enlargements of cones of *Protodammara*, sections of the wood of *Brachyphyllum* and other gymnosperms, demonstrating their Araucarineous affinities, and sections of *Pityoreylon* with resin canals, indicating the probable sources of the amber which is abundant in the deposits.

Some Vestigial Characters in the Cone of Pines: Professor E. C. JEFFREY, Harvard University.

Classification of the Genus Panicum: A. S. HITCHCOCK, U. S. Dept. of Agriculture.

In the comprehensive works of Bentham and Hooker (Gen. Pl.) and Engler and Prantl (Pl. Fam.) the large genus *Panicum* was divided into a number of sections, such as *Digitaria, Trichachne, Thrasya, Echinochloa, Hymenachne, Ptychophyllum* and *Eupanicum*. Most of these sections may more conveniently be considered as genera, this division being based upon well-marked characters of both structure and habit. Retaining the name *Panicum* for most of what has been included in the section *Eupanicum,* this genus may again be divided into groups, of which the following are American: *Ramileta, Fasciculata, Prostrata, Agrostoidia, Laxa, Maxima, Brevifolia, Verrucosa, Capillaria, Prolifera, Dichotoma, Parvifolia, Virgata, Diffusa, Divaricata* [Sect. Lasiacis]. These names should not be considered as sectional names. They are merely group names formed from a well-known specific name of each group. *Ptychophyllum* and *Lasiacis* should probably be assigned generic rank. The group *Dichotoma,* which includes about one hundred closely allied species of the southeastern United States, can again be divided into a number of subgroups, the classification being based upon habit, size and pubescence of skelatils, ligner, pubescence of culms, sheaths and blades, and the manner of branching of the fall culms. These groups are: *Depauperata, Laxiflora, Angustifolia, Eudichotoma, Nitida, Lanuginosa, Unciphylla, Eusifolia, Sphærocarpa, Corurnutata, Lancearia, Oligosanthia, Scoparia, Latifolia.*

DUNCAN S. JOHNSON,
Secretary

THE JOHNS HOPKINS UNIVERSITY

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

MEETING OF SECTION E—GEOLOGY AND GEOGRAPHY

ON account of the special meeting of the association at Ithaca in July, 1906, at which Section E had a full program of papers and excursions, no effort was made to get papers for the New York meeting of the association. Some fifteen papers, however, were spontaneously offered in addition to the vice-presidential address, and Section E held four sessions during the New York meeting.

At the session for organization held at Schermerhorn Hall, Columbia University, directly after the adjournment of the first general session of the association on December 27, Professor J. B. Woodworth, of Harvard University, was elected a member of the sectional committee for the term of five years, Professor N. M. Fenneman was elected a member of the general committee, and Professor William H. Hobbs a member of the council of the association. Professor Fenneman was also elected press secretary.

The section, some sixty members being present, passed, with unanimity, a resolution recommending to the council of the association that the annual meeting of the association be held regularly during the summer vacation season. The section also passed a resolution recommending the formation of a seismological committee composed of fellows of the association from the different sections interested in the subject.

At 2 P.M. of the same day Professor William North Rice read his address as retiring vice-president and chairman, his subject being 'The Contributions of America to Geology' (SCIENCE, February 1, 1907). At 3 o'clock the section adjourned in favor of the Geological Society of America, which held its first business session in the same room. The Geological Society continued its sessions on Friday and Saturday in the American Museum of Natural History, and on Monday, likewise at the Museum, Section E resumed its meeting. Two sessions were held, at which the papers described below were read and actively discussed. The attendance at these sessions was highly satisfactory, especially in view of the lateness of the date and of the fact that the Association of American Geographers was holding simultaneous sessions in a neighboring building.

At the business meeting of the sectional committee held on Monday, December 31, Professor Joseph P. Iddings, of Chicago University, was nominated for the office of vice-president of the association and chairman of the section. The resignation of E. O. Hovey as secretary was presented and accepted, and Dr. F. P. Gulliver, of Norwich, Conn., was elected to fill the vacancy for one year thus caused.

The papers, abstracts of which follow, were read in full by their authors on Monday, December 31, at the American Museum of Natural History.

The Detrital Flanking Slopes of the Mountains of the Southwest: WILLIAM P. BLAKE, New Haven, Conn.

This paper described the long regular slopes of coarse sand, gravel and boulders bordering high rocky elevations of the mountains of the southwestern portion of the United States, particularly of the Great Basin and the arid region of Arizona, and discussed their origin, age and geological relations. Evidence was given that the detritus of mountain gorges was formerly deposited under water and was spread out by oceanic action so as to form the long smooth flanking slopes with their highest portions from 3,500 to 4,000 feet and the elevation of the country to an equal amount. The grade in slopes of from eighteen to twenty miles in length averages 100 feet to the mile. They have great antiquity, being probably preglacial, and certainly older than the existing drainage, by which they have in part been modified in form and partly destroyed. The study of these slopes throws much light upon problems of Quaternary geology and meteorology.

Professor Blake's paper was discussed by Professors Kemp, Ogilvie and Gill.

Perspective View of the Submarine Canyon of the Hudson River: J. W. SPENCER, Washington, D. C. (Abstract not received.)

Eurypterus Fauna of the Shawangunk Grit: JOHN M. CLARKE, Albany, N. Y.

The Shawangunk grit throughout its extent along its western ridge from Ulster County into the Kittatinny Mountains of New Jersey and on its eastern from Skunnemunk Mountain, Orange County, to Green Pond, New Jersey, had never furnished fossils until the work of the past season brought them to light. In some of the Orange County exposures it has been found that above the basal conglomerate of the formation through the grit layers for a thickness of about 600 feet there are frequent repetitions of thin, black shale layers, inconstant in extent along the outcrops and in number and most of them bearing the remains of merostome crustaceans of the genera *Eurypterus, Pterygotus, Hughmilleria* and their allies. The fauna must have been an extensive one, as the remains are various and abundant, but the preservation leaves much to be desired, especially in the case of the larger crustaceans, whose surface has afforded opportunity for shearing and consequent deformation or destruction of the part. Yet in some respects the preservation has been remarkably favorable for small individuals, and these shales have afforded the most diminutive examples of these interesting creatures yet brought to light. The presence here of the genus *Hughmilleria,* heretofore known only in the Pittsford shale at the base of the Salina series in Monroe County, is sufficient evidence of the contemporary age of this arenaceous mass. In themselves the fossils are extremely interesting, affording some details of ontogeny not before recorded for these ancient merostomes. It is entirely evident, in the author's opinion, that these crustacean faunules running through the strata for so great a thickness indicate temporary and very changeable brackish water pools over the surface of a rapidly accumulating delta derived from the drainage of the high, folded lands to the northeast, and the deposit laid down in an embayment entirely separated from the salt pans and Dead Sea conditions of central and eastern New York by a barrier lying approximately in the present position of the Helderberg Mountains.

Dr. Clarke's paper was discussed by Professors Grabau, Lane, Ami and Clarke.

Kentucky Rock Asphalt for Common Highways: Malcolm Hart Crump, Bowling Green, Ky.

This paper treated Kentucky rock asphalt as to its composition, analysis and probable origin and discussed its geological horizon. The location and amount of available material and its economic uses as a water-proof and dust-proof road-surfacing substance were considered, and experiments as to its use were described. Its durability as shown by tests made on the streets of Bowling Green, Ky., and the results of a recent preliminary inspection by the Office of Public Roads, U. S., were given. The present demand for such material was stated and the cost of mining, crushing and grinding the Kentucky rock ready for use was detailed.

Professor Crump's paper was discussed by Professors A. M. Miller, Ami and Sternberg.

Portheus molossus, Cope, and Other Fishes from the Kansas Chalk: Charles H. Sternberg, Lawrence, Kansas.

This communication described a specimen of *Portheus molossus,* Cope. The paper was illustrated by a photograph in which were shown the tail fins, the upper one 28 inches long, 37 continuous vertebræ with dorsal fin in position as are also the pelvic, so found for the first time after experience of 40 years, exploration of the fossil beds of the west. The author also called attention to the beautiful set of pectoral fins with connecting arches, each three feet long, armed with 60 teeth. The specimen is now the property of the British Museum.

Subaerial Erosion Cliffs and Talus in the Lower Devonic of Michigan: A. W. Grabau, Columbia University.

Cliffs fronted by talus heaps of huge limestone blocks were formed in the lower Devonic of Michigan. The cliffs consisted of the Marine calcilutytes and their débris was incorporated in the Dundee calcarenytes. The brecciated limestone of Mackinac Island is a characteristic portion of these early Devonic talus heaps.

Professor Grabau's paper was discussed by Professors Lane, Foerste, A. M. Miller and Foerste.

The Naples Fauna in Michigan: A. W. Grabau, Columbia University.

The occurrence of the strata at the top of the Traverse group with the Goniatite fauna of the Naples horizon was described.

Professor Grabau's paper was discussed by Professors Ami and Bell.

Types of Cross-Bedding and Their Stratigraphic Significance: A. W. Grabau, Columbia University.

Four types of cross-bedding, the subaqueous, or delta, type, the torrential type and the æolian type, were considered. The occurrence of the last two as evidence of continental origin of the formations in which they are found was discussed. The type of cross-bedding of the Pottsville conglomerate bears out the conclusions already reached from its overlap relations, namely, that this formation is of fluvial origin.

Professor Grabau's paper was discussed by Professors A. M. Miller, W. G. Smith, Fairchild, Grabau, Lane, Ami, Fairchild, Grabau, Lane, Hovey, Woodman and Grabau.

The Cumberland Coal Basin, Nova Scotia: J. Edmund Woodman, Dalhousie Univ., Halifax, N. S.

The paper described the Carboniferous and Permian rocks of this basin, especially with reference to the source of material, and the great overlap produced by the New Glasgow conglomerate. From the structure and lithology of the basin, the following conclusions are reached:

(1) That the sediments were laid upon a differentially sinking floor, the shore line a variable one against the Cobequid range to the south, the detritus of the sediments being directly derived from these mountains; (2) conditions favorable to the formation of coal existed over a large part of the area bounded on the north by the outcrop of the Joggins zone of seams, on the east by the Springhill district, on the south by the Cobequid mountains, and on the west by a line down the middle of Chiquecto bay; (3) these conditions became adverse shoreward, because of the violence of wave action, and seaward, through deepening of the water; (4) through the differential sinking of the bottom and shore, beginning during the time of the coal measures and continuing into the Permian, slight in amount in the east and increasing westward, an overlap of the Permian upon the Cobequids has been established, burying the coal horizons over all the southern part of the basin except the east end; (5) the horizons containing the Joggins and Springhill seams extend under the basin, blanketing at a considerable depth, and rising westward, outcropping under the center of Chiquecto bay on the west; (6) this horizon swings eastward at the north, appearing on the shore at the Joggins, and eastward at the south, plunging beneath the Permian overlap before reaching the shore, hence covered at all points on the south side of the basin; (7) it is probable that coal occupies this horizon throughout this area, as indicated by the persistence of the Joggins zone of seams, the abundance of the Springhill coal, and the occurrence of a thick seam at 2,250 feet in a borehole at Fullerton lake, less than half-way from the shore of Chiquecto bay to Springhill; (8) the summit of the Coal Measure should lie against

the Cobequid granite beneath a point slightly south of Apple River; (9) the Apple River borehole and the second hole at Fullerton lake indicate that the fine sediments of the Coal Measures are replaced shoreward by coarse detritus, continuous upward with the New Glasgow conglomerate of the Permian, so that coal, if present in the main portion of the western half of the basin, frays out southwards; (10) that the strata on the shore of Chiquecto bay are such as bring the Coal Measures 1,400 feet nearer the surface than at the first Fullerton Lake borehole, and over 2,000 feet nearer than on the floor of the basin north of Fullerton Lake.

Professor Woodman's paper was discussed by Professors Grabau, Woodman, A. M. Miller, Lane and Woodman.

Charles Willson Peale's Painting. 'The Exhuming of the First American Mastodon': ARTHUR BARNEVELD BIBBINS, Baltimore, Md.

Charles Willson Peale, who was born in Chestertown, Maryland, in 1741, and known as 'the artist of the Revolution' was among the first to interest himself in American vertebrate paleontology. Although very few of his hundreds of paintings deal with this subject, one has lately come to light which vividly portrays his keen and practical interest in this direction. This is a canvas six by five feet, painted in 1823 a few years before the artist's death. It is in a good state of preservation and owned by a direct descendant of Peale. The subject is 'The Exhuming of the First American Mastodon.' Peale is represented as personally supervising the excavation, with other scientific worthies of the day and some members of his family in attendance. Although the figures are small the detail is so perfect that the several personages shown are readily recognizable. An elaborate and ingenious device for ridding the excavation of water is a notable feature.

Family history has it that the locality was somewhere in Delaware or New Jersey, that the skeleton was first erected in Philadelphia, that the discovery was celebrated by a dinner held beneath it and that the building containing it was ultimately burned. Family history also relates that Peale was induced to undertake the exhuming of the mastodon by Baron von Humboldt, who had visited this country a few years earlier and was the artist's guest.

The Peale family was also among the first in America to encourage the establishment of natural-history museums, for among the records we find that while Humboldt was visiting Charles Willson Peale and while these gentlemen were entertained at a formal 'three-o'clock dinner' by President Monroe, the guests improved their opportunity by asking the president to endeavor to induce Congress to establish a National Museum; also that Peale returned from the interview much elated by the assurances that action would shortly be taken.

In 1813 the artist's son, Rembrandt, started the erection of a Natural History Museum in Baltimore, having previously excited interest in such matters by the exhibition of the skeleton of a mammoth. This building, later known as the 'Old City Hall,' is still standing, and bears the original legend 'Peale's Museum.'

Additional Evidence of Tropical Climate on the Middle Atlantic Coast during the Lower Cretaceous: ARTHUR BARNEVELD BIBBINS, Baltimore, Md.

No better evidence of tropical or subtropical climate is needed than the existence of dinosaurs as a dominant faunal element, if it be presupposed that reptiles as a class have always been as partial to such climate as they are in our own time.

On the same principle, tropical climate is implied by the domination of the flora of a period by such plants as the cycads; and at least subtropical conditions by such conifers as the Sequoia. All of these warm climate representatives have long been known to have prevailed on the Middle Atlantic Coast during Middle Secondary or Lower Cretaceous time; but during the past year another tropical group has suddenly come to light, viz.: the palms. Mr. E. W. Berry appears to have been the first to detect them in a somewhat doubtful fragment of a frond from the Magothy formation or the Upper Chesapeake Bay. Shortly after, there came to the writer's notice some half dozen fragments of different silicified palm trunks, chiefly from a single neighborhood in the Lower Cretaceous belt, between Baltimore and Washington, suggesting that these fossils are likely to prove, upon systematic search, in other Lower Cretaceous areas to be of scarcely less frequent occurrence than the silicified sequoia and cycad trunks of those areas, thus greatly emphasizing the evidence of the tropical climate of their time.

Geology of Core Bank: Collier Cobb, Chapel Hill, N. C.

The coast from Hatteras southward is rising, not subsiding. As the dunes advance toward the Sound side they depress by their weight the swamp muck in which the trees of that side grow, and these are left exposed on the seaward side when the dunes have passed. This compression of the muck is often mistaken for subsidence of the land. On the land opposite the Core bank successive strata of muck filled with well-rounded blown-sands rise twenty feet above Core Sound at Atlantic. Kitchen-middens, too, mark this line of elevated dunes.

Drum Inlet was opened by a storm on October 17, 1906, and Tertiary shell-rock thrown upon the bank. Numerous Cretaceous fossils, such as the author has already reported from Currituck Bank were found along the entire length of Core Bank, which dates back to Cretaceous time at least. Whalebone Inlet between Core Bank and Portsmouth Island has again been closed.

There is thus no longer any question as to the origin of Core Bank or of Currituck Bank, for they are both essentially parts of the mainland. Currituck Sound was formerly a river that flowed into the old Albemarle or Caroline River before the present Albemarle Sound was formed by the drowning of that valley; and Core Sound was for the greater part of its length a southern tributary of the large river made up of the Pamlico and the Neuse and passing to seaward through the present Ocracoke Inlet. The Albemarle River passed through the present fresh ponds just south of the Kill Devil Hills, and the margin of the continent was some three score miles eastward of its present position.

The following papers were read by title:

The Low-Water Channel of the Mississippi River: Robert Marshall Brown.

Walnut Canyon, Arizona, Section compared With Rocks of Similar Age in the Territory: H. W. Shimer.

Structural Control of Surface Features in the Highlands of the Hudson: Charles P. Berkey.

The Occurrence of Diamonds in North America: George Frederick Kunz.

A Lower-Middle Cambrian Transition Fauna From Braintree, Mass.: H. W. Shimer.

Notes on the Upper Aubrey of Northwestern Arizona: H. W. Shimer.

Edmund Otis Hovey,
Secretary

SCIENTIFIC BOOKS

Lectures on the Theory of Functions of Real Variables. Volume I. By James Pierpont. Boston, Ginn and Company. Pp. xii + 560.

A considerable part of the present volume is in very close touch with problems which confront the students of elementary mathematics, dealing with such questions as the difference between rational and irrational numbers, the theory of limits and the concepts of continuity and discontinuity. Bright and thoughtful students frequently seek more light on these subjects than they can find in the elementary text-books and many teachers will doubtless rejoice to find that a large amount of most interesting information along these lines has been made accessible by a scholar in whom they can have the utmost confidence.

A little more than a hundred pages are devoted to the fundamental matters which are to serve as a basis for the notion of function in general. This notion is illustrated by means of the trigonometric functions with which the reader is supposed to be familiar and a very brief proof is given of the interesting fact that these functions are transcendental. The descriptive introduction to functions is followed by a similar introduction to point aggregates in which several fundamental theorems relating to limiting points are proved and a number of the common terms are defined and illustrated. The theory of point aggregates furnishes some of the most interesting instances of the distinction between finite and infinite multitudes, and the importance of this theory is partially illustrated by the fact that one of its terms (dense) is needed as early as page 20 to describe the system of rational numbers.

The greater part of the present volume deals with questions which the student approaches in the elementary calculus. The processes of differentiation and integration are treated with a completeness which seems impracticable in a first course, yet this completeness is essential for a thorough comprehension of the subject. A very helpful feature is furnished by the 'numerous examples of incorrect forms of reasoning currently found in standard works on calculus.' It has been "the author's experience that nothing stimulates the student's critical sense so powerfully as to ask him to detect the flaws in a piece of reasoning which at an earlier stage of his training he considered correct."

The vast extent of the applications of the processes of calculus have frequently led writers to overlook the regions where these processes do not always lead to correct results. Even some of the most useful formulas, such as

$$dy/dt = dy/dx \cdot dx/dt,$$

appear in nearly all, if not in all, of the other English texts with an incomplete demonstration. Arts. 378–80, which are devoted to a satisfactory demonstration of this formula, exhibit also the missing elements in the common demonstrations and suggest a method for an elementary demonstration in case a function has only a finite number of oscillations.

The last three chapters are devoted to improper integrals and to multiple proper integrals. These naturally contain much more original matter than those which precede. This is especially true of the last chapter, which is practically an original contribution. The definition of an integral is taken in the most general fashion and includes all the possible fields, whereas until then the most general was Jordan's and this is restricted to the inner points of a field. No other work contains such a complete treatment of the subject of uniform convergence as is found in these chapters.

The present volume, which is to be followed by another along the same lines, seems especially timely in view of the movement to employ the notion of function much more generally in the elementary courses in algebra and geometry. Teachers of secondary mathematics should have a clear understanding of the concept of function and we know of no other work where an accurate knowledge of this concept can be acquired as readily as from the earlier chapters and the criticisms of the present treatise. The fact that Ginn & Company should undertake the publication of such works as this and Goursat's 'Course in

Mathematical Analysis' is a very encouraging sign of the growing interest in higher mathematics and these works will doubtless do much towards increasing this interest. In following the pages of Professor Pierpont's work one feels that one is being led by a master of his subject and a sympathetic teacher, and these elements combined with the nature of the subject make the present work one of the most significant publications on pure mathematics that have ever appeared in this country.

G. A. Miller

University of Illinois

Electrical Nature of Matter and Radioactivity. By Harry C. Jones. New York, D. Van Nostrand Company. Pp. viii + 220. Price $2.

Another semi-popular book upon a well-worn subject, but a book which on the whole justifies its existence by the treatment, found in the last seventy-five pages, of the results of investigations and discussions so recent that they have not yet found place in other books on radioactivity. Thus the discussions of recent work on the origin and distribution of radium, of the properties of the α and β rays, as lately worked out by Rutherford and Bragg, of the 'radiobes' of Burke, of the decomposition products of actinium, and of radiothorium, are all new and all thoroughly commendable.

The book as a whole lacks somewhat in unity of treatment, the different sections differing considerably in value and in method of presentation. The treatment of radioactivity, which occupies all save the first third of the book, although it is non-mathematical, is on the whole thoroughly scientific, being characterized by an admirable moderation of statement, a scholarly collection of all the available experimental data, evidently from the original sources, and a judicious balancing of arguments for and against rival hypotheses. It will be read with interest and profit by physicists and chemists. It contains a commendably small amount of the sort of material which seems to be designed chiefly as food for the popular imagination.

The chapters dealing with the electrical nature of matter seem, on the other hand, to have been written largely for popular consumption and their faults are those most common to literature of this type, namely, incompleteness in the presentation of the facts and a rather immoderate haste in arriving at positive conclusions, the author's attitude being that of the ardent convert to the electrical-nature-of-matter *hypothesis* rather than that of the judicious disseminator of the present state of scientific knowledge in this field. Thus in discussing in the first chapter the value of e/m for the corpuscle, he slurs over the differences between the values found by different observers working with cathode rays, Lenard rays, photo-electric effects, the Zeeman effect, and radium rays, and says simply that the answer to the question as to the constancy of e/m for negative corpuscles is unmistakably given by the results which have been obtained. When it is remembered that these values vary for slow-moving corpuscles from 4×10^6 to more than four times that number, namely, 18.7×10^6, the statement appears rather too strong even for a popular article. Thus far these differences are certainly not to be explained by *probable* observational errors. It is to be hoped that further experimenting will soon reveal the causes of the discrepancies. The value of e/m which the author uses throughout the book is 7.7×10^6 instead of 18.7×10^6, the value given by the most reliable experiments, especially those of Seitz (*An. d. Phy.,* Vol. 8, p. 223), who succeeded in bringing the results obtainable by the three different methods used in the study of cathode rays into close accord. The value 7.7×10^6 is, of course, inconsistent with Kaufmann's measurements upon the variation of e/m with speed according to which this quantity changed from 6×10^6 to 13.1×10^6 as the speed varied from .94 to .7 that of light.

The feature of this part of the book, however, which is least commendable is the confusion either of ideas or of terms involved in such statements as the following: "Matter is then a pure 'hypothesis'—'there is not the least evidence for its existence.' Energy is the only reality." Now, of course, every trained reader knows that in the ultimate analysis of things there is nothing in the universe which is not hypothetical to any particular individual except the fact of his own consciousness. But the ordinary reader will scarcely understand that in the above statements the author is merely denying the existence of matter in the broad, metaphysical sense in which the philosopher denies the existence of any external world whatever. He will rather understand him to be using language in the sense in which it is commonly used in books on physical subjects, and to be tacitly assuming the existence of an external world and yet denying the existence of matter as a constituent of that world; and indeed this is certainly what he does do, since in the next sentence we find him asserting the reality of energy.

Such assertions seem to me to be particularly fruitful of confusion of thought in the minds of the untrained, while to the trained they are devoid of all meaning. For *matter* 'as we ordinarily understand the term' does not involve any particular hypothesis as to the inner nature of the atom. As commonly understood, matter is merely that something which possesses the properties of weight and inertia. Its existence is, therefore, just as real as the existence of these properties. As investigation goes on the more properties which we find ourselves agreed in associating with weight and inertia the more definite does our idea of matter become. Thus there is now practical unanimity in regarding matter as composed of discrete particles, and recently some evidence has appeared which makes it plausible at least to endow the discrete particles with an electrical property as well as with weight and inertia, and it has also been suggested that the inertia property may be entirely wrapped up in the electrical property. If further experimenting should justify this hypothesis the term matter would lose none of its present significance, but would rather gain additional meaning, just as the term 'light' gained rather than lost in significance when Maxwell and Hertz discovered a relation between light and electricity. The assertion that light 'is a pure hypothesis, that there is not the least evidence for its existence,' would be in every respect as warrantable as the similar assertion regarding matter. Either assertion, I take it, is completely misleading in popular writing, even though there may be some technical justification for it.

But I can see no sort of justification, technical or otherwise, in denying the existence of matter and in the same breath asserting that 'energy is the only reality'; for, since energy is *defined* only in terms of matter and motion, it is obviously absurd to consider it any more real than matter. It is merely a case of the recrudescence of the confusion of ideas which Boltzmann and Planck eliminated to so large an extent from German thinking by their masterly articles on 'Energetik' which appeared in *Wiedemann's Annalen* in the winter of 1906. Of course, no one will deny that it might, perhaps, be possible to describe natural phenomena from some other view-point than that which has been adopted by the master minds of science from Galileo and Newton down to J. J. Thomson, and to start with a fundamental something which might be called energy instead of with the something which we now call matter, but this possibility, if it be a possibility, has certainly not yet been realized, and the attempts which have thus far been made in this direction have resulted only in a confused mass of logical contradictions, so that, in point of fact, energy, as the term is now used in scientific literature, is still defined in terms of matter, space and time. In view of the gross abuse which the word energy commonly receives at the hands of the unthinking, an abuse which is well illustrated by the effort which is sometimes made by high school teachers to 'get at everything,' as they say, from the standpoint of energy, even before their pupils have been taught enough mechanics to make a concise conception of the meaning of energy possible, in view, I say, of this popular abuse of the term it is particularly desirable that men of science do not add to the confusion by using it in a loose and indefinite sense.

R. A. Millikan

University of Chicago,
January 28, 1907

SCIENTIFIC JOURNALS AND ARTICLES

The American Museum Journal for January announces that in order to emphasize its news features it will henceforth be issued monthly from October to May, inclusive. The guide leaflets will not be included, but will be issued at intervals as occasion requires. The number notes 'A Zoological Expedition to New Mexico and Arizona,' describes 'The Skeleton of the Columbian Mammoth' recently placed on exhibition and gives its height at the shoulder as ten feet six inches. There are accounts of the three expeditions sent out last year by the department of vertebrate paleontology, and of the Selma meteorite, an aerolite weighing 310 pounds, which ranks it among the largest ten of this class of meteorites.

The Museum Gazette, of Haslemere, Eng., for January, contains among other interesting articles an extract from 'The General Guide to the Contents of the British Museum' published in 1762, which tells how admission was then obtained to that institution:

> Some of my readers may be ignorant of the Manner of applying to see the Museum; for their Information I shall add, that fifteen Persons are allowed to view it in one Company; the Time allotted is two Hours; and when any Number not exceeding fifteen are inclined to see it, they must send a List of their Christian and Sir-names, Additions, and Places of Abode, to the Porter's Lodge, in order to their being entered in the Book; in a few Days the respective Ticket will be made out, specifying the Day and Hour in which they are to come, which on being sent for, are delivered. If by any Accident some of the Parties are prevented from coming, it is proper they send their Tickets back to the Lodge, as no body can be admitted with it but themselves. It is to be remarked, that the fewer Names there are in a List, the sooner they are likely to be admitted to see it.

Under 'Museum Statistics' we learn that there are 330 museums and art galleries in the United Kingdom, situated in 225 cities, towns and villages. This seems a pretty good showing, but is evidently not so regarded by the writer. The attendance, outside the national institutions runs from 1,480,000 at the Glasgow Art Gallery to 'practically *nil*' at Frome. The best comparative attendance is at Ilkley where there were 4,000 visitors in a population of only 7,000.

Bird-Lore for January-February has for its frontispiece the bust of Audubon recently unveiled at the American Museum of Natural History and this is followed by the address on 'John James Audubon' delivered at the unveiling, by C. Hart Merriam. 'Florida Bird Notes,' by T. Gilbert Pearson, show the great increase of the pelicans. There is a good article on 'Bluebird Tenants,' by Marian E. Hubbard, and then comes 'Bird-Lore's Seventh Christmas Bird Census,' a most comprehensive series of observations. We have the first paper on 'The Migration of Thrushes.' F. A. Lucas and Thomas H. Montgomery discuss 'Oology as a Science' and 'The Question of the Amount of Science in Oology.' The Educational Leaflet, by Mabel Osgood Wright is devoted to the 'Bluebird.' The reports of Audubon societies from various parts of the country are very encouraging and show an increasing effort to protect the birds, with here and there a jarring note from some one who is quite willing to wipe them out of existence.

The Bulletin of the Charleston Museum for January contains the report of the director for 1906, which shows most encouraging progress in various lines. Dr. Rea is to be congratulated on his success in revivifying this, the oldest of our museums, and on his foresight and energy in hunting up the collections and preserving their records.

The Plant World for February will contain the first installment of a series of articles by Dr. Pehr Olsson-Seffer, who is now on a journey around the world with the special purpose of studying methods of tropical agriculture. There is also a paper by Dr. MacDougal upon field hybrids among oaks; and by Professor F. E. Lloyd on the diurnal flowering period of certain cacti. The issue contains, also, a large number of short items of general interest.

SOCIETIES AND ACADEMIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 626th regular meeting was held January 5, 1907, with President Hayford in the chair.

The first paper of the evening was presented by Mr. L. W. Austin, describing some recent developments of wireless telegraphy and giving a comparison of the efficiency of continuous and broken wave trains in wireless signaling.

The experiments were carried on at the wireless station at Brant Rock, Mass., during the past autumn, the distance between the two antennæ being about three miles. The continuous wave trains were produced by a small Fessenden high frequency dynamo which during the experiments was run at 50,000 cycles per second. The broken wave trains were of the same frequency and produced by a spark in the usual manner. The receiver used was of the electrolytic type. Below are given the data of one of the experiments:

	Machine	Spark	
Energy	50 watts	225 watts	
Current in sending antenna	1.6 amp.	0.9 amp.	
Strength of signal	5	20	in arbitrary units
Current in receiving antennæ	2.2	4.5	in arbitrary units

the loudness of the signal being proportional to the square of the received current. If we reduce the received current to terms of the same radiation current, the spark would give 3.6 times as strong received current as the machine. Reduced to terms of the same energy the two are nearly equal in efficiency, the advantage being slightly in favor of the machine.

Mr. O. B. French spoke of 'The Recent Use of Invar Tapes for the Measurement of Primary Bases.'

Since the discovery of the alloy of nickel and steel (called invar, from invariable) which possesses a very small coefficient of expansion, its use for precise measuring apparatus has been tested very carefully. Most of these investigations have been made under the direction of C. E. Guillaume, of the International Bureau of Weights and Measures at Paris. His experiments having proved the metal to be fairly stable, the Coast and Geodetic Survey decided to try it for the measurement of primary base lines.

In December, 1905, the survey purchased from J. Agar Baugh, London, England, several ribbons of the invar tapes, 6.3 mm. in width, 0.5 mm. in thickness and 53 meters in length, which were prepared for measuring tapes in the instrument division of the survey.

During 1906 the survey measured six base lines, using on each base three invar tapes, in daylight (standardized at the National Bureau of Standards) and also three steel tapes, at night (standardized in the field).

Several pieces of the invar tapes, tested at the Bureau of Standards, showed a tensile strength of 100,000 pounds per square inch (about one half that of steel tapes), with the elastic yield point about 70 per cent. of the tensile strength.

The tapes were tested for considerable ranges of temperature, reeled and unreeled a large number of times, and also tested for continued application and removal of light loads, without showing any change in length. The coefficient of expansion of the invar tapes was found to be .0000004 per degree centigrade or 1/28 that of steel.

The steel and invar measures of the six bases were computed independently. The differences between them are small, the largest being 1:300,000 (3 mm. per km.) and the average about 1:500,000.

The probable errors of the lengths of the bases from the steel measures are more than double those from the invar measures. The final probable errors of the bases, giving the invar double weight, are between 1:2,500,000, and 1:5,000,000 (0.4 mm. and 0.2 mm. per km.).

In 1900 the Coast and Geodetic Survey demonstrated that steel tapes gave practically the same accuracy as bar apparatus with one third of the cost. It is now shown that the invar tapes give results considerably more accurate and economical than the steel tapes.

Mr. W. P. White then made some informal remarks upon 'Suspended Galvanometer Supports,' stating that the more rapid movement of the Julius suspensions and similar supports, probably on account of the rapidity, does not perceptibly affect ordinary moving coil galvanometers; hence for such galvanometers placing at the center of gravity, etc., is usually unnecessary, and the supports for such galvanometers may be given great simplicity and wide variety.

The effectiveness of a suspended support can easily be shown to be less for slower movements of the building; hence for a double reason these slower motions constitute the chief difficulty with the moving coil galvanometer.

By hanging one support from another, each provided with its own damping arrangements, a considerable gain in efficiency can be shown theoretically. This has not yet been thoroughly tested experimentally.

R. L. Faris,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 27th annual and 422d regular meeting was held December 15, 1906. The following officers were elected for the ensuing year:

President—Leonhard Stejneger.

Vice-presidents—T. S. Palmer, W. P. Hay, E. L. Greene, E. W. Nelson.

Recording Secretary—M. C. Marsh.

Corresponding Secretary—W. H. Osgood.

Treasurer—Hugh M. Smith.

Councilors—A. D. Hopkins, J. N. Rose, A. K. Fisher, A. B. Baker, David White.

President Stejneger was nominated as a vice-president of the Washington Academy of Sciences.

The 423d regular meeting was held January 12, 1907, President Stejneger in the chair and forty persons present.

Mr. Maxon exhibited a nest made apparently by a mouse chiefly from horse hair. The nest was found upon the ground near Oneida, N. Y. Mr. Bailey said the locality of the specimen made it remarkable. The harvest mouse weaves nests of this type, using similar fine material, but Washington is the northern limit of distribution of the species. Dr. H. M. Smith noted the late autumnal flowering (November 10, 1906) of the bluet, *Houstonia cœrulea,* an early spring species. Mr. Morris observed on November 15 a late flowering of *Phlox subulata,* and Mr. Clark on January 6 saw the early flowers of the skunk cabbage and the latest of the witch hazel.

Dr. Smith commented on the death on December 16, 1906, of Captain Z. L. Tanner, retired, a naval officer prominent in marine research as commander for many years of the *Albatross* on both coasts, in which capacity he has rendered great service to science.

Mr. Van Deman exhibited specimens of the Grimes apple and remarked upon the superiority of this variety. Mr. Titcomb showed an interesting anomaly in a frog which had an additional pair of hind legs. A radiograph of the specimen was shown.

Mr. M. B. Waite presented the first paper of the evening on 'A New Peach Blight from California.' The speaker stated that this peach blight was not entirely new, it having been described by Beijerinck. Pierce, in his bulletin on peach leaf-curl noted the presence of a winter blight on the peach, which, he stated, 'is probably induced by a *Coryneum.*' He mentioned the gumming habit as similar to that of *Coryneum Beyerinckii.*

The disease has evidently been in California for several years, but during the last three or four years it has increased to alarming proportions in the great interior valley of California and in the adjacent smaller districts. Especially in the more humid sections has it seriously crippled the peach industry, cutting down the production of some of the most profitable orchards to less than one half, or even a quarter, of a crop.

While in California investigating pear-blight, the writer was appealed to by peach growers for assistance. When this disease was thus submitted to him, early in February, 1905, it was easy, on microscopic examination, to promptly identify it as produced by the gumming fungus of Beijerinck, *Coryneum Beyerinckii* Oud., the writer having become familiar with the disease in 1898 through specimens sent in from Clyde, Ohio. It was known to occur occasionally in other eastern peach-growing sections. It had not attracted attention, however, as a serious disease until this recent outbreak in California. The next day, after identifying the fungus, in the orchards at Suisun, Cal., the serious character of the outbreak of this disease became evident. All over the one-year twigs of the peach trees the small spots of the fungus were apparent. Many of the spots were necrotic and exuded gum at that early date. Scarcely an inch of the sound twig growth but carried one or more spots. Furthermore, the fungus had attacked the buds and killed and ruined by far the larger proportion of them, in many cases from 90 to 95 per cent. were already dead. On jarring the trees a shower of these dead buds fell to the ground, others were glued fast to the twigs by the gummy exudate. As the season advanced and the buds opened into blossoms the scarcity of the latter became more and more apparent. The spots on the twigs and the diseased buds exuded drops of gum which frequently ran down the twigs or dropped to the ground.

Spraying with Bordeaux mixture was at once suggested as a promising remedy, but the writer was informed that this had been tried and had proved to be a failure. Upon further questioning and an examination of the sprayed trees it was found that the treatment had been made some two weeks before, while the spots were in many cases older. The suggestion was then immediately made that spraying would have to be done in the fall or early winter, considerably ahead of the first appearance of the new infections.

This proved to be the key to the treatment of the disease. Three growers at Suisun, J. S. Brown, Geo. Reed and J. S. Chadbourne, sprayed blocks of trees in December, 1905, with Bordeaux mixture; in one case where the disease had been unusually severe the year before the trees had been sprayed a second time about January 15, 1906. Upon examining them late in March there was to be found scarcely a single diseased bud upon the sprayed trees, and where one was found, it was on a twig not reached by the spray. The contrast between the sprayed and unsprayed trees was very striking, the disease being even more severe in 1906 than in previous years. I was informed that the good results became even more noticeable as the fruit was harvested, immense crops of fine fruit being picked from the treated blocks, while adjacent orchards, often of the same variety, separated only by a wire fence, were practically failures. The crop of fruit in one case where the trees had been thoroughly treated, reached 42 tons from 100 trees, and in another case 400 trees yielded 100 tons of fruit, and similar productiveness occurred with other varieties.

The disease scarcely ever kills the tree, except possibly a young one, but it kills a great many of the branches, cripples the tree and ruins its productiveness.

Further experiments are in progress this year, beginning early in November, 1906, to test more definitely the most desirable dates for spraying, number of treatments, strength of Bordeaux mixture and the possibilities of using other sprays, especially the lime-sulphur preparation. This lime-sulphur spraying is done quite regularly in many California orchards, as well as in certain sections of the eastern states to prevent the San Jose scale. If this spray proves effective it may only be necessary to change the date from late winter or early spring to fall or early winter and thus prevent both the scale and the peach blight.

This *Coryneum* occurs not only on the peach, but is seriously injurious on the almond and apricot as well in California. It is known to be an important factor in most of the recent failures in the productiveness of these orchards. Experiments are also in progress in the treatment of the disease on these fruits.

In answer to a question, Mr. Waite said there was little doubt that the blight existed in Oregon and Washington, though it had not been definitely observed.

The second paper, by Mr. John W. Titcomb, was entitled 'Some Work of the Beaver.' While engaged in field work Mr. Titcomb visited Maskinonge County in the province of Quebec and during the month of June discovered on Lake Madam Henry a series of four freshly-built beaver dams. The upper one raised the lake some two and a half to three feet, overflowing quite a large area of lowland at the head of the lake where the beavers proceeded to cut down poplar trees, and two months later from these cuttings had constructed a house on one side of the lake midway between the overflowed land and the dam.

Views were shown of the dam, including one showing the methods of the Indians in trapping beavers (unlawfully), of the house as it appears from the lake and also from the shore, and of the house together with a pile of cuttings for the winter's supply. Several views were also shown of the work of the beaver in the overflowed land. A view was also presented of a beaver dam on another lake about 100 miles distant from Madam Henry which practically divided the lake into two parts, the water level of one half being raised considerably above that of the other half. Incidentally a view was shown of beavers at work, a picture taken in the daytime by Mr. W. E. Balch, which was awarded first prize by *Recreation*. The award was afterward withdrawn, because it was charged that the beavers had been killed and fastened into position before the photograph was taken.

In the discussion following Mr. Titcomb referred to the unreasoning methods of the beaver, citing their apparent inability to control the direction in which the tree should fall, this apparently depending on chance. Trees felled are often found unused and with the limbs uncut, owing to their unfavorable position. The beaver frequently cuts in two a stick it is dragging, in order to get it over a log, instead of going around. Dr. Hopkins related an instance occurring in Maine in which beavers had attacked a man-made dam which had backed water into their own works. They had confined their assaults to the braces, which were considerably gnawed, leaving untouched the posts and sills. A guard became necessary until the beavers gave up their attempt.

Dr. Evermann called attention to a study of a large number of beaver dams from an engineering standpoint by Mr. Edward R. Warren, of Colorado Springs. The general conclusion reached was that beavers show little engineering sense in their construction work.

Mr. Vernon Bailey offered the third paper, on 'The Mountain Haymakers or Pikas' (Ochotona), little animals related to both the rabbit and the guinea-pig, sometimes called cony, pika, little chief hare, maginty rabbit, or maginty. The paper was illustrated with lantern slides.

The ochotonas live among the rocks, high up in the mountains, mainly near timber-line, from New Mexico and California to Alaska, and while often abundant are comparatively little known. They are approximately of the size and form of the guinea-pig, with rounded ears, short legs, and no visible tail. Their call or alarm note is a nasal squeak somewhat resembling the bleat of a very young lamb.

During late summer and the short autumn these little animals are busy gathering their winter store of hay, including plants of many species that they cut and stack in dry places under the shelter of broken rocks that lie in masses on the steep mountain slopes. Often a bushel or more of well-cured vegetation is gathered into one of these sheltered deposits and a dozen or more stacks are sometimes found within the area of a not very extensive rock slide. Almost every plant within reach is gathered, with apparently little specific discrimination. In one place on the side of Pecos Baldy in New Mexico 34 species of plants were recognized in the hay, including 9 species of grass, a sedge, two species of clover, part of a large thistle, flowers and stems of the blue columbine, a purple *Pentstemon,* a little sour-dock, a saxifrage, a *Polygonum,* a larkspur, two species of *Potentilla,* a *Geum, Senicio, Erigeron, Wyethia, Aster, Achillea, Caltha, Veratrum, Geranium,* two umbellifers, a *Silene* and an *Aralia.* Many additional species of plants have been noted in other localities and the hay often contains numerous dried flowers and some berries.

Nothing is known of the habits of these animals in winter when they and their haystacks are buried deep under the snow except that in spring the haystacks are found reduced to a few dry sticks and stems and the ochotonas seem to have survived the arctic winter in good condition.

While too small to be counted as game, these little animals serve a worthy purpose in adding a feature of great interest to the upper slopes of the mountains.

In reply to a question, Mr. Bailey said the cony of the Bible was a *Hyrax*. Dr. Gill said the cony of old England was the rabbit and that the biblical scholars, mistakenly supposing the animal referred to was a rabbit, used the term cony in translating. The genus is now called *Procavia* instead of *Hyrax*. Both of the scientific names are also misapplications, the hyrax of the ancient Greeks being a shrew mouse and the biblical cony or daman being in no wise related to a *Cavia*. However, *Procavia* it must remain; the genus is the type of a very distinct family—Procaviidæ—as well as of a peculiar suborder.

M. C. MARSH,
Recording Secretary

THE ELISHA MITCHELL SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA

THE 169th meeting was held in the main lecture room of Chemistry Hall, Tuesday January 15, 7:30 P.M., with the following program:

PROFESSOR H. V. WILSON: 'The Regenerative Power of Sponges.'

PROFESSOR J. W. GORE: (1) 'Direct Current Transmission of Power,' (2) 'The Electrical Aging of Flour.'

A. S. WHEELER,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

THE GEOGRAPHIC BOARD OF CANADA

THE Geographic Board of Canada, organized in 1898 with aim, constitution and publications very like those of the older United States Board on Geographic Names, has just published its sixth report. As I have an interest in all matters pertaining to the geography of New Brunswick, I wish to make some comments upon the decisions of the board affecting that province.

The first duty for which the board was organized is to decide upon 'all questions concerning geographic names in the Dominion,' and its decisions up to the present are in the report before us. The great majority of these, so far as the province of New Brunswick is concerned, are admirable; but some of them, in my opinion, are quite indefensible. Thus, an important old English settlement in the province is called *Point de Bute,* sometimes printed *Pointe de Bute.* The board, called upon to decide between Point and Pointe, rejects the whole name and decides upon *Pont à Buot,* on the ground, as it has explained, that this is the original historic form of the name. Aside from the fact that this origin is only supposed and is not proved, the French form has not once been used since the English replaced the French in 1755; yet these English-speaking people are expected by the board to abandon their usage of a century and a half and adopt a form which is not only to them wholly new, but also very difficult to pronounce. Again, there is a small river and settlement which appear upon maps and in local newspapers, etc., variously as *Canouse, Canoos* and *Canoose,* the last being the commonest form and expressing exactly its local pronunciation. The board, called upon to choose between these forms, rejects them all, and decides upon an entirely new form, *Kanus,* explaining, in answer to inquiries, that this conforms to the Royal Geographical Society's rules for native names. Aside from the question as to the wisdom of changing century-old and locally-familiar words to newly-invented and strange ones to make them fit with a set of rules designed for a very different purpose, there is in this case the practical trouble that the board's form implies an erroneous pronunciation; for certainly most strangers, reading the form Kanus, would throw the accent on the first syllable and sound the a long, the exact reverse of local usage in both cases. Again, the board, very properly eliding the final possessive s in all cases of divided usage, extends this principle to cases where there is no local diversity. Thus an important bay and settlement are called *Maces Bay,* and a river and settlement are called *Cains River,* and those forms are locally invariable. Yet the board selects them for change and decides upon *Mace Bay* and *Cain River,* forms not only strange to New Brunswick ears, but, as they sound to me, less euphonious and distinctive than the forms in use. Again, there are two important names, *Nepisiguit* and *Shippegan,* which the board decides must be spelled *Nipisiguit* and *Shippigan,* despite the fact that in both cases the former are in best accord with the history of the words, with the best maps, with the common local usage, and, as it seems to me, with a greater symmetry of construction of words. In fact in this case, while the board's forms can be found upon some maps, I can not find a single reason, even in theory, for their adoption in preference to the others. I can not take space to cite further examples, but these are the extreme cases of a number of similar sort.

The first thought of any geographer on reading these observations will be that the board has made these decisions in ignorance of local usage and will reconsider them when the facts are placed before it. Unfortunately, this supposition would not be correct. In the first place, the board has a New Brunswick representative to whom it can turn for local information; but I have in my possession evidence which shows that some at least of these decisions have not the approval of the New Brunswick representative. In the second place, when these decisions were announced by the board four years ago, they were fully discussed and the facts stated at length in a local newspaper, of which copies were sent the board, and to which indeed the board published a reply, though, in my opinion, an insufficient one. Further, within a year past, the facts were fully restated in a new communication sent through a prominent member of the board who agreed to, and doubtless did, lay it before the board. Since the new report affirms all the old decisions without change, we can only conclude that they represent the deliberate judgment of the board, and embody the methods which they propose to apply to Canadian geographical nomenclature. How different this position is from that of the United States board will be evident to every person concerned with geography. The United States board places convenience above all, adopts the best local usage, attempts no reforms upon theoretical grounds, and is steadily reducing confusion in the nomenclature of its territory. The Canadian board disregards local usage and convenience, attempts to reform nomenclature to accord with abstract principles, and is steadily increasing the confusion it was organized to lessen. It will be interesting to observe the comparative worth of the two methods in the geographical development of the future.

W. F. Ganong

Northampton, Mass.

ELIMINATION VS. THE FIRST-SPECIES RULE

Now that both sides of this controversy have presented their arguments, it appears desirable to briefly state the case and give a recital of the principal facts brought out by this discussion.

Briefly speaking, the point at issue is this: In every case where a new genus was founded on two species, neither of which was designated as the type, the advocates of the first-species rule claim that the first species cited or described under such genus is the *de facto* type, and can not become the type of any subsequently established genus. In opposition to this view the advocates of the elimination rule hold that in a case of this kind the action of a later author in selecting the first species as the type of a new genus is regular, and that the remaining species thereby becomes the type of the original genus. In case that the original genus contained three or more species and the later author selected any two of them to form a new genus, only one of them (the one that is the type of the new genus) is eliminated, and the remaining species may be designated the type of the original genus, or it may be subsequently selected as the type of a second new genus.

The advocates of the first-species rule claim for their method that it is the easier of the two and that it always leads to the same results, whereas the elimination method, by requiring a greater knowledge of the literature, is liable to lead to different results in the hands of different persons, according to whether they had consulted a greater or lesser number of publications on the subject.

The principles involved and facts established in this discussion may be grouped as follows:

1. *The method of elimination is correct in principle.* Even the advocates of the first-species rule admit this. It therefore follows that, since these two methods are diametrically opposed to each other, one of them *must* be wrong. The inevitable conclusion, therefore, is reached that the advocates of the first-species rule are contending for a confessedly wrong principle.

2. *The method of elimination is in harmony with the law of priority* It upholds the action of the author who first took out the first species and made it the type of a new genus. In seeking to nullify such action the exponents of the first-species rule are proceeding in direct opposition to the law of priority —the basic law on which, more than on any other, the stability of our nomenclature confessedly depends.

3. *The principle of elimination is embodied in the majority of the codes of nomenclature from the very first.* The advocates of the first-species rule are, therefore, seeking to overthrow a principle that has long been authoritatively recognized and adopted.

4. *The difficulty in elimination is a decided benefit to science.* The subject of nomenclature is altogether too important to be entrusted to the amateur; only the seasoned scientist, who is thoroughly conversant with the literature of the subject, should ever attempt so important a matter.

5. *Elimination is as certain in its results as is the first-species rule.* With a perfected set of rules, any two trained scientists can be depended upon to arrive at the same conclusion in practically every case by the elimination method. The first-species method is not more certain, owing to the fact that in several cases the first species cited was incorrectly identified, and by accepting this name we should thereby be led into an error. Nothing short of an examination of the literature on the subject will secure correct results.

This is the gist of the whole matter. Now, I ask in all seriousness: Can any thoughtful person, having the best interests of science at heart, conscientiously advocate the adoption of the first-species rule—a rule that is admittedly wrong in principle, that is in direct opposition to the fundamental law of priority, that is also in opposition to the codes of nomenclature that have been officially adopted from the earliest times, and that is liable to lead to erroneous results?

D. W. Coquillett
U. S. National Museum,
January 29, 1907

THE U. S. GEOLOGICAL SURVEY

'The good of the cause' must ever be held paramount in the estimation of every right-minded worker. It is for this reason alone, as I state from abundant knowledge, that many earnest students of American geology have refrained from going into print on matters of criticism affecting the U. S. Geological Survey. I should woefully regret the necessity of adopting Dr. Branner's conclusion as to the prime reason for the rule of silence among working geologists outside the survey. The best friends of the national organization have not publicly expressed opinions often privately uttered, simply because personal considerations have been held secondary to the progress of science. The field of American geology is so wide and the best possible achievements of one handicapped by other obligations is so limited, that the local investigator and the expeditionary observer learn to heartily welcome honest review of their own work by men better equipped with tools, duly qualified to gather the facts and not less capable of ratiocination, by reason of previous training, breadth of experience and ability to demonstrate and show cause for the conclusions given in their publications.

The recent unfortunate controversy illustrated by the letters of Messrs. Walcott, Branner and Hobbs in the columns of Science would be deplorable enough under any circumstances, and it might be passed without further remark were it not for several important facts and certain issues which ought not to be longer left in doubt.

1. The undisputed high standing of all these persons, and their many and valuable contributions to American geology, make it incon-

ceivable that the one in command, or either one of the others, could thus publicly discuss a matter involving mere personal issues.

2. The issues joined in the letters themselves do certainly raise questions affecting every American geologist, in or out of the survey, both in his professional capacity and in his relations to the survey as a citizen of the United States.

3. The scope and attitude of the U. S. Geological Survey in its field of work becomes of serious moment if the institution can be justly laid under suspicion of employing its prestige to throttle free discussion.

4. The internal adjustments of the survey as affects its personnel must always have interest to men of science, and it is not a trifling matter when several who have builded their life-work into its structure are compelled to leave it with words of protest.

5. The relation of the survey to other public (state) surveys is also a matter upon which American geologists have an undoubted right, if not a bounden duty, to express opinions freely.

6. The relation of the survey to sporadic workers and others closely concerns every fellow of the Geological Society of America and every geologist who has contributed his mite to the development of this branch of science.

7. The economic aspects of the national bureau and its industrial connections have given cause for more harsh criticism than any other features of its most versatile employment.

The director suggests that discussion can not serve a useful purpose. This is not the first, or second, or third time that this plea, urged by friends of the survey, including the writer, has been used to stem a more or less insistent spirit of adverse criticism. A number of times mild editorials have appeared which would have been followed by more drastic writing had not well-wishers of the survey (by no means its beneficiaries, some even who had suffered injustice from it) interfered successfully in its behalf. Nay more; for many years members of the survey staff have persistently ignored and, directly or by innuendo, thrown a veil of discredit over work previously done, without offering any evidence to offset it, but, on the contrary, confirming the earlier conclusions and taking the credit therefor. These are plain facts. Yet the sufferers thereby, patiently awaiting the vindication of time, have stood in the breach and fought for the honor of the survey—not for fear of any more harm from the same source, but because their devotion to eternal science transcended all personal and temporary considerations. I state these facts very reluctantly in the hope that the director and his staff may learn from them what useful purpose may be served by a plain, straightforward agitation of this whole question now. It can not harm the survey, but do it unspeakable good, if all be well with its heart and soul, as Mr. Walcott assures us is the case. But, with equal regard for Mr. Walcott, the names and work and characters of Branner and Hobbs and others are so cherished by American geologists that very strong proof must be adduced to convince them that they are now wholly in error.

It is because of the achievements of the survey corps under the present able director, that his most true friends have used their best endeavors to uphold and strengthen his hands in times of inimical attacks, and not always in accordance with the dictates of their own best judgment.

To be more explicit. It is very possible that the art of a politican is more effective in securing ample appropriations from congress than could be any amount of geologic ability. But may it not be equally true that a tithe of the amount thus obtained, if actually applied to geologic research, would accomplish much more in the legitimate field of geology than can now be so utilized?

The expansion of the geological survey to cover fields of questionable appropriateness has notoriously partaken of political claptrap, justified or palliated by the friends of the survey on the ground of expediency only. And the supreme test of this outside work is mainly yet to be applied. In those portions where the knowledge and experience of the practical geologist would appear to be most essential, there has often been small provision for the searching preliminary investigations demanded by the situation. Studies of the mining fields are numerous, some excellent and thorough, but many have been entrusted to men of little experience, whose results are anything but satisfactory to those who try to use them in actual practise. Almost invariably these reports ignore the accessible but hardly wrought opinions of precedent workers who have successfully applied their observations in hundreds of instances. This method has become so clearly recognized as a 'geological survey habit' that one does not now expect otherwise. The survey has grown to such gigantic proportions that it can not much longer contain itself. It would be better to diminish its scope than to essay the suicidal rôle of autocrat of American science. With a better appreciation of the shortcomings of some of its own crew, whose hasty and superficial work has caused them to guess that they know more than they really have learned, it is probable that the national bureau would raise more enduring monuments than can be possible under existing methods. Studies of regional geology and monographic productions at the hands of the recognized leaders in geology have largely given place to '*omnium gatherum*' publications of temporary and chamber of commerce application.

In conclusion, it does appear to one friend of the survey that the value of the good will and well-deserved support of the ablest workers in geology is of more consequence than the ephemeral and illusive prestige which may enable the organization to ride rough shod over all as supreme arbiter. Such greed of power, if it really exists, as many have long suspected, can not be long concealed. And once it comes out in the open, its death-blow is self-inflicted. The real fear, that thing of which geologists derated by the survey are actually afraid, is that the just outcome of its energy and resources may not accrue to the legitimate ends in view in its original establishment. This question transcends personal considerations, and it certainly is involved to some extent in the recently published discussion which is the text for this communication.

THEO. B. COMSTOCK

LOS ANGELES, CAL.,
January 12, 1907

SPECIAL ARTICLES

VARIATION IN MOSQUITO HABITS

DURING the summer of 1906, a more systematic series of observations was made on the salt-marsh area near the city of Elizabeth, New Jersey, partly to determine the number of broods, partly to ascertain the relative proportion of *C. cantator* and *C. sollicitans*, and partly to learn more of their migrations.

Generally speaking, we knew that the number of broods varied with the season, and that it was largely a matter of tides and storms as to how many there would be. We knew also that in the southern part of New Jersey *C. sollicitans* was the dominant species, *C. cantator* forming only a small minority early in the season, and that *cantator* was dominant in the northern section at least during the early part of the summer. Concerning the migrations we knew that they occurred; but just how long they were continued and how far they extended was yet a question.

As early as April 19 there was a full brood of larvæ in the pools and these matured before the end of the month. It formed brood I. of the season, was almost all *cantator*, and the adults left the meadow soon after hatching and traveled inland along the valleys of the Rahway and Elizabeth Rivers. On May 2 they were met with in great numbers at Millburn and covered the entire territory between that and the marsh, a distance of about twelve miles in a direct line. This migration was not followed from the marsh directly.

The II. brood was in the pools, already well grown May 10 and reached the adult stage May 15, 80 per cent. *cantator*, 20 per cent. *sollicitans*. It was also a large brood, left the meadow promptly in large proportion, and was followed through the Elizabeth Valley to Elizabeth, Aldene, Salem, Union, Springfield and Maplewood. It also extended all along the first ridge of the Orange Mountains and reached Summit, a distance of fifteen miles from their place of birth. Just how far beyond that individuals traveled we could not determine, but as that country is hilly they got no further in swarms. Specimens in numbers are recorded from Madison, however, from a previous year's collection.

June 1, while brood III. was in the early larval stage and all the migrants had left the marsh, one of my assistants spent the night on it with instructions to capture and record every specimen that alighted on him or attempted to bite. The record is as follows:

Between the Hours	Cantator	Sollicitans
5– 6 P.M.	16	9
6– 7 "	23	19
7– 8 "	30	27
8– 9 "	13	14
9–10 "	5	7
10–11 "	1	1
11–12 "	2	0
12– 1 A.M.	1	0
1– 2 "	2	0
2– 3 "	0	2
3– 4 "	1	3
4– 5 "	14	20
5– 6 "	22	46
	130	148

It appears, therefore, that as between the two, *sollicitans* seems to have a period of rest during the middle of the night, but is much more active than *cantator*, especially in the morning hours. And the proportion is greater than shown by the figures, because the actual number of *cantator* on the meadow was as about 8 to 2 of *sollicitans*.

The advance guard of brood III. emerged from the pools June 10 and then came a series of high tides that brought killifish everywhere on the meadow and simply swept what remained out of existence. The survivals were 70 per cent. *cantator* to 30 per cent. *sollicitans* and none of these extended inland further than Short Hills, a distance of about ten miles from the marsh.

Brood number IV. was in the pools June 24, and on the wing July 3. It was a small brood, almost evenly divided between the two species, and the flights extended to Short Hills July 14 and probably to Summit as well; this latter record based upon specimens sent in at the time.

Brood V. came to maturity between July 23 and 28, consisted of 80 per cent. *cantator* and did not get beyond Irvington, about five miles away. It was small in numbers.

Brood VI. was a very large one which reached the adult stage August 13, 85 per cent. *sollicitans* to 15 per cent. *cantator*. It was the first brood in which *sollicitans* was dominant and it left the meadows almost immediately. A day after they began to emerge the highlands approaching the marsh were swarming with adults while the marsh itself was comparatively free. This brood traveled almost due west and supplied the heaviest swarm of the season for Summit and intervening places. Fully 90 per cent. of this brood left the meadow.

Brood VII. matured August 31 and was a small one, 65 per cent. *sollicitans*, 35 per cent. *cantator*. Contrary to the one before, this was a stay-at-home brood; not 10 per cent. left the meadows and none got much if any beyond North Elizabeth, only a mile or two from the marsh.

Brood VIII. was on the wing September 18, 70 per cent. *sollicitans*, and was also a stay-at-home. It was also a small one and did not get much beyond the immediately surrounding highlands.

Brood IX. was on the wing October 2, also small in size, 90 per cent. *sollicitans* and not a migrant.

Brood X. was in the pools October 12, largely *sollicitans* and most of them fell victims to fish carried up over the meadows by the early fall tides. Very few adults were observed later and there was no migration. After this breeding was irregular and while larvæ of *cantator* were found as late as November 30, it is perhaps questionable whether any of the insects reached the egg-laying condition.

The interesting point here is the difference between the broods in their tendency to migrate. The early broods always migrate freely as far as my observations extend, and whenever meadow conditions are favorable, the first days of May find a cloud of marsh mosquitoes sweeping inland.

As the marshes are usually waterlogged in early spring and every pool clean up to the highland holds water, the hibernating eggs hatch in large proportion and the broods are large.

An interesting question arises here. The migrants are almost exclusively sterile females: the eggs from which many of them hatch have been on the marsh from early the year before, ready to hatch when opportunity offered. Is there any relation between the age of the egg and the sterility of the females resulting from them? The matter will not be easy to demonstrate because of the difficulty of securing pairings in confinement.

John B. Smith

COLOR INHERITANCE IN MAMMALS

Professor Castle's interesting article in Science of January 25 clears up an important point, and renders it possible to explain certain phases of color inheritance in swine and in cattle. For the most part, his factor A, which determines the arrangement of pigment giving the agouti color, seems to be wanting in these two classes of domesticated animals. Perhaps it has been lost. It seems to be present in the wild boar of Europe, which has been used in breeding experiment by Mr. Q. I. Simpson, whose work has furnished important data for the elucidation of color inheritance in swine. In a few instances there is a tendency in certain breeds of swine for red pigment to predominate near the extremities of hairs; in the Berkshire breed occasional individuals show this tendency, and I have seen the same in crosses between this breed and Hampshires. The tendency is never well marked, so that in these animals the function A is presumably present in a weakened condition.

For the most part black and red in swine and cattle evidently behave just as they do in guinea-pigs. Aberdeen-Angus (black) cattle crossed on Herefords (red and white) give blacks. The heterozygotes bred back to Herefords give blacks and reds in approximately equal numbers. In swine, red and black each appear to present more than one type, and the various reds and blacks do not behave quite the same. Tamworths, a red breed of swine, present at least two distinguishable forms of red, namely, light red and dark red. The light becomes lighter with age, and the dark darker. Light is also dominant to dark. When light red is crossed on Chester white the progeny is red roan. Dark red crossed on Chester white gives clear white.

Most black breeds of swine, when crossed with Tamworths or Duroc-Jersey (both red), give black and red spotted, but Hampshires (black with white belt) crossed with red give the Hampshire coloring. This shows that Hampshire black and Berkshire black differ.

It is highly significant that the same color factors should exist (apparently) in guinea-pigs, rats, mice, rabbits, swine and cattle. This fact may be of great service in breeding fixed color types in farm animals.

Professor Castle's clear explanation of color types in Guinea pigs will doubtless aid greatly in comprehending the data on color inheritance in swine which the committee on animal hybrids is collecting for the American Breeders Association.

The object of this communication is not, however, to call attention to the parallel in color factors in different classes of mammals; for there is not at hand sufficient data to demonstrate a complete parallel. It is rather to call attention to a simple method of expressing the allelomorphic constitution of organisms, and one which renders it easy, when this constitution is known, to display the necessary results of a given line of breeding. We may use Professor Castle's data in illustrating the method.

The allelomorphic formula of a homozygous individual may be represented by AA, BB, CC, etc. The gametes produced by such an individual would be ABC, etc. Letting G stand for the factor which determines the agouti color, Bl for black, and R for red pigment, and letting A stand for the absence of G, B for the absence of Bl, and C for the absence of R, the formulæ for the several types of color discussed by Professor Castle would be:

(1) Agouti,	GG, BlBl, RR, producing gametes			G Bl R
(2) Black,	AA, BlBl, RR,	"	"	A Bl R
(3) Red,	AA, BB, RR,	"	"	A B R
(4) Red,	GG, BB, RR,	"	"	G B R

The cross between (2) and (3) gives AA, BlB, RR, which is black. The gametes produced by this hybrid are ABlR and ABR. The fortuitous union of these gives, in generation F_2:

1 AA, BlBl, RR; 2 AA, BlB, RR; 1 AABBRR,
or 3 black and 1 red.

The cross between (2) and (4) gives AG, BlB, RR (agouti), producing gametes ABlR, ABR, GBlR, GBR. The fortuitous union of these gives:

a) 1 AA, BlBl, RR; (*d*) 2 AG, BlBl, RR; (*g*) 1 GG, BlBl, RR;
(*b*) 2 AA, BlB, RR; (*e*) 4 AG, BlB, RR; (*h*) 2 GG, BlB, RR;
(*c*) 1 AA, BB, RR; (*f*) 2 AG, BB, RR; (*i*) 1 GG, BB, RR
or

1 Black	2 Agouti	1 Agouti
2 "	4 "	2 "
1 Red	2 Red	1 Red.

This gives 9 agouti, 4 red, and 3 black.

If the cross (2) $\times$ (4) (= AG, BlB, RR), is crossed with (3) (AA, BB, RR) we get:

Gametes of (2) x (4)		Gametes of (3)
ABlR	x	ABR = AA, BlB, RR (black)
ABR	x	ABR = AA, BB, RR (red)
GBlR	x	ABR = GA, BlB, RR (agouti)
GBR	x	ABR = GA, BB, RR (red),

or 2 reds, 1 black, and 1 agouti. Professor Castle states that 4 types of reversionary agoutis probably occur in the cross (2) $\times$ (4), 3 of which have been obtained in his experiments. Table I. above, giving generation F_2 of this cross, shows that the type (*e*) (AG, BlB, RR) occurs in four sixteenths of this generation, type (*h*) (GG, BlB, RR) in two sixteenths, and type (*d*) (AG, BlBl, RR) in two sixteenths. These are the three types he found in his experiments. The fourth type, (*g*) (GG, BlBl, RR) occurs in only one sixteenth of this generation. The small number of individuals in which this type occurs doubtless accounts for the fact that it has not yet been found in the experiments.

From the formulæ of these four types we readily see why they behave as stated by Professor Castle. Type (*e*) is the same as the cross (2) $\times$ (4), and therefore gives, in generation F_2, 9 agoutis, 4 reds, and 3 blacks; (*h*) produces gametes GBlB and GBR. These uniting with ABR (red) give half AG, BlB, RR (agouti) and half AG, BB, RR (red); (*d*) produces gametes ABlR and GBlR. These uniting with ABR (red) give half AA, BlB, RR (black) and half AG, BlB, RR (agouti). The remaining type (*g*) is pure agouti, and breeds true.

The above formulæ may seem complex at first, but they are really quite simple, and render the process of determining the character content of any cross and its progeny exceedingly easy.

One of the above crosses shows how two characters that are not allelomorphic to each other may still give the numerical relations in generation F_2 of a pair of allelomorphs. An individual having the color black may have the formula BlBl, RR, the red not being noticeable because indiscriminately mixed with, and concealed by, the black pigment. A red individual may have the formula AA, RR, in which A represents the absence of Bl. The respective gametes are BlR and AR; the hybrid being ABl, RR, which is black. The gametes produced by this hybrid are AR and BlR. Fortuitous union of these gives:

AR x AR = AA, RR (red)
AR x BlR = ABl, RR } (black-hybrids)
BlR x AR = ABl, RR }
BlR x BlR = BlBl, RR (black-pure)

This is clearly the same result, as far as color of progeny is concerned, as if Bl and R were a pair of allelomorphs. We know they are not, since in the pure agouti type both colors are present in such form as to be transmitted to all the progeny.

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NOTES ON ORGANIC CHEMISTRY

DIAZOAMINOMETHANE (DIMETHYLTRIAZINE)

Many members of the important class of compounds known as the diazoamino derivatives are known, but these, hitherto, have all belonged to the aromatic series. Otto Dimroth[1] has, however, recently succeeded in isolating the first and lowest representative of the aliphatic division—*diazoaminomethane* or *dimethyltriazine,* as it may also be called, $CH_3N{:}NNHCH_3$. The compound is interesting not only for the reasons given, but also on

[1] *Ber. d. Chem. Ges.*, **39**, 3905 (1906).

account of its peculiar properties, which rendered its isolation and investigation a matter of extreme difficulty.

Sodium azoimide, NaN_3, when treated with dimethyl sulphate, yields methylazide,

$$CH_3N<\begin{matrix}N\\ \vdots\\ N\end{matrix},$$

and this, by the action of methylmagnesium iodide (Grignard's reagent) and water gives diazoaminomethane, which is a colorless liquid, melting at $-12°$. It is extremely reactive and is decomposed during the course of its preparation by the catalytic action of the small quantity of impurity usually present in magnesium. It boils at 92°, but promptly decomposes, volatilizes readily at the ordinary temperature, more rapidly at the boiling point of ether and is miscible in all proportions with every solvent. Acids convert it instantly into nitrogen, methylamine and the methyl ester of the acid. In dilute solution it has a sweet taste, but the pure compound rapidly cauterizes and blisters the skin, and its vapor, when inhaled, produces severe headache accompanied by a prolonged feeling of lassitude. Diazoaminomethane forms a *silver salt*, $CH_3N{:}NN(Ag)CH_3$, and a *cuprous salt*, $CH_3N{:}NN(Cu)CH_3$, the latter crystallizing in large, lustrous, yellow prisms. It is by means of this compound that the separation and final purification of the diazoaminomethane was effected.

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CURRENT NOTES ON METEOROLOGY

MOISTURE FROM CLOUDS

Some interesting observations have lately been made by Marloth on the amount of moisture deposited by the S. E. trade clouds on vegetation growing on the summit of Table Mountain, South Africa (*Trans. So. Afr. Phil. Soc.*, XIV., Pt. 4, Oct., 1903; XVI., Pt. 2, Oct., 1905; *Met. Zeitschr.*, Dec., 1906). In this southwestern extremity of South Africa the winter is rainy and the summer dry. About three quarters of the annual precipitation falls in the six winter months, and in the three summer months (Dec.–Feb.) only about eight per cent., 2.16 inches, falls on the average. It occasionally happens that two months may pass without a drop of rain. The vegetation on the hills and on the lower slopes of the mountains clearly reflects the deficiency of summer precipitation, but on the mountains vegetation is much more abundant, and shows much more favorable conditions of moisture supply. The latter has been shown by Marloth to come from the clouds formed over the mountains in the S. E. trade wind. The plants collect the cloud drops in sufficient quantity, not only to keep themselves wet, but even to furnish enough water to produce a permanent swamp on the top of Table Mountain in winter, and a periodic swamp in summer. The summer swamp dries up during long spells of clear weather, but appears again when the S. E. cloud is formed. Small ponds actually form, sometimes even in late summer, on the top of Table Mountain. A photograph of a pond appears in Marloth's report. An interesting piece of evidence as to the effect of the water thus collected by vegetation is given in the note that in the case of a mountain stream in this region, which can furnish sixty horse-power, three days after a fire which burned off the bushes and grass at the head of this stream, the water furnished only twenty horse-power. The cloud on Table Mountain is a mixture of an ordinary cloud and very finely distributed rain-drops in process of formation. The whole mass moves at high velocity (the trade velocity is there often forty miles an hour), which prevents the fall of small drops. It is not until they come in contact with a solid object, and when the velocity is reduced, that the drops are held by the obstacle, and gradually reach the ground.

In connection with this phenomenon reference may be made to various suggestions that have been brought forward regarding the possible utilization of fog for the uses of vegetation in California (*Mo. Wea. Rev.*, Oct., 1898, 466; 1899, 301, 473); also to Hann's 'Handbook of Climatology' (English translation), 195–196.

MONTHLY WEATHER REVIEW

The October, 1906, number of the *Monthly Weather Review* (dated Jan. 4, 1907) contains the following papers of general interest: 'A Rare Cumulus Cloud of Lenticular Shape,' by H. H. Clayton; illustrated by three half-tone views, and accompanied by comments on similar clouds by Professor Abbe. 'Monthly Review of the Progress of Climatology throughout the World,' by C. F. Talman; notes on meteorological stations in Iceland, and on the climates of British East Africa, the Solomon Islands and of Sistan. 'Sonora Storms and Sonora Clouds of California,' by Archibald Campbell, with a weather map showing the conditions which prevail during a Sonora cloud period, and a half-tone picture of one of these clouds. 'Has the Gulf Stream any Influence on the Weather of New York City?' by James Page, of the Weather Bureau. This is a brief discussion which we hope may find its way very generally into the daily press, and set right the many erroneous ideas which are prevalent on this question. Professor F. H. Bigelow continues his study of the meteorological conditions of the Cottage City waterspout of 1896 with a highly mathematical paper.

ALTITUDE AND PNEUMONIA

Dr. Isaac W. Brewer, of Fort Huachuca, Ariz., after studying the medical statistics of the army concludes (1) that altitude has nothing to do with the mortality from lobar pneumonia; (2) that latitude within the range afforded by the territory of the United States has nothing to do with the mortality; (3) that the mortality among the colored troops is about twice as great as among the white soldiers (*So. Cal. Practitioner,* Dec., 1906).

THE FRENCH SAHARA

The meteorological observations made in the French Sahara by the *Mission Saharienne* (*Mission Foureau-Lamy d'Alger au Congo par le Tchad,* Pts. I. and II., pp. 551, Paris, Masson) are welcome as throwing light on the climate of a region concerning which but little is known. The temperature fell below freezing twenty-five times. The maximum was 119°. The minimum temperature was recorded about 5 A.M.; the maximum between 1 and 2 P.M. Dew was observed on fourteen occasions. There was rain on 116 days (out of 645). In the Aïr highlands thunder and lightning were noted almost every afternoon.

R. DeC. Ward

Harvard University

WILLIAM WELLS NEWELL

William Wells Newell, the founder of the American Folk-Lore Society, died at his summer home in Wayland on January 21st, 1907, at the age of sixty-eight.

The broad culture of Mr. Newell and his extended interest in many branches of literature and science made his name known throughout the country. Those best acquainted with him were often amazed at the accurate knowledge and the sincere appreciation of subjects widely diverse in interest. He was especially gifted as a student of folklore and comparative literature and as a classical scholar, a linguist and a craftsman. Mr. Newell made a special study of the Arthurian myth and his collection of tales, 'King Arthur and the Round Table,' published in 1897, showed deep research and an intimate knowledge of the literature of the time. His translation of Sophocles's 'Œdipus Tyrannus' reveal him as a student of the classics. 'Words for Music,' a little volume of verse, most of which was original, contains charming bits of a more or less personal nature which show Mr. Newell as his intimate friends knew him, lovable, kindly and appreciative of all that was good. The book itself is an example of Mr. Newell's abilities as a craftsman. It was printed upon his private press at Hazelbrook, Wayland, and is an example of typographical excellency.

It is, however, the cause of American Folk-Lore that has suffered most in the death of Mr. Newell. It has lost its most enthusiastic worker and devoted friend. The American Folk-Lore Society was his from the very beginning, and it was owing to his untiring energies that the *Journal of American Folk-Lore* was started and has since been successfully carried on.

His death has left a void in the lives of many and has deprived the cause of culture of a strong supporter. A. M. T.

SCIENTIFIC NOTES AND NEWS

By the will of M. Daniel Osiris, the Pasteur Institute of Paris receives an additional endowment of $5,000,000. It is said that the institute will establish branches for scientific research in various places in France and the French colonies.

The Berlin Academy of Sciences has elected to membership Dr. Johannes Orth, professor of pathological anatomy; Dr. Max Rubner, professor of hygiene, and Dr. Albrecht Penck, professor of geography, all of the University of Berlin.

As has been already announced, the buildings of the Carnegie Institute, Pittsburg, will be dedicated on April 11, 12 and 13. A number of distinguished foreigners will be present, including among men of science: Sir Robert Ball, professor of astronomy, Cambridge University; Mr. Guglielmo Marconi; Dr. P. Chalmers Mitchell, secretary of the London Zoological Society; Sir William Henry Preece, electrical engineer; Sir William Turner, principal of Edinburgh University; M. Marcellin Boule, director of the Paris Museum of Natural History, and Professor Friedrich S. Archenhold, director of the Treptow Observatory.

Sir Charles Todd, F.R.S., director of the Astronomical and Meteorological Observatory of South Australia, has retired, having reached the age of eighty years. He was, until last year, also postmaster general and superintendent of telegraphs. Sir Charles is succeeded in the directorship of the observatory by Mr. F. Griffith.

At a meeting of the Nebraska Academy of Medicine, held in Lincoln on January 10, a committee of five, consisting of Drs. Solon R. Towne, Alexander S. von Mansfelde, Henry B. Ward, Robert H. Wolcott and H. Winnett Orr, was appointed to make an effort to obtain the Nobel prize for Dr. James Carroll, U. S. Army.

Dr. N. L. Britton, director of the New York Botanical Garden, accompanied by Mrs. Britton and Dr. Charles F. Millspaugh, curator of botany in the Field Museum of Natural History, have gone to Nassau, where a schooner will be chartered for a cruise among the smaller islands of the Bahamian group. This is Dr. Britton's fourth trip to the Bahamas, and it is understood that a volume on their flora will be published by him in connection with Dr. Millspaugh.

At the Leicester meeting of the British Association the evening lectures will be by Mr. W. Duddell, on 'The Arc and the Spark in Radio-telegraphy,' and by Dr. F. A. Dixey, on 'Recent Developments in the Theory of Mimicry.' The lecture to the operative classes will be given by Professor H. A. Miers, F.R.S., on 'The Growth of a Crystal.'

Lord Avebury has been elected president of the Royal Microscopical Society.

At the annual meeting of the London Entomological Society, on January 23, the retiring president, Mr. F. Merrifield, made the address. Mr. C. O. Waterhouse was elected president for the ensuing year.

Professor A. Lawrence Lowell, who holds the chair of the science of government at Harvard University, has been selected to be the special Harvard lecturer at Yale University for 1907. This lectureship, as will be remembered, was founded in 1905 by the gift of $10,000 from an anonymous Harvard graduate; the income of the fund to be used in securing members of the Harvard faculty to give lectures at Yale.

Dr. David P. Barrows, director of the Bureau of Education of the Philippine Islands, gave a lecture on 'Mohammedanism in the Philippine Islands' before the California branch of the American Folk-lore Society on February 7.

The ninth lecture in the Harvey Society Course will be delivered by Professor W. T. Councilman, professor of pathology, Harvard University, at the New York Academy of Medicine, on Saturday, February 23 at 8:30 P.M. Subject: 'The Relation of Certain

Leucocytes to Infectious Diseases.' All interested are cordially invited to be present.

The Portuguese members of the Fifteenth International Medical Congress, held in April last, have presented Professor Miguel Bombarda with a gold medal and an address expressing their appreciation of his services as organizing secretary of the congress.

The Emperor of Austria has conferred on Duke Karl Theodor, of Bavaria, the Order for Art and Science in recognition of his distinction as an ophthalmologist.

The British Academy has received the sum of £10,000 for the purpose of establishing a memorial to the late Mr. Leopold Schweich of Paris. In accordance with the wishes of the donor, the endowment is to be called 'The Leopold Schweich Fund,' and is to be devoted to the furtherance of research in the archeology, art, history, languages, and literature of ancient civilization, with reference to Biblical study.

Professor Thomas Condon, who held the chair of geology in the University of Oregon, died on February 11, at the age of seventy-five years.

Mr. William Simms, senior fellow of the Royal Astronomical Society, died on January the second in his ninetieth year.

Mr. E. B. McClellan, third assistant at the Radcliffe Observatory, Oxford, died on January 2, at the age of forty-five years.

Mr. F. P. H. Stirling, professor of mathematics in the Christian College at Madras, has died at the early age of twenty-six years.

The death is announced of Professor Pierre Budin, of Paris, known for his work on obstetrics and the hygiene of infancy.

We regret also to learn of the death of Dr. Clement Schlueter, who not long ago retired from the professorship of geology at Bonn University. Professor Schlueter was a paleontologist who had devoted much attention to the cretaceous fossils of north Germany, particularly to the Echinoidea. His work on them was distinguished by learning, care and accuracy.

There will, on March 5, be civil service examinations for geologists in the Philippine service, at a salary of $2,000, and for irrigation manager in the Office of Experiment Stations, of which there are five vacancies, at salaries of from $1,800 to $2,500.

The National Educational Association will hold its fiftieth anniversary meeting at Los Angeles from July 8 to 12. It was originally intended to hold this meeting in Philadelphia, where the association was organized in 1857, but the railways would not grant the usual terms, by which the membership fee is collected with the ticket and return privileges granted until September 1. Dr. Nathan C. Schaeffer, superintendent for Pennsylvania, is president of the association.

The fourteenth International Congress of Hygiene and Demography will be held at Berlin from September 23 to 29. Communications relative to the congress should be addressed to the general secretary, Dr. Nietner, Eichhornstrasse 9, Berlin, 9 W.

The seventh International Congress of Physiology will be held this year at Heidelberg from August 13 to 16, under the presidency of Professor August Kossel. In connection with the congress there will be an exhibition of scientific apparatus. Announcements of communications should be sent to the Physiological Institute, Heidelberg, before June 15.

An International Congress for Psychiatry, Neurology and the Care of the Insane is being organized under the auspices of the Dutch Society of Psychiatry and Neurology, to be held at Amsterdam from the second to the ninth of September of the present year. Those who have been invited to be American members of the committee of arrangements are: G. Alden Blumer, Providence; Charles K. Mills, John K. Mitchell and S. Weir Mitchell, of Philadelphia, and R. G. Rabinovitch, of New York City.

We learn from the *Journal of the American Medical Association* that the universities at Heidelberg, Berlin and Tübingen have each received 10,000 Marks from the estate of the late eminent ophthalmologist, Herman Cohn, of Breslau. The income from each endowment is to be awarded as a prize for research in ophthalmology.

THE Wisconsin Archeological Society has secured an option on and will purchase the remarkable man mound near Baraboa and thus make it accessible to the public. The money necessary to its purchase will be raised by popular subscription.

AT a meeting held on January 29 at the residence of M. Beernaert, Minister of State, it was decided to organize a new Belgian South Polar expedition. It is also announced that a new British expedition to the South Polar regions will leave England next October under the command of E. H. Schackleton, who was third lieutenant on the *Discovery* expedition.

WE learn from *Nature* that an Association for the Promotion of Flight is in course of formation. The association will aim at assisting inventors and investigators to carry out experiments in artificial flight. In order to secure that no funds shall be subscribed by speculators with any hope of return, it is proposed that, in the case of its ultimate success in its object, the valuable assets, such as a facility for constructing practicable flight machines, should be handed, free of cost, to the nation. A provisional committee has been appointed, which includes the Hon. C. A. Parsons, F.R.S., Sir William Crookes, F.R.S., Major B. F. S. Baden-Powell and others.

THE Prussian government has authorized the organization of a lottery for the benefit of Gen. Count Zeppelin's further experiments in airship building.

THE Rothamsted Experimental Station in Hertfordshire has received a gift of £2,000 from the Permanent Nitrate Committee.

REUTER'S AGENCY is informed that Major Powell Cotton has sent home a complete skeleton of an okapi, the skull of which is said to be probably one of the most remarkable specimens ever brought to this country. In addition, there is a beautifully marked and perfect skin in a better condition than that now in the national collection. Major Powell Cotton has also sent to England the skin of a young okapi. All are now at the British Museum.

UNIVERSITY AND EDUCATIONAL NEWS

THE appellate division of the Supreme Court has decided that the will of the late Wallace Andrews of New York City, bequeathing more than $1,500,000 for a school for girls at Willoughby, Ohio, is valid. The money was to go to the Smithsonian Institution in case the bequest for the school was invalid.

MR. A. MCCHARLES has bequeathed $10,000 to the University of Toronto, to establish prizes for scientific discoveries.

SIR COWASJEE JEHANGHIR READYMONEY has offered to the Bombay government the sum of $80,000 for the erection of a university examination hall in Bombay, thus following the example of his father in giving to the city the Elphinstone College buildings and the Senate-hall of the university.

MR. W. F. STANLEY, of the firm of optical and scientific instrument makers, has built and endowed a new trade technical school at South Norwood Hill, for 400 boys, at a cost of some $250,000.

THE late Dr. John Wight has left £3,000 to Aberdeen University to found four or more medical bursaries.

WE learn from *The British Medical Journal* that Dr. Schorstein, whose early death deprived the London Hospital of one of the ablest members of its medical staff, has bequeathed £500 to that hospital. He has also bequeathed £500 to the regius professor of medicine at Oxford, and the residue of his estate to the university chest at Oxford, subject to trusts to pay the income to his mother and sister for their lives, with ultimate remainder for such purposes as the Hebdomadal Council may decide. He expressed the hope that the bequest would be used for something in connection with the medical school at Oxford. The Oxford medical school will probably eventually benefit by a capital sum of between seven and eight thousand pounds.

MEDICAL journals note the beginning of a movement to establish a university in Frankfort. Rich citizens are said to have collected funds for the purpose, but since the creation of universities belongs exclusively to the state and not, as in America, to private initiative, the funds will be given to the government if it approves of the objects of the contributors. Frankfort has for a long time possessed the nucleus of a medical faculty; the old Senckenberg Institute, founded by a wealthy citizen, contains a number of well-furnished chemical, physical, anatomical and other laboratories; the Royal Institution for Experimental Therapy under the direction of Professor Ehrlich attracts every year a great many graduates from other parts of Germany and from abroad; the hospitals of Frankfort are of the first rank, and contain an immense amount of clinical material which has not hitherto been used for teaching purposes. The establishment of a university will, therefore, be easy, so far as the medical faculty is concerned. No new universities have been founded in Germany for about a century, except at Strasburg, where the old university existing previously to the French occupation was reestablished in 1872.

A department of forestry, professional in character, has, as we have already noted, been organized at the Pennsylvania State College, the first registrations having been made for the spring session of 1907. The department is organically arranged in the School of Agriculture, the studies of the first year being in common. An announcement by Dr. B. E. Fernow, professor of forestry, says: "The profession of forestry, although practised in Europe for more than a century and a half, is quite new in this country, the first professional school having been established less than a decade ago, yet the need of foresters has grown more rapidly than the several schools which followed the first have been able to provide. At present the largest demand is made by the federal forest service, but the various states, and especially the state of Pennsylvania, as well as private owners and corporations, are bound to call for the services of fully equipped foresters in large numbers, as the needs and advantages of a better treatment of our woodlands becomes recognized. The state of Pennsylvania has set aside state forest reservations to the extent of nearly one million acres, and adds annually more. It is only fair to assume that graduates of the Pennsylvania State College must ultimately find a field of usefulness in their management."

The Hungarian government is said to have under consideration the foundation of a new university at Pressburg. The existing universities of Hungary are those of Buda Pesth and Klausenburg.

The Senate of London University have accepted an invitation from the University of Paris to send eighty representatives of the university to visit Paris.

The University of Virginia will this year conduct a summer school.

The classes for workingmen of New Haven, inaugurated by the Sheffield Scientific School, held their first session on January 17. Over 150 men appeared at the first classes, overflowing the rooms originally assigned.

A principal is to be chosen in March for the Dunn County School of Agriculture. The salary is $2,000. Candidates are to write to the present principal, Dr. F. C. Davis, Menomonie, Wis. The school is said to be the first of its kind to be established in America, and has made a valuable place for itself by the instruction of the young men and women of the vicinity along agricultural and other economic lines. Also by carrying on some twenty-five lines of useful work for farmers, such as testing cows for butter fat—testing herds for tuberculosis, etc., and by holding many farmers' institutes each year. Dr. Davis leaves the place in June to become dean of the new state school of agriculture at Canton, N. Y.

Dr. John W. Harshberger has been promoted to be assistant professor of botany in the University of Pennsylvania.

Professor Kuenen has resigned the Harris chair of physics in University College, Dundee, and accepted the new chair of physical chemistry in the University of Leyden.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MARCH 1, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE ENERGIES OF MEN[1]

WE habitually hear much nowadays of the difference between structural and functional psychology. I am not sure that I understand the difference, but it probably has something to do with what I have privately been accustomed to distinguish as the analytical and the clinical points of view in psychological observation. Professor Sanford, in a recently published 'Sketch of a Beginner's Course in Psychology,' recommended 'the physician's attitude' in that subject as the thing the teacher should first of all try to impart to the pupil. I fancy that few of you can have read Professor Pierre Janet's masterly works in mental pathology without being struck by the little use he makes of the machinery usually relied on by psychologists, and by his own reliance on conceptions which in the laboratories and in scientific publications we never hear of at all.

Discriminations and associations, the rise and fall of thresholds, impulses and in-

[1] Delivered as the presidential address before the American Philosophical Association at Columbia University, December 28, 1906.

hibitions, fatigue,—these are the terms into which our inner life is analyzed by psychologists who are not doctors, and in which, by hook or crook, its aberrations from normality have to be expressed. They can indeed be described, after the fact, in such terms, but always lamely; and everyone must feel how much is unaccounted for, how much left out.

When we turn to Janet's pages, we find entirely other forms of thought employed. Oscillations of the level of mental energy, differences of tension, splittings of consciousness, sentiments of insufficiency and of unreality, substitutions, agitations and anxieties, depersonalizations—such are the elementary conceptions which the total view of his patient's life imposes on this clinical observer. They have little or nothing to do with the usual laboratory categories. Ask a scientific psychologist to predict what symptoms a patient must have when his 'supply of mental energy' diminishes, and he can utter only the word 'fatigue.' He could never predict such consequences as Janet subsumes under his one term 'psychasthenia'—the most bizarre obsessions and agitations, the most complete distortions of the relation between the self and the world.

I do not vouch for Janet's conceptions being valid, and I do not say that the two ways of looking at the mind contradict each other or are mutually incongruous; I simply say that they are incongru*ent*. Each covers so little of our total mental life that they do not even interfere or jostle. Meanwhile the clinical conceptions, though they may be vaguer than the analytic ones, are certainly more adequate, give the concreter picture of the way the whole mind works, and are of far more urgent practical importance. So the 'physician's attitude,' the 'functional psychology,' is assuredly the thing most worthy of general study to-day.

I wish to spend this hour on one conception of functional psychology, a conception never once mentioned or heard of in laboratory circles, but used perhaps more than any other by common, practical men —I mean the conception of the *amount of energy available* for running one's mental and moral operations by. Practically every one knows in his own person the difference between the days when the tide of this energy is high in him and those when it is low, though no one knows exactly what reality the term energy covers when used here, or what its tides, tensions, and levels are in themselves. This vagueness is probably the reason why our scientific psychologists ignore the conception altogether. It undoubtedly connects itself with the energies of the nervous system, but it presents fluctuations that can not easily be translated into neural terms. It offers itself as the notion of a quantity, but its ebbs and floods produce extraordinary qualitative results. To have its level raised is the most important thing that can happen to a man, yet in all my reading I know of no single page or paragraph of a scientific psychology book in which it receives mention—the psychologists have left it to be treated by the moralists and mind-curers and doctors exclusively.

Every one is familiar with the phenomenon of feeling more or less alive on different days. Every one knows on any given day that there are energies slumbering in him which the incitements of that day do not call forth, but which he might display if these were greater. Most of us feel as if we lived habitually with a sort of cloud weighing on us, below our highest notch of clearness in discernment, sureness in reasoning, or firmness in deciding. Compared with what we ought to be, we are only half awake. Our fires are damped, our drafts are checked. We are making use of only a small part of our possible mental and physical resources. In some persons this sense of being cut off from their rightful resources is extreme, and we then get the formidable neurasthenic and psychasthenic conditions, with life grown into one tissue of impossibilities, that the medical books describe.

Part of the imperfect vitality under which we labor can be explained by scientific psychology. It is the result of the inhibition exerted by one part of our ideas on other parts. Conscience makes cowards of us all. Social conventions prevent us from telling the truth after the fashion of the heroes and heroines of Bernard Shaw. Our scientific respectability keeps us from exercising the mystical portions of our nature freely. If we are doctors, our mind-cure sympathies, if we are mind-curists, our medical sympathies, are tied up. We all know persons who are models of excellence, but who belong to the extreme philistine type of mind. So deadly is their intellectual respectability that we can't converse about certain subjects at all, can't let our minds play over them, can't even mention them in their presence. I have numbered among my dearest friends persons thus inhibited intellectually, with whom I would gladly have been able to talk freely about certain interests of mine, certain authors, say, as Bernard Shaw, Chesterton, Edward Carpenter, H. G. Wells, but it wouldn't do, it made them too uncomfortable, they wouldn't play, I had to be silent. An intellect thus tied down by literality and decorum makes on one the same sort of impression that an able-bodied man would who should habituate himself to do his work with only one of his fingers, locking up the rest of his organism and leaving it unused.

In few of us are functions not tied-up by the exercise of other functions. G. T. Fechner is an extraordinary exception that proves the rule. He could use his mystical faculties while being scientific. He could be both critically keen and devout. Few scientific men can pray, I imagine. Few can carry on any living commerce with 'God.' Yet many of us are well aware how much freer in many directions and abler our lives would be, were such important forms of energizing not sealed up. There are in everyone potential forms of activity that actually are shunted out from use.

The existence of reservoirs of energy that habitually are not tapped is most familiar to us in the phenomenon of 'second wind.' Ordinarily we stop when we meet the first effective layer, so to call it, of fatigue. We have then walked, played, or worked 'enough,' and desist. That amount of fatigue is an efficacious obstruction, on this side of which our usual life is cast. But if an unusual necessity forces us to press onward, a surprising thing occurs. The fatigue gets worse up to a certain critical point, when gradually or suddenly it passes away, and we are fresher than before. We have evidently tapped a level of new energy, masked until then by the fatigue-obstacle usually obeyed. There may be layer after layer of this experience. A third and a fourth 'wind' may supervene. Mental activity shows the phenomenon as well as physical, and in exceptional cases we may find, beyond the very extremity of fatigue-distress, amounts of ease and power that we never dreamed ourselves to own, sources of strength habitually not taxed at all, because habitually we never push through the obstruction, never pass those early critical points.

When we do pass, what makes us do so? Either some unusual stimulus fills us with emotional excitement, or some unusual idea of necessity induces us to make an extra effort of will. *Excitements, ideas,* *and efforts,* in a word, are what carry us over the dam.

In those hyperesthetic conditions which chronic invalidism so often brings in its train, the dam has changed its normal place. The pain-threshold is abnormally near. The slightest functional exercise gives a distress which the patient yields to and stops. In such cases of 'habit-neurosis' a new range of power often comes in consequence of the bullying-treatment, of efforts which the doctor obliges the patient, against his will, to make. First comes the very extremity of distress, then follows unexpected relief. There seems no doubt that we are each and all of us to some extent victims of habit-neurosis. We have to admit the wider potential range and the habitually narrow actual use. We live subject to inhibition by degrees of fatigue which we have come only from habit to obey. Most of us may learn to push the barrier farther off, and to live in perfect comfort on much higher levels of power.

Country people and city people, as a class, illustrate this difference. The rapid rate of life, the number of decisions in an hour, the many things to keep account of, in a busy city man's or woman's life, seem monstrous to a country brother. He doesn't see how we live at all. But settle him in town; and in a year or two, if not too old, he will have trained himself to keep the pace as well as any of us, getting more out of himself in any week then he ever did in ten weeks at home. The physiologists show how one can be in nutritive equilibrium, neither losing nor gaining weight, on astonishingly different quantities of food. So one can be in what I might call 'efficiency-equilibrium' (neither gaining nor losing power when once the equilibrium is reached), on astonishingly different quantities of work, no matter in what dimension the work may be measured. It may be physical work, intellectual work, moral work, or spiritual work.

Of course there are limits: the trees don't grow into the sky. But the plain fact remains that men the world over possess amounts of resource, which only very exceptional individuals push to their extremes of use.

The excitements that carry us over the usually effective dam are most often the classic emotional ones, love, anger, crowd-contagion, or despair. Life's vicissitudes bring them in abundance. A new position of responsibility, if it do not crush a man, will often, nay, one may say, will usually, show him to be a far stronger creature than was supposed. Even here we are witnessing (some of us admiring, some deploring—I must class myself as admiring) the dynamogenic effects of a very exalted political office upon the energies of an individual who had already manifested a healthy amount of energy before the office came.

Mr. Sydney Olivier has given us a fine fable of the dynamogenic effects of love in a late story called 'The Empire Builder,' in the *Contemporary Review* for May, 1905. A young naval officer falls in love at sight with a missionary's daughter on a lost island, which his ship accidentally touches. From that day onward he must see her again; and he so moves Heaven and earth and the Colonial Office and the Admiralty to get sent there once more, that the island finally is annexed to the empire in consequence of the various fusses he is led to make. People must have been appalled lately in San Francisco to find the stores of bottled up energy and endurance they possessed.

Wars, of course, and shipwrecks, are the great revealers of what men and women are able to do and bear. Cromwell's and Grant's careers are the stock examples of how war will wake a man up. I owe to

Professor Norton's kindness the permission to read to you part of a letter from Colonel Baird-Smith, written shortly after the six weeks' siege of Delhi in 1857, for the victorious issue of which that excellent officer was chiefly to be thanked. He writes as follows:

* * * My poor wife had some reason to think that war and disease between them had left very little of a husband to take under nursing when she got him again. An attack of camp-scurvy had filled my mouth with sores, shaken every joint in my body, and covered me all over with sores and livid spots so that I was marvelously unlovely to look upon. A smart knock on the ankle-joint from the splinter of a shell that burst in my face, in itself a mere bagatelle of a wound, had been of necessity neglected under the pressing and incessant calls upon me, and had grown worse and worse till the whole foot below the ankle became a black mass and seemed to threaten mortification. I insisted however on being allowed to use it till the place was taken, mortification or no; and though the pain was sometimes horrible, I carried my point and kept up to the last. On the day after the assault I had an unlucky fall on some bad ground, and it was an open question for a day or two whether I hadn't broken my arm at the elbow. Fortunately it turned out to be only a very severe sprain, but I am still conscious of the wrench it gave me. To crown the whole pleasant catalogue, I was worn to a shadow by a constant diarrhœa, and consumed as much opium as would have done credit to my father-in-law.[2] However, thank God I have a good share of Tapleyism in me and come out strong under difficulties. I think I may confidently say that no man ever saw me out of heart, or ever heard one croaking word from me even when our prospects were gloomiest. We were sadly scourged by the cholera and it was almost appalling to me to find that out of twenty-seven officers present, I could only muster fifteen for the operations of the attack. However, it was done, and after it was done came the collapse. Don't be horrified when I tell you that for the whole of the actual siege, and in truth for some little time before, I almost lived on brandy. Appetite for food I had none, but I forced myself to eat just sufficient to sustain life, and I had an incessant craving for brandy as the strongest stimulant I could get. Strange to say, I was quite unconscious of its affecting me in the slightest degree. *The excitement of the work was so great that no lesser one seemed to have any chance against it, and I certainly never found my intellect clearer or my nerves stronger in my life.* It was only my wretched body that was weak, and the moment the real work was done by our becoming complete masters of Delhi, I broke down without delay and discovered that if I wished to live I must continue no longer the system that had kept me up until the crisis was past. With it passed away as if in a moment all desire to stimulate, and a perfect loathing of my late staff of life took possession of me.

[2] [Thomas De Quincey.—W. J.]

Such experiences show how profound is the alteration in the manner in which, under excitement, our organism will sometimes perform its physiological work. The metabolisms become different when the reserves have to be used, and for weeks and months the deeper use may go on.

Morbid cases, here as elsewhere, lay the normal machinery bare. In the first number of Dr. Morton Prince's *Journal of Abnormal Psychology,* Dr. Janet has discussed five cases of morbid impulse, with an explanation that is precious for my present point of view. One is a girl who eats, eats, eats, all day. Another walks, walks, walks, and gets her food from an automobile that escorts her. Another is a dipsomaniac. A fourth pulls out her hair. A fifth wounds her flesh and burns her skin. Hitherto such freaks of impulse have received Greek names (as bulimia, dromomania, etc.) and been scientifically disposed of as 'episodic syndromata of hereditary degeneration.' But it turns out that Janet's cases are all what he calls psychasthenics, or victims of a chronic sense of weakness, torpor, lethargy, fatigue, insufficiency, impossibility, unreality, and powerlessness of will; and that in each and all of them the particular activity pursued, deleterious though it be, has the temporary result of raising the sense of vitality and making the patient feel alive again. These things reanimate; they would reanimate *us;* but it happens that in each patient the particular freak-activity chosen is the only thing that does reanimate; and therein lies the morbid state. The way to treat such persons is to discover to them more usual and useful ways of throwing their stores of vital energy into gear.

Colonel Baird-Smith, needing to draw on altogether extraordinary stores of energy, found that brandy and opium were ways of throwing them into gear.

Such cases are humanly typical. We are all to some degree oppressed, unfree. We don't come to our own. It is there, but we don't get at it. The threshold must be made to shift. Then many of us find that an excentric activity—a 'spree,' say—relieves. There is no doubt that to some men sprees and excesses of almost any kind are medicinal, temporarily at any rate, in spite of what the moralists and doctors say.

But when the normal tasks and stimulations of life don't put a man's deeper levels of energy on tap, and he requires distinctly deleterious excitements, his constitution verges on the abnormal. The normal opener of deeper and deeper levels of energy is the will. The difficulty is to use it; to make the effort which the word volition implies. But if we *do* make it (or if a god, though he were only the god Chance, makes it through us), it will act dynamogenically on us for a month. It is notorious that a single successful effort of moral volition, such as saying 'no' to some habitual temptation, or performing some courageous act, will launch a man on a higher level of energy for days and weeks, will give him a new range of power.

The emotions and excitements due to usual situations are the usual inciters of the will. But these act discontinuously; and in the intervals the shallower levels of life tend to close in and shut us off. Accordingly the best practical knowers of the human soul have invented the thing known as methodical ascetic discipline to keep the deeper levels constantly in reach. Beginning with easy tasks, passing to harder ones, and exercising day by day, it is, I believe, admitted that disciples of asceticism can reach very high levels of freedom and power of will.

Ignatius Loyola's spiritual exercises must have produced this result in innumerable devotees. But the most venerable ascetic system, and the one whose results have the most voluminous experimental corroboration is undoubtedly the Yoga system in Hindostan. From time immemorial, by Hatha Yoga, Raja Yoga, Karma Yoga, or whatever code of practise it might be, Hindu aspirants to perfection have trained themselves, month in and out, for years. The result claimed, and certainly in many cases accorded by impartial judges, is strength of character, personal power, unshakability of soul. But it is not easy to disentangle fact from tradition in Hindu affairs. So I am glad to have a European friend who has submitted to Hatha Yoga training, and whose account of the results I am privileged to quote. I think you will appreciate the light it throws on the question of our unused reservoirs of power.

My friend is an extraordinarily gifted man, both morally and intellectually, but has an instable nervous system, and for many years has lived in a circular process of alternate lethargy and over-animation: something like three weeks of extreme activity, and then a week of prostration in bed. An unpromising condition, which the best specialists in Europe had failed to relieve; so he tried Hatha Yoga, partly out of curiosity, and partly with a sort of desperate hope. What follows is a short extract from a letter sixty pages long which he addressed me a year ago:

Thus I decided to follow Vivekananda's advice: "Practise hard: whether you live or die by it doesn't matter." My improvised chela and I began with starvation. I do not know whether you did try it ever * * * but voluntary starvation is very different from involuntary, and implies more temptations. We reduced first our meals to twice a day and then to once a day. The best authorities agree that in order to control the body fasting is essential, and even in the Gospel the worst spirits are said to obey only those who fast and pray. We reduced very much the amount of food, disregarding chemical theories about the need of albumen, sometimes living on olive oil and bread; or on fruits alone; or on milk and rice; in very small quantities—much less than I formerly ate at one meal. I began to get lighter every day, and lost 20 pounds in a few weeks; but this could not stop such a desperate undertaking * * * rather starve than live as a slave! Then besides we practised *asana* or postures, breaking almost our limbs. Try to sit down on the floor and to kiss your knees without bending them, or to join your hands on the usually unapproachable upper part of your back, or to bring the toe of your right foot to your left ear without bending the knees * * * these are easy samples of posture for a Yogi.

All the time also breathing exercises: keeping the breath in and out up to two minutes, breathing in different rhythms and positions. Also very much prayer and Roman Catholic practises combined with the Yoga, in order to leave nothing untried and to be protected against the tricks of Hindu devils! Then concentration of thought on different parts of the body, and on the processes going on within them. Exclusion of all emotions, dry logical reading, as intellectual diet, and working out logical problems. * * * I wrote a Handbook of Logic as a *Nebenprodukt* of the whole experiment.[3]

After a few weeks I broke down and had to interrupt everything, in a worse state of prostration than ever. * * * My younger chela went on unshaken by my fate; and as soon as I arose from bed I tried again, decided to fight it out, even feeling a kind of determination such as I had never felt before, a certain absolute will of victory at any price and faith in it. Whether it is my own merit or a divine grace, I can not judge for certain, but I prefer to admit the latter. I had been ill for seven years, and some people say this is a term for many punishments. However base and vile a sinner I had been, perhaps my sins were about to be forgiven, and Yoga was only an exterior opportunity, an object for concentration of will. I do not yet pretend to explain much of what I have gone through, but the fact is that since I arose from bed on August 20, no new crisis of prostration came again, and I have now the strongest conviction that no crisis will ever return. If you consider that for the past years there has not been a single month without this lethargy, you will grant that even to an outside observer four successive months of increasing health are an objective test. In this time I underwent very severe penances, reducing sleep and food and increasing the task of work and exercise. My intuition was developed by these practises: there came a sense of certainty, never known before, as to the things needed by the body and the mind, and the body came to obey like a wild horse tamed. Also the mind learned to obey, and the current of thought and feeling was shaped according to my will. I mastered sleep and hunger, and the flights of thought, and came to know a peace never known before, an inner rhythm of unison with a deeper rhythm above or beyond. Personal wishes ceased, and the consciousness of being the instrument of a superior power arose. A calm certainty of indubitable success in every undertaking imparts great and real power. I often guessed the thoughts of my companion * * * we observed generally the greatest isolation and silence. We both felt an unspeakable joy in the simplest natural impressions, light, air, landscape, any kind of simplest food; and above everything in rhythmical respiration, which produces a state of mind without thought or feeling, and still very intense, indescribable.

These results began to be more evident in the fourth month of uninterrupted training. We felt quite happy, never tired, sleeping only from 8 P.M. to midnight, and rising with joy from our sleep to another day's work of study and exercise. * * *

I am now in Palermo, and have had to neglect the exercises in the last few days, but I feel as fresh as if I were in full training and see the sunny side of all things. I am not in a hurry, rushing to complete ——.

And here my friend mentions a certain life-work of his own about which I had

[3] This handbook was published last March.—W. J.

better be silent. He goes on to analyze the exercises and their effects in an extremely practical way, but at too great length for me to entertain you with. Repetition, alteration, periodicity, parallelism (or the association of the idea of some desirable vital or spiritual effect with each movement), etc., are laws which he deems highly important. "I am sure," he continues, "that everybody who is able to concentrate thought and will, and to eliminate superfluous emotions, sooner or later becomes a master of his body and can overcome every kind of illness. This is the truth at the bottom of all mind-cures. Our thoughts have a plastic power over the body."

You will be relieved, I doubt not, to hear my excentric correspondent here make connection at last with something you know by heart, namely, 'suggestive therapeutics.' Call his whole performance, if you like, an experiment in methodical self-suggestion. That only makes it more valuable as an illustration of what I wish to impress in as many ways as possible upon your minds, that we habitually live inside our limits of power. Suggestion, especially under hypnosis, is now universally recognized as a means, exceptionally successful in certain persons, of concentrating consciousness, and, in others, of influencing their bodies' states. It throws into gear energies of imagination, of will, and of mental influence over physiological processes, that usually lie dormant, and that can only be thrown into gear at all in chosen subjects. It is, in short, dynamogenic; and the cheapest terms in which to deal with our amateur Yogi's experience is to call it auto-suggestive.

I wrote to him that I couldn't possibly attribute any sacramental value to the particular Hatha Yoga processes, the postures, breathings, fastings and the like, and that they seemed to me but so many manners, available in his case and his chela's, but not for everybody, of breaking through the barriers which life's routine had concreted round the deeper strata of the will, and gradually bringing its unused energies into action.

He replied as follows:

> You are quite right that the Yoga exercises are nothing else than a methodical way of increasing our will. Because we are unable to will at once the most difficult things, we must imagine steps leading to them. Breathing being the easiest of the bodily activities, it is very natural that it offers a good scope for exercise of will. The control of thought could be gained without breathing-discipline, but it is simply easier to control thought simultaneously with the control of breath. Anyone who can think clearly and persistently of one thing needs not breathing exercises. You are quite right that we are not using all our power and that we often learn how much we *can* only when we *must*. * * * The power that we do not use up completely can be brought [more and more] into use by what we call *faith*. Faith is like the manometer of the will, registering its pressure. If I could believe that I can levitate, I could do it. But I can not believe, and therefore I am clumsily sticking to earth. * * * Now this faith, this power of credulity, can be educated by small efforts. I can breathe at the rate of say twelve times a minute. I can easily believe that I can breathe ten times a minute. When I have accustomed myself to breathe ten times a minute, I learn to believe it will be easy to breathe six times a minute. Thus I have actually learned to breathe at the rate of once a minute. How far I shall progress I do not know. * * * The Yogi goes on in his activity in an even way, without fits of too much or too little, and he is eliminating more and more every unrest, every worry—growing into the infinite by regular training, by small additions to a task which has grown familiar. * * * But you are quite right that religious-crises, love-crises, indignation-crises, may awaken in a very short time powers similar to those reached by years of patient Yoga practise. * * * The Hindus themselves admit that Samadhi can be reached in many ways and with complete disregard of every physical training.

Allowance made for every enthusiasm and exaggeration, there can be no doubt of my friend's regeneration—relatively, at any rate. The second letter, written six months later than the first (ten months after beginning Yoga practise, therefore), says the improvement holds good. He has undergone material trials with indifference, traveled third-class on Mediterranean steamers, and fourth-class on African trains, living with the poorest Arabs and sharing their unaccustomed food, all with equanimity. His devotion to certain interests has been put to heavy strain, and nothing is more remarkable to me than the changed moral tone with which he reports the situation. Compared with certain earlier letters, these read as if written by a different man, patient and reasonable instead of vehement, self-subordinating instead of imperious. The new tone persists in a communication received only a fortnight ago (fourteen months after beginning training)—there is, in fact, no doubt that profound modification has occurred in the running of his mental machinery. The gearing has changed, and his will is available otherwise than it was. Available without any new ideas, beliefs, or emotions, so far as I can make out, having been implanted in him. He is simply more balanced where he was more unbalanced.

You will remember that he speaks of faith, calling it a 'manometer' of the will. It sounds more natural to call our will the manometer of our faiths. Ideas set free beliefs, and the beliefs set free our wills (I use these terms with no pretension to be 'psychological'), so the will-acts register the faith-pressure within. Therefore, having considered the liberation of our stored-up energy by emotional excitements and by efforts, whether methodical or unmethodical, I must now say a word about *ideas* as our third great dynamogenic agent. Ideas contradict other ideas and keep us from believing them. An idea that thus negates a first idea may itself in turn be negated by a third idea, and the first idea may thus regain its natural influence over our belief and determine our behavior. Our philosophic and religious development proceeds thus by credulities, negations and the negating of negations.

But whether for arousing or for stopping belief, ideas may fail to be efficacious, just as a wire at one time alive with electricity, may at another time be dead. Here our insight into causes fails us, and we can only note results in general terms. In general, whether a given idea shall be a live idea, depends more on the person into whose mind it is injected than on the idea itself. The whole history of 'suggestion' opens out here. Which are the suggestive ideas for this person, and which for that? Beside the susceptibilities determined by one's education and by one's original peculiarities of character, there are lines along which men simply as men tend to be inflammable by ideas. As certain objects naturally awaken love, anger, or cupidity, so certain ideas naturally awaken the energies of loyalty, courage, endurance, or devotion. When these ideas are effective in an individual's life, their effect is often very great indeed. They may transfigure it, unlocking innumerable powers which, but for the idea, would never have come into play. 'Fatherland,' 'The Union,' 'Holy Church,' the 'Monroe Doctrine,' 'Truth,' 'Science,' 'Liberty,' Garibaldi's phrase 'Rome or Death,' etc., are so many examples of energy-releasing abstract ideas. The *social* nature of all such phrases is an essential factor of their dynamic power. They are forces of detent in situations in which no other force produces equivalent effects, and each is a force of detent only in a specific group of men.

The memory that an oath or vow has been made will nerve one to abstinences and efforts otherwise impossible: witness the 'pledge' in the history of the temperance movement. A mere promise to his sweetheart will clean up a youth's life all over—at any rate for a time. For such effects an educated susceptibility is required. The idea of one's 'honour,' for example, unlocks energy only in those who have had the education of a gentleman, so called.

That delightful being, Prince Pueckler-Muskau, writes to his wife from England that he has invented "a sort of artificial resolution respecting things that are difficult of performance." "My device," he says, "is this: I give my word of honour most solemnly to myself to do or to leave undone this or that. I am of course extremely cautious in the use of this expedient, but when once the word is given, even though I afterwards think I have been precipitate or mistaken, I hold it to be perfectly irrevocable, whatever inconveniences I foresee likely to result. If I were capable of breaking my word after such mature consideration, I should lose all respect for myself—and what man of sense would not prefer death to such an alternative? * * * When the mysterious formula is pronounced, no alteration in my own views, nothing short of physical impossibility, must, for the welfare of my soul, alter my will. * * * I find something very satisfactory in the thought that man has the power of framing such props and weapons out of the most trivial materials, indeed out of nothing, merely by the force of his will, which thereby truly deserves the name of omnipotent."[4]

Conversions, whether they be political, scientific, philosophic, or religious, form another way in which bound energies are let loose. They unify, and put a stop to ancient mental interferences. The result is freedom, and often a great enlargement of power. A belief that thus settles upon an individual always acts as a challenge to his will. But, for the particular challenge to operate, he must be the right challeng*ee*. In religious conversions we have so fine an adjustment that the idea may be in the mind of the challengee for years before it exerts effects; and why it should do so then is often so far from obvious that the event is taken for a miracle of grace, and not a natural occurrence. Whatever it is, it may be a highwater mark of energy, in which 'noes,' once impossible, are easy, and in which a new range of 'yeses' gain the right of way.

We are just now witnessing—but our scientific education has unfitted most of us for comprehending the phenomenon—a very copious unlocking of energies by ideas, in the persons of those converts to 'New Thought,' 'Christian Science,' 'Metaphysical Healing,' or other forms of spiritual philosophy, who are so numerous among us to-day. The ideas here are healthy-minded and optimistic; and it is quite obvious that a wave of religious activity, analogous in some respects to the spread of early Christianity, Buddhism and Mohammedanism is passing over our American world. The common feature of these optimistic faiths is that they all tend to the suppression of what Mr. Horace Fletcher has termed 'fearthought.' Fearthought he defines as 'the self-suggestion of inferiority'; so that one may say that these systems all operate by the suggestion of power. And the power, small or great, comes in various shapes to the individual, power, as he will tell you, not to 'mind' things that used to vex him, power to concentrate his mind, good cheer, good temper; in short, to put it mildly, a firmer, more elastic moral tone. The most genuinely saintly person I have ever known is a friend of mine now suffering from cancer of the breast. I do not assume to judge

[4] 'Tour in England, Ireland and France,' Philadelphia, 1833, p. 435.

of the wisdom or unwisdom of her disobedience to the doctors, and I cite her here solely as an example of what ideas can do. Her ideas have kept her a practically well woman for months after she should have given up and gone to bed. They have annulled all pain and weakness and given her a cheerful active life, unusually beneficent to others to whom she has afforded help.

How far the mind-cure movement is destined to extend its influence, or what intellectual modifications it may yet undergo, no one can foretell. Being a religious movement, it will certainly outstrip the purviews of its rationalist critics, such as we here may be supposed to be.

I have thus brought a pretty wide induction to bear upon my thesis, and it appears to hold good. The human individual lives usually far within his limits; he possesses powers of various sorts which he habitually fails to use. He energizes below his maximum, and he behaves below his optimum. In elementary faculty, in coordination, in power of inhibition and control, in every conceivable way, his life is contracted like the field of vision of an hysteric subject—but with less excuse, for the poor hysteric is diseased, while in the rest of us it is only an inveterate *habit*—the habit of inferiority to our full self—that is bad.

Expressed in this vague manner, everyone must admit my thesis to be true. The terms have to remain vague; for though every man of woman born knows what is meant by such phrases as having a good vital tone, a high tide of spirits, an elastic temper, as living energetically, working easily, deciding firmly, and the like, we should all be put to our trumps if asked to explain in terms of scientific psychology just what such expressions mean. We can draw some child-like psychophysical diagrams, and that is all. In physics the conception of 'energy' is perfectly defined. It is correlated with the conception of 'work.' But mental work and moral work, although we can not live without talking about them, are terms as yet hardly analyzed, and doubtless mean several heterogeneous elementary things. Our muscular work is a voluminous physical quantity, but our ideas and volitions are minute forces of release, and by 'work' here we mean the substitution of higher *kinds* for lower *kinds* of detent. Higher and lower here are qualitative terms, not translatable immediately into quantities, unless indeed they should prove to mean newer or older forms of cerebral organization, and unless newer should then prove to mean cortically more superficial, older, cortically more deep. Some anatomists, as you know, have pretended this; but it is obvious that the intuitive or popular idea of mental work, fundamental and absolutely indispensable as it is in our lives, possesses no degree whatever of scientific clearness to-day.

Here, then, is the first problem that emerges from our study. Can any one of us refine upon the conceptions of mental work and mental energy, so as later to be able to throw some definitely analytic light on what we mean by 'having a more elastic moral tone,' or by 'using higher levels of power and will'? I imagine that we may have to wait long before progress in this direction is made. The problem is too homely; one doesn't see just how to get in the electric keys and revolving drums that alone make psychology scientific to-day.

My fellow-pragmatist in Florence, G. Papini, has adopted a new conception of philosophy. He calls it the *doctrine of action* in the widest sense, the study of all human powers and means (among which latter, *truths* of every kind whatsoever figure, of course, in the first rank). From this point of view philosophy is a *prag-*

matic, comprehending, as tributary departments of itself, the old disciplines of logic, metaphysic, physic and ethic.

And here, after our first problem, two other problems burst upon our view. My belief that these two problems form a program of work well worthy of the attention of a body as learned and earnest as this audience, is, in fact, what has determined me to choose this subject, and to drag you through so many familiar facts during the hour that has sped.

The first of the two problems is *that of our powers*, the second *that of our means of unlocking them or getting at them.* We ought somehow to get a topographic survey made of the limits of human power in every conceivable direction, something like an ophthalmologist's chart of the limits of the human field of vision; and we ought then to construct a methodical inventory of the paths of access, or keys, differing with the diverse types of individual, to the different kinds of power. This would be an absolutely concrete study, to be carried on by using historical and biographical material mainly. The limits of power must be limits that have been realized in actual persons, and the various ways of unlocking the reserves of power must have been exemplified in individual lives. Laboratory experimentation can play but a small part. Your psychologist's *Versuchsthier*, outside of hypnosis, can never be called on to tax his energies in ways as extreme as those which the emergencies of life will force on him.

So here is a program of concrete individual psychology, at which anyone in some measure may work. It is replete with interesting facts, and points to practical issues superior in importance to anything we know. I urge it therefore upon your consideration. In some shape we have all worked at it in a more or less blind and fragmentary way; yet before Papini mentioned it I had never thought of it, or heard it broached by anyone, in the generalized form of a program such as I now suggest, a program that might with proper care be made to cover the whole field of psychology, and might show us parts of it in a very fresh light.

It is just the generalizing of the problem that seems to me to make so strong an appeal. I hope that in some of you the conception may unlock unused reservoirs of investigating power.

William James

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE EXPANSION OF PHYSIOLOGY[1]

Looking forward into the far future, we may perhaps dimly discern the day when morphology and physiology will again join hands * * * but that day is as yet most distant.

When Dr. Michael Foster, the eminent physiologist, was writing the lines quoted above, the two grand divisions of biology to which he refers seemed separated as if by a great gulf. In England and America morphology was the reigning favorite and in the higher institutions of learning physiology as such hardly existed. Both zoology and botany had come almost everywhere to mean morphology, and morphological problems were the popular themes of the day. Even in medical schools, physiology was as yet generally denied an independent existence, being commonly appended to or combined with the chair of anatomy, *i. e.*, one of morphology.

Dr. Foster was writing in the early eighties, and those who like myself can recall the conditions of biological teaching and research at that time will testify that his words were justified. It is true that a

[1] Address of the vice-president and chairman of Section K—Physiology and Experimental Medicine, New York meeting, 1906.

full professorship of physiology had been established in the Harvard Medical School in 1876, and that a brilliant young physiologist, one of Foster's own pupils, in the same year assumed the professorship of biology at Johns Hopkins. And yet I distinctly remember how strange and how full of chemistry and physics the first edition of Foster's physiology seemed as it was shown to me in 1876 by my professor of comparative anatomy, and also how two years later I studied physiology in the medical school of one of our leading universities for a whole season, under a young physician of more than ordinary ability and promise, without once seeing a demonstration—still less doing an experiment. The class simply recited,—upon so many pages of Dalton's 'Human Physiology.'

All this was the more remarkable because, according to Dr. (now President) G. Stanley Hall, physiology was at that very time "'The' German Science"—a fact stated and emphasized by Dr. Hall by the title of one of a series of contemporary essays which, re-read to-day, almost cause one to regret that the author abandoned the career of literature for that of administration. (Cf. 'Aspects of German Culture,' by G. Stanley Hall, 1881.)

Physiology, says Dr. Hall, has been characterized as just now preeminently *the* German science. This is probably true, whether it means that German physiologic methods and results are less known in other countries than those of other sciences, or that they reflect more peculiarly the national characteristics. Till Foster's text-book appeared, very little was known in England and America of German physiology, save by specialists who themselves had studied in Germany. * * * Fick terms physiology 'the highest and most fruitful generalization of the collective natural sciences.' Czermak, who devoted his wealth to building and equipping a magnificent laboratory and lecture room and his time to the end of his life to the popularization of physiology, was never weary of insisting that it should be taught in every high school. Once more, evolution in the sense of Darwin or Haeckel is far from being a finality for the physiologist. It is for him rather a morphological assumption that all animals and men belong to one family; and he defines his science with Pflüger as the chemistry and physics of living matter.

Physiology in Great Britain and America had, in fact, so far lagged behind that the publication of Foster's text-book with its revelation of some of the German physiology of the day created a real sensation. In the English-speaking countries morphology was everywhere the fashion and biologists, whether botanists or zoologists, were then, almost without an exception, morphologists. Even ten years after Foster's book appeared, Huxley, who had himself previously defined the grand divisions of biology as morphology and physiology rather than zoology and botany, speaks of zoology, in his Queen's jubilee essay on 'The Progress of Science' (1887), as if this were really morphology when he writes: "It is only in the present epoch that zoology and physiology have yielded any great aid to pathology and hygiene."

When Foster's text-book appeared descriptive zoology and embryology were already rivals in popularity, and the appearance in 1880 of Balfour's 'Comparative Embryology' made this subject for zoologists almost a passion. And yet to-day Balfour's then fascinating work seems strangely descriptive and somewhat overanxious after merely structural homologies. In his 'Introduction' Balfour, while defining embryology as covering "the anatomy and physiology of the organism during the whole period included between its first coming into being and its attainment of the adult state," is careful to add: "The present treatise deals only with the embryology of animals, and the science is moreover treated from the morphological or anatomical rather than the physiological side." So much was embryology the fashion of the day that Foster himself issued with Balfour a well-known volume on the 'Embryology of the Chick,' and this too as a morphological, not a physiological, treatise. In view of these evidences of Balfour's preoccupation with morphological problems, it is interesting to learn from Dr. A. C. Haddon, one of his students, that Balfour always looked upon this preoccupation as temporary and that he intended to devote himself eventually to comparative physiology. Huxley, again, omitted all reference to physiology in embryology when in 1878 he defined the latter as 'an account of the anatomy of a living being at the successive periods of its existence, and of the manner in which one anatomical stage passes into the next.' And yet he incidentally recognized the equality of physiology and morphology by remarking that "geology is, as it were, the biology of our planet as a whole. In so far as it comprises the surface configuration and the inner structure of the earth it answers to morphology; in so far as it studies changes of condition and their causes it corresponds with physiology."

This supremacy of morphology continued well on into the nineties, but about ten or twelve years ago signs of a change began to appear, and no one who has observed even superficially the progress of biology during the last decade can have failed to perceive an immense and increasing interest in general and comparative physiology, accompanied by a decline of interest, relatively speaking, in pure morphology. Investigations in chemical physiology, mental physiology, embryological physiology, cytological physiology, comparative physiology, and in the general physiology of the response and the behavior of animals, have rapidly come to the front, while the field of vegetable physiology is being cultivated as never before. In its various aspects general physiology is to-day probably receiving from investigators more attention than special or mammalian (including human) physiology, and displacing in the hands of zoologists, to a remarkable extent, more strictly morphological studies of a systematic, phylogenetic or ontogenetic character.

Twenty years ago to be a zoologist meant to be a morphologist, but to-day many professors of zoology are either becoming or have already become veritable physiologists. Most of the 'experimental zoology' and 'embryology' of the present is really general physiology. So also are large parts of physiological chemistry, physiological psychology, cytology, protozoology, microbiology and bacteriology. Hygiene, climatology, experimental medicine, pharmacology, and many other modern branches of biology are also chiefly physiological rather than morphological. Foster's guarded prophecy of 1885 had an almost hopeless tone, for he put 'most distant,' and in 'the far future,' the day when 'perhaps' morphology and physiology will come together once more; and here again, for the thousandth time, prediction touching the future of science has proved to be empty and vain—for scarcely had a score of years gone by before Foster's 'most distant' day was already brightly dawning, and physiology and morphology were again 'joining hands' in experimental zoology. So far, indeed has this movement extended that even the general biologist may now claim the workers in the newer fields as immigrants into his own, pointing with pride to the breadth and depth of their work as justifying that still older idea of physiology in which it was essentially what we now call 'biology'; or even that oldest idea of all, in which physiology was the equivalent of the *ultima thule* of all these sciences, 'natural philosophy'—a term hallowed on its mathematical side by the name of Isaac Newton, and in its entirety reaching back to the pupils of Aristotle.

That the recent expansion of physiology is not really a new departure but rather a return to an older as well as a more normal condition, is another interesting fact. In 1854 there was published in London, and republished in America, the fourth edition of a thick volume of 700 pages by Dr. W. B. Carpenter, on 'Comparative Physiology,' an examination of which shows that the zoologists of that day were fully alive to many of the very problems upon which so many of our modern zoologists are engaged at the present time. The first thing that strikes us in this work is the fact that plants and animals, some high and some low in the scale of life, are equally considered, and always side by side. It is, therefore, really a treatise on general biology. The next is that physiology ordinarily so called, that is to say human physiology, is nowhere much in evidence. We also find that functions, rather than organs, are dwelt upon, and the general functions of organisms—such as alimentation, nutrition, reproduction and the liberation of heat, light and electricity—as well as the special functions of organs—absorption, circulation, respiration, and the like; the general functions always in plants as well as animals. It was therefore truly a *comparative* physiology.

Dr. Carpenter's work was published the year before I was born, but when, as a special student of biology twenty years later, I began the study of physiology, the book was never mentioned and the subject never touched upon,—both being apparently little esteemed if not actually forgotten. It was very likely this work that Foster had in mind when he wrote in the context to the passage quoted at the outset of this article: "In its more general meaning physiology was largely used of old, and is still occasionally used in popular writings, to denote an inquiry into the nature of living beings. * * * In its older sense * * * (it) corresponded to what is now called biology." Recalling the fundamental, original and epoch-making 'Handbuch' of Johannes Müller published in Germany between 1834 and 1840 (and in English from 1837) and Carpenter's textbook just referred to, on 'Comparative Physiology,' published in England and America in 1854, we are compelled to regard the present remarkable development of physiology as not merely an expansion, but also a renascence or revival—a return, as it were, to an earlier normal.

The question naturally arises, How did it happen that general and comparative physiology, after beginnings so brilliant, was virtually eclipsed from the time of Carpenter to that of Verworn—(for Claude Bernard's 'Leçons sur les phénomènes de la vie communs aux animaux et aux végétaux' published in 1878, had at the time very little general effect, and were hardly more than a succès d'estime). Why was it, we may well inquire, that the pendulum of biological research and teaching swung so far over to the morphological side, while mammalian and medical (or human) physiology rapidly advanced—at least in Germany—separated itself from anatomy, a branch of morphology, and secured for itself important and independent recognition with sustaining professorships?

To this question the answer is, I think, extremely simple—the dates mentioned, and the fact that the phenomenon was most marked in English-speaking countries, giving us the proper clue. In all probability the rapid rise of interest in general morphology and the corresponding neglect of general physiology after 1860 were alike due to the almost complete and universal absorption of biologists, and especially English-speaking biologists, in the problem of the origin of species. For getting light upon this all-important problem, studies in morphological embryology, in comparative anatomy and in systematic zoology and botany were simply indispensable as sources of evidence bearing upon the doctrine of descent, and hence studies ontogenetic and phylogenetic, rather than physiologic, were for the time being enthusiastically and almost exclusively pursued. That medical physiology did not suffer a similar total eclipse by morphology, but rather continued to advance (at least in Germany) until in 1881 Stanley Hall could describe it as "'The' German Science," was obviously because of its technical importance to medicine. That it flourished in Germany and not in England was doubtless due partly to the persisting influence of Johannes Müller's great work and partly to the fact that the struggles over Darwinism were severest and most distracting on English soil.

Whatever the reason, it had somehow come to pass that in the eighties there was nowhere any physiology to speak of outside the medical schools (while that inside these schools was often of the poorest) and that my own generation grew up almost totally ignorant of general and comparative physiology. If the reason, as seems likely, was the rise and all-embracing influence of Darwinism we may perhaps be pardoned if Verworn's innocent remark, that the doctrine of descent has not thus far 'been fruitful in physiology' seems to some of us so far within the truth as not to touch it. But at last, when, after nearly forty years, descriptive embryology and the phylogeny of animals and plants had been well worked out, and when even the noise of the great struggle over the origin of species had mostly died away, opportunity came for that remarkable expansion of physiology which we are now witnessing and which, if I am right, is not merely an expansion, but a renascence. It is a renascence, however, not like the great period of that name in history, preceded by a dark or middle age in general knowledge, for between 1854 and 1894 a splendid development of all other sciences had taken place, theological bonds had been broken, and the freedom of speech and of research enlarged and strengthened. Chemistry and physics had wonderfully expanded and developed and were ready to shed new light upon physiological processes, so that we might say once more, and may to-day repeat with renewed confidence, what George Henry Lewes said before the eclipse—"The hope of science at the present day is to express all phenomena in terms of dynamics."

As for the importance of the revival and of the recent expansion of physiology for biology, making of the latter once more that rounded whole—*totus teres atque rotundus*—which it ought to be, it is difficult to exaggerate. If, as we believe, biology is only the chemistry and physics of living matter, and if our hope 'is to express all phenomena in terms of dynamics,' we cannot but rejoice that having accumulated within the last fifty years a vast and precious store of morphological material we may now pass on to the investigations of questions of the relation, causation and coordination of activities; of processes rather than homologies, of behavior rather than form, of mechanism rather than framework. I am informed by an excellent authority that a similar tendency is apparent in medicine itself, and that to-day the processes rather than the results of disease occupy the center of interest in pathology.

Clerk Maxwell long ago remarked concerning biology, that "sciences of this kind are rich in facts, and will be well occupied for ages to come in the coordination of these facts." Surely it is a matter for rejoicing that physiology is to-day dealing with a wider range of facts than ever before. It is no longer confined within medical schools, in which mammalian, or at least vertebrate, facts must always be of paramount importance, for, as has been shown above, zoologists and botanists in our universities and colleges are turning their attention to the behavior and activities of the lower forms of life, both plant and animal. It is, however, unfortunate that the beginner still generally finds no physiology, under that name, offered in our higher institutions of learning outside the medical schools, in which physiology is necessarily, and rightly enough, influenced to a great degree by the needs of technical students, since—as Huxley said long ago—"a medical school is a technical school: a school in which a practical profession is taught." For while physiologists have abundantly demonstrated that pure science may thrive in a technical atmosphere, still, it must always be true that in a technical school the applications of science will be most in demand, and hence most influential. Instead of reproving medical physiologists for their failure to cover the whole field we ought, however, to be grateful to them for having stuck to their guns so devotedly after all other physiologists, both general and comparative, had deserted the field and followed after morphological gods, such as embryology, homology and phylogeny. At last, however, the zoologists and botanists are returning to their own and taking up their old work. It is greatly to be hoped that some of these may eventually come to acknowledge themselves physiologists, and that, very soon, students of biology in our higher institutions of learning, whether zoologists or botanists, may have offered to them equal opportunities in general or comparative morphology and general or comparative physiology. (An excellent *résumé* of present tendencies may be found in the various addresses in Vol. V., Congress of Arts and Science, Universal Exposition. St. Louis, 1906.)

Meantime, what is most important is to realize that physiology is still and always will be one of the two grand divisions of biology; that it offers, to-day, especially in its general and its comparative divisions, a field white for the harvest and—what makes it still more inviting—one mostly unworked since the publication of the origin of species. When we realize these facts and also what a wealth of new knowledge the progress of other sciences such as chemistry and physics has placed at our disposal since 1860, it is clear that to general physiology we may probably look in the immediate future for the greatest advances in biology. We have already got from medical physiologists the broad outlines of the physiology of animal organs and from plant physiologists of the organs of plants; we are getting from the experimental zoologists—and particularly the embryologists and cytologists—the physiology of animal cells—including various protoplasms. Our next great advance must come in the physiology of organisms as wholes, and that not merely of the lower organisms, but of the higher also. In this direction studies on nutrition are already beginning to tell, and epidemiology has much to teach. When climatology—a science of rare possibilities—and the numerous divisions of public hygiene shall have 'coordinated their facts,' we shall have at least the groundwork of a complete physiology of the higher organisms.

The discussion which is to follow—a discussion possible indeed only after morphology had cleared the way—should afford in a consideration of the actions and reactions between parasitic protozoa and mankind, an excellent example of the broader general physiology of to-day and to-morrow.

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THE AMERICAN FEDERATION OF TEACHERS OF THE MATHEMATICAL AND THE NATURAL SCIENCES

In accordance with a call, issued by joint action of a committee of the American Society of Teachers of Mathematics and the Natural Sciences and one of the Central Association of Science and Mathematics Teachers, a meeting of delegates of a number of associations was held in New York on December 27, 1906, for the purpose of discussing the formation of a federation of associations of teachers of science and mathematics. A roll of the meeting was taken, and it was found that there were present 27 delegates, representing seven associations, as follows: The Association of Teachers of Mathematics of the Middle States and Maryland, 9 delegates; the New York State Science Teachers' Association (Mathematics Section), 6 delegates; the Central Association of Science and Mathematics Teachers, 5 delegates; the Association of the Teachers of Mathematics of New England, 3 delegates; the Association of the Teachers of Physics of Washington City, 2 delegates; the Missouri Society of Teachers of Mathematics and Science, 1 delegate; the New Jersey State Science Teachers' Association, 1 delegate.

Professor T. S. Fiske, of the Association of Teachers of Mathematics of the Middle States and Maryland, was elected chairman of the meeting, and Professor C. R. Mann, of the Central Association of Science and Mathematics Teachers, was made secretary.

After some preliminary discussion, it was, on motion duly seconded, unanimously voted:

That it is recommended that there be formed, by the various associations of teachers of science and of mathematics, an 'American Federation of Teachers of the Mathematical and the Natural Sciences.'

The question of the form of the organization was then taken up. Two different forms were proposed: one, that of a single society of teachers of mathematics and the mathematical sciences, the membership to be limited to associations that publish literature and reports; the other, a rather loose federation of all associations of teachers of either mathematical or natural sciences, the membership being limited to associations that have more than fifty members. The first of these forms was that adopted by the American Society of Teachers of Mathematics and the Natural Sciences at the conference held at Asbury Park in 1905. The latter form was proposed by the Central Association of Science and Mathematics Teachers.

In the discussion of this question, the latter form of organization was shown to be less formal and more flexible and to interfere less with the individual activities of the associations. Because this form of federation appeared to furnish the necessary basis for a first step toward a more complete organization, and because it was considered advisable that associations not represented at the meeting should have a voice in the final decision, it was, on motion duly seconded, unanimously voted:

That the form of organization proposed by the Central Association of Science and Mathematics Teachers in the printed circular issued by them be tentatively adopted for the coming year, the final form of organization to be decided at the next meeting.

No officers were elected; but an executive committee, which should look after the formation and development of the Federation pending permanent organization, was elected as follows: T. S. Fiske, Columbia University, *chairman;* C. R. Mann, University of Chicago, *secretary-treasurer;* H. W. Tyler, Massachusetts Institute of Technology; R. E. Dodge, Teachers College, New York; F. N. Peters, Kansas City High School.

On motion duly seconded, it was voted: That this executive committee have power to fill vacancies and to add to its membership by unanimous vote.

On motion, the meeting adjourned, subject to the call of the executive committee.

C. R. MANN,
Secretary

SCIENTIFIC BOOKS

Animal Micrology: Practical Exercises in Microscopical Methods. By MICHAEL F. GUYER, Ph.D., Professor of Zoology in the University of Cincinnati. Chicago, The University of Chicago Press. 1906.

This little book of 240 pages is devoted to a concise, eminently practical and well-classified treatment of the methods and 'tricks' of convenience fundamental to modern microscopic study. While it is intended primarily for the beginner, its consultation will be found profitable to all of us who have to suffer the trials and time-consuming details of microscopical technique. The author's years of experience in giving instruction in general zoology and microscopic anatomy, combined with his marked ingenuity in mechanical and chemical manipulation, has resulted, not only in a well-grounded knowledge of the fundamental principles upon which depend the successful application of the various methods, but also in the devising and proving of numerous little simplifications and time-saving 'short-cuts' of procedure which will be appreciated by the advanced student and investigator as well. On the other hand, the treatment is expressly detailed enough for the piloting of the beginner safely through the various methods, and, methods for given purposes being chosen for him, he is saved from the bewildering maze of the superfluity of present-day methods. The student is told definitely what to do with his material, what method to apply for a given result, how to proceed step by step, and is given either the positive or the most probable reasons for the various steps.

Unlike other books of a similar nature, Professor Guyer's book is not confined to a single branch of the subject, such as histological or embryological methods exclusively, nor does it attempt to include material to the extent of making it bulky and unwieldy. However, it embraces the methods necessary in practically the whole field of the more usual biological courses and is thus purposely adapted for those combination courses given in high schools and colleges, which begin, preliminarily, with the simpler forms of life and pass to the consideration of the tissues and organology of the higher forms, giving some attention to embryology, and neurology as such.

The book is divided into seventeen chapters and five appendices. Beginning with a useful list of the apparatus and supplies usually required, the former well illustrated, the arrangement thence consists of a general statement concerning methods and the needs for them, followed by the procedures for 'killing' and 'fixing,' a description of the simple methods of sectioning, the methods of imbedding and sectioning in paraffin and celloidin, the processes of staining and 'mounting,' the method of frozen sections, the methods involving the precipitation of metallic substances for special differentiations, methods for the isolation of elements by 'tricks of teasing' and use of dissociating fluids, continuing with methods for the treatment of bone and other hard substances and methods for the injection of the blood- and lymph-vascular systems. Then is interpolated a chapter entitled, Objects of General Interest, in which are discussed subjects such as 'cell making' and the preparation of fluid mounts, and in which are given some ingenious devices for making 'in toto' preparations of the smaller organisms, such as water mites, transparent larvæ, small crustacea, worms, small insects and parts of insects, and for making 'opaque mounts' of such as beetles, wings of butterflies, etc. This chapter is followed by methods for the preparation and study of blood and a chapter dealing with the general procedures for the staining and mounting of bacteria. Chapter XVI. describes some of the methods necessary in the study of embryology, including technique for whole mounts and for the measuring and serial sectioning of embryos, special applications for the chick, teleosts, amphibia and mammalia, and directions for the artificial fecundation and study of the early cleavage of forms permitting it. Chapter XVII. gives the two most generally used methods for the reconstruction of specimens from sections, namely, reconstruction with wax plates and geometrical reconstruction.

Memoranda are given at the end of each chapter and these are often more interesting to one familiar with the general working of the methods than the procedure for the methods to which the chapter is devoted, for it is in these memoranda that various adaptive modifications of the methods are given, valuable suggestions as to technique in dissection, the choice of tissues for the purpose in mind, the construction and manipulation of the necessary apparatus, the selection and making up of the reagents required, and, equally important, suggestions as to the most probable causes of failure and the steps in procedure at which special care should be exercised. The substance of the memoranda might, less wisely, have been included in the body of the chapters, but, as the author states, they are, instead, appended to each chapter in order to supply additional information more or less pertinent without obscuring the main features of the methods under consideration.

Of the appendices, the first is devoted to the construction and discussion of the microscope and the optical principles involved in its use, with directions for its manipulation and an alphabetically arranged list of the more commonly used microscopical terms and appliances. In the second appendix is given a series of formulæ well chosen as representing some of the more efficient and frequently used reagents, including fixing and hardening fluids, stains, indifferent fluids, dissociating and decalcifying fluids. After each fluid is noted its peculiar advantages and some of the tissues and purposes to which it is best adapted. The third appendix is a tabulation of a large number of tissues and organs arranged alphabetically in systems with concise directions in appropriate columns for the obtaining, fixing and after-treatment of each; while the fourth appendix is especially devoted to directions for the collection and preparation of the various materials necessary for a general course in zoology. The last appendix consists merely of four conveniently constructed reference tables of equivalent weights and measures.

The book is amply illustrated as to the different apparatus required and, while one might criticize the prominence with which the names of firms making the apparatus frequently stand out in the cuts, a little advertising is allowable in exchange for the excellence of the cuts used.

On the whole, the extended scope of the book, together with its conciseness of construction and reasonable price, renders it highly commendable, and, in my opinion, it will be found useful to a larger number of people than any other book of its kind at present in existence in English. Since each experienced worker in microscopical technique has his own devices of manipulation which work best for him, there are, of course, some instances in which the author's 'steps' and 'tricks' may be disputed as being the most efficient. Professor Guyer modestly recognizes this. However, with such workers, the book will be found full of helpful suggestions, new to many. The general student will find that all the methods recommended will yield good results when the directions are intelligently followed, and the fact that the author has striven to make the book thoroughly practical: 'to omit everything that is not essential, and, above all, to give definite statements about things,' has resulted in a much-desired brevity of treatment and obviation of bulk.

Irving Hardesty
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Recent Progress in the Study of Variation, Heredity and Evolution. By Robert Heath Lock. Pp. 299. London, John Murray. 1906.

At this time when the systematic botanists and zoologists differ greatly in regard to their large number of tissues and organs arranged reader will find the book under review a most useful help in arriving at sensible conclusions. Mr. Lock is well fitted to discuss the subjects announced in the lengthy title of his book. He is intimately acquainted with the recent work in breeding, and has, himself, made some valuable contributions to knowledge. As a student at the Royal Botanical Gardens in Peradeniya, Ceylon, and later in Cambridge he has worked with Indian corn and with peas.

The book begins with an introduction in which are briefly discussed: Linnæan species, Jordan's species, variation, mutation, discontinuity of species, the work of Mendel and evolution theories. Later chapters are largely given to a fuller discussion of the topics here introduced. The first half of the book is rather elementary, intended presumably for the general reader. Natural selection, evidences of evolution and 'biometry' are treated in detail. It must not be supposed that the treatment of these topics is purely perfunctory. Even in the driest parts of the work there are sharp and valuable criticisms of the theories of the day. The author pays his respects to the theories of 'protective resemblances,' 'mimicry' and 'inheritance of acquired characters.' He shows the inadequacy of natural selection for the origin of species and prepares the reader for the subject evidently most dear to his own heart—'Mendelism.'

In describing the operation of Mendel's Law our author is at his best. He makes clear some things not generally understood in regard to the position of the 'Mendelians.' Thus (p. 180) he says: "dominance is by no means an universal phenomenon. * * * In a considerable number of instances the heterozygote is found to exhibit an appearance which is more or less intermediate between the types of character shown by the parents."

On page 205 it is shown that new forms arising in the midst of an old-established species need not be 'swamped' by intercrossing. A chapter on 'Recent Cytology' is chiefly an elementary account of the cell, but some discussion is given of the probable relation between chromosomes and Mendelian characters. Weismann's views and those of the 'Mendelians' are contrasted (pp. 261–262). The discussion of the alternating generations of plants as the 'x-generation' and '$2x$-generation' (p. 270 et seq.) will interest some readers, while his remarks on the improvement of the breed in the human race will not be taken more seriously than intended by the author.

The book has few glaring faults. There is no bibliography. This is most unfortunate, since the work is so well calculated to introduce college students to the problems of heredity and evolution. Certainly a few of the more useful works might have been named. On page 92 Davenport's 'Statistical Methods' is referred to as 'Structural Methods.' Dr. MacDougal is called Macdougal (p. 139). Perhaps 'nitch' (p. 286) is not a misprint for 'niche' but an example of reformed spelling. A lack of subheadings makes the book less easily used for reference than it should be.

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SCIENTIFIC JOURNALS AND ARTICLES

The opening (January) number of volume 8 of the *Transactions of the American Mathematical Society* contains the following papers:

G. A. MILLER: 'Generalization of the groups of genus zero.'

F. MORLEY: 'On reflexive geometry.'

G. A. MILLER: 'The groups in which every subgroup is either abelian or hamiltonian.'

H. F. BLICHFELDT: 'On modular groups isomorphic with a given linear group.'

W. E. STORY: 'Denumerants of double differentiants.'

A. RANUM: 'The groups of classes of congruent matrices, with application to the group of isomorphisms of any abelian group.'

CLARA E. SMITH: 'A theorem of Abel and its application to the development of a function in terms of Bessel's functions.'

W. B. FITE: 'Irreducible linear homogeneous groups whose orders are powers of a prime.'

L. P. EISENHART: 'Applicable surfaces with asymptotic lines of one surface corresponding to a conjugate system of another.'

THE December number (volume 13, number 3) of the *Bulletin of the American Mathematical Society* contains: Report of the September Meeting of the San Francisco Section, by W. A. Manning; 'Projective Differential

Geometry,' by E. J. Wilczynski; 'On Loci the Coordinates of whose Points are Abelian Functions of Three Parameters,' by J. I. Hutchinson; 'Associated Configurations of the Cayley-Veronese Class,' by W. B. Carver; 'Von Helmholtz,' by E. B. Wilson; 'Pierpont's Theory of Functions' (Review of Pierpont's Theory of Functions of Real Variables, Volume I.), by G. A. Bliss; 'The Mathematical Tripos for 1906,' by Virgil Snyder; Shorter Notices: Simon's Ueber die Entwicklung der Elementar-Geometrie im XIX. Jahrhundert and Simon's Methodik der Elementaren Arithmetik in Verbindung mit Algebraischer Analysis, by D. E. Smith; Randall's Elements of Descriptive Geometry and Ferris's Elements of Descriptive Geometry, by L. I. Hewes; 'Notes'; 'New Publications.'

The January number contains: Report of the October Meeting of the American Mathematical Society, by F. N. Cole; Report of the Stuttgart Meeting of the Deutsche Mathematiker-Vereinigung, by A. B. Frizell; 'A New Approximate Construction for π,' by George Peirce; 'Note on Conjugate Potentials,' by O. D. Kellogg; 'Groups of Order p^m Containing Exactly $p+1$ Abelian Subgroups of Order p^{m-1},' by G. A. Miller; 'Note on Systems of In- and Circumscribed Polygons,' by Miss S. F. Richardson; 'Hermite's Works' (Review of Picard's Oeuvres de Charles Hermite, Volume I.) by James Pierpont; 'Projective Differential Geometry' (Review of Wilczynski's Projective Differential Geometry of Curves and Ruled Surfaces), by Virgil Snyder; Shorter Notices: Nielsen's Handbuch der Theorie der Cylinderfunktionen, by F. H. Safford; Mach's Space and Geometry in the Light of Physiological, Psychological and Physical Inquiry, by C. J. Keyser; 'Notes'; 'New Publications.'

The February number contains: Report of the Preliminary Meeting of the Southwestern Section, by A. S. Chessin; 'Selected Topics in the Theory of Boundary Value Problems of Differential Equations,' by Max Mason; 'Note on Fourier's Constants,' by E. H. Moore; 'On the Minimum Number of Operators Whose Orders Exceed Two in any Finite Group,' by G. A. Miller; 'Note on the Orientation of a Secant,' by L. D. Ames; 'On Euler's ϕ-Function,' by R. D. Carmichael; Shorter Notices: Muir's Theory of Determinants, Revised Edition, by G. A. Miller; Schmall's First Course in Analytical Geometry, by Miss E. B. Cowley; Hefter and Koehler's Lehrbuch der analytischen Geometrie, by Miss E. B. Cowley; Teixeira's Tratado de las Curvas Especiales Notables, by C. H. Sisam; The Scientific Papers of J. Willard Gibbs, by E. B. Wilson; Gerland's Leibnizens Nachgelassene Schriften physikalischen, mechanischen und technischen Inhalts, by Florian Cajori; 'Notes'; 'New Publications.'

SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

The fourth regular meeting of the session of 1906–7 was held at the Chemists' Club, 108 West 55th Street, on February 8.

The following papers were presented:

The Alkylation of 4-Quinazolones: M. T. Bogert and H. A. Seil.

The Synthesis of Naphthotetrazines from p. Diamino Terephthalic Compounds: J. M. Nelson and M. T. Bogert.

Note on the Use of Ultra-violet Light in Concentrating Willemite: G. C. Stone.

Ultra-violet light is used during the concentration of Willemite to determine when the tailings are free from the ore, the degree of fluorescence giving a good indication of the amount of ore present in the sample under examination. This method of analysis was illustrated on the lecture table by subjecting samples of Willemite which had been more or less completely extracted to the action of light rich in ultra-violet rays. Of the several samples of tailings examined, some showed no signs of fluorescence, while others showed the presence of sufficient ore to make it worth while to rework them.

The Determination of Sulphurous Acid in Gelatin: A Manufacturer's Position with Regard to the Pure Food Act: Jerome Alexander.

In the determination of sulphurous acid by the official method, the sample is distilled with dilute phosphoric acid in a current of carbon dioxide, the distillate collected in standard iodine solution, the excess of which is titrated with sodium thiosulphate.

The method more generally used consists in collecting the sulphurous acid in iodine solution, boiling off the iodine and precipitating the resulting sulphuric acid as barium sulphate. This method gives lower results than the official method, indicating that substances other than sulphurous acid distil over and reduce the iodine.

Samples of gelatin were analyzed by both the above methods and by different chemists. The results obtained varied considerably between the methods and between the chemists. Furthermore, sulphurous acid was reported where no sulphur in any form had been added to the gelatin. This last is explained by a consideration of some recent work on the determination of sulphurous acid in meat where a certain amount of sulphur is found to be normally present.

This work on gelatin makes it evident that since most of the food standards are based on the presence or absence of definite percentages of certain elements or compounds, it is of vital importance that chemists compare notes and see what degree of concordance is practicable with our present tentatively official methods and agree upon some reasonable limit of tolerance to cover the differences due to personal equation and imperfect analytical methods.

The technical portion of the paper was supplemented by remarks from Dr. Leo Baekeland.

A Preliminary Communication on the Toxicity of some Aniline Dyestuffs: GUSTAVE M. MEYER.

Seven dyestuffs commonly used as food colorants were obtained from a dealer and investigated. The experiments were made on dogs and included, besides observations of the general influence of these substances, also studies of their elimination. The amount of dye used was increased daily until toxic symptoms were shown. The animals were finally chloroformed and subjected to a post-mortem examination. With one exception, the only outward symptoms induced by the feeding of these dyestuffs were such as would be brought about by the administrations of equally large amounts of any of the ordinary saline purgatives.

In a general way it may be said that, judging from the amounts given and the comparatively slight effects produced, these dyestuffs can hardly be classed among virulent poisons. What symptoms they would produce if administered daily in small doses during a very long period to unhealthy animals is still undetermined.

Studies of the influence of coal-tar colors on peptic digestion in vitro indicated that all dyestuffs almost completely inhibited peptolysis when present in a concentration of 0.62 per cent. or more. Gudeman's statement that synthetic dyes have a certain food value was investigated by this method without obtaining confirmatory results. Direct conclusions as to the effect of these substances on the human body can not, however, be drawn from experiments on digestion in vitro since certain substances have opposite effects in the two cases.

Dr. Meyer's paper was ably discussed by several experts on the subject who were present. These included Messrs. Coblentz, Lieber and Schweitzer.

C. M. JOYCE,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 627th meeting was held on January 19, 1907, President Hayford in the chair. The evening was devoted to a paper by Dr. R. S. Woodward upon 'The Theory and Application of the Double Suspension Pendulum.'

THE 628th meeting was held on February 2, 1907.

Professor Newcomb read a paper on the 'Optical and Psychological Principles Involved in the Interpretation of the Markings on the Discs of the Planets.'

Two sets of principles were discussed: one optical—including all the causes which affect the formation of an image on the retina of the eye, the other psychological—including all the causes that affect the observer's perception of the image. The first portion of the paper was concerned almost exclusively with the secondary aberration of the refracting telescope. He found that it was impossible to bring more than a small fraction, perhaps one fourth of the light emanating from a star within a radius of one tenth of a second. For this reason a black line on the planet would not be imaged as black in the telescope, but only as a gray diffused line. This effect was one that could not be cured by any arrangement or figuring of the lenses, but could be diminished by increasing the ratio of focal length to the aperture of the telescope.

The second part of the paper was devoted to what the author proposes to call 'visual inference.' This includes the process by which the eye, from the image on the retina, infers the nature of the object which produces the image. He showed at some length the extent to which this form of inference might be carried. It is based largely on experience, but in cases where this is wanting habit may take its place. One result of this process is that different people may see the same image very differently when it approaches the limit of visibility. It was shown by diagrams that broken lines under certain conditions appeared continuous, that double lines may be seen as single, or as a group of which the observer could not give the number without closer inspection. A peculiarity near the two ends of a line affected the judgment throughout the whole length of the line.

Lowell's observations of Mars were very highly spoken of as superior to all others, both in the favorable conditions under which they were made and their careful and critical character; but the general conclusion reached was that his drawings of the canals could not be accepted as certainly correct without a more complete investigation of the possible effects of visual inference in influencing the perception of the observer.

R. L. Faris,
Secretary

THE TORREY BOTANICAL CLUB

The annual meeting was called to order at the American Museum of Natural History, at 8:30 P.M., with Vice-President Burgess in the chair.

Following the presentation and acceptance of annual reports from officers and committees, the annual election resulted as follows:

President—H. H. Rusby.

Vice-Presidents—E. S. Burgess and L. M. Underwood.

Corresponding Secretary—J. K. Small.

Recording Secretary—C. Stuart Gager.

Treasurer—C. C. Curtis.

Editor—J. H. Barnhart.

Associate Editors—Philip Dowell, A. W. Evans, T. E. Hazen, M. A. Howe, W. A. Murrill, H. M. Richards, Miss A. M. Vail.

John Hendley Barnhart,
Secretary pro tem.

DISCUSSION AND CORRESPONDENCE

A SCIENCE TRUST

With the liberal appropriations by Congress for the study of the problems relating to scientific agriculture, there seems to be a growing tendency to form classes, and 'rings,' even as in the commercial and political activities of the nation. In too many cases the executive heads of the experiment stations take to themselves the credit of all that is done in their respective stations and, from the vantage ground of publicity, hamper and cripple, in many ways, the real workers in the respective fields. In many cases, of course, this injustice is unintentional; but it is none the less real. Often the true state of affairs is not realized by the offending directors; in other instances, naturally, it is not conceded.

Directors frequently assume the attitude of the political 'boss,' and attempt to 'pull the wires' in such a way that there can be no recourse for the workers except to humble themselves and 'pay court,' or to resign. They even go farther and deliberately plan to make it difficult for a worker to go from one field to a more congenial field, by throwing out insinuations as to obscure 'outs' that make a change very desirable. In other words, the 'political boss' director claims everything in sight, attempts to bully the workers into what he pleases to call 'respect for authority,' and aims to cut off any possible redress either from local sources or from equally good situations elsewhere.

Of course, the hypothetical case here given is typical only of men of small caliber who happen to occupy directors' chairs. Nevertheless, the proposed concerted action of executive officers to prevent competition in the securing of men for certain positions, by precluding the possibility of transfer, may in many cases work injustice to the men who are, in fact, responsible for the success of every station. By this combination, and the heading off of competition, salaries are held down to a disproportionately low figure, and the inspiration of possible advancement is withdrawn.

While the importance of retaining the services of valued members of a station staff is unquestioned, and while some means of ridding the service of undesirable workers is also essential, it is equally important for the success of the work that the individual have a sense of the security of his position and that he be not subject to the whims and moods of a 'boss' who does not recognize the difference between a body of educated gentlemen, who have quite as much at stake as he himself, and a force of clerks in a mercantile establishment or a factory.

On the other hand, and every well-balanced station worker recognizes the fact, the station must move forward as a unit, and there can be but one head. With a mutual understanding, and mutual confidence between the executive head and the heads of the scientific departments, the work will move forward without the necessity of 'combining' to hold down the workers, and with much saving of friction for all parties concerned. W.

THE PRIMARY SEPTA IN RUGOSE CORALS

IN SCIENCE for August 24, 1906, and in a more recent and longer paper[1] Dr. J. E. Duerden deals in a critical and analytical way with a paper read by me before the New York Academy of Sciences and published in full in the *American Journal of Science* for February, 1906. In that paper I offered another interpretation of certain figures which Duerden had drawn of sections made through *Lophophyllum proliferum*[2] and offered evidence for the support of my interpretation. Dr. Duerden's articles call for a reply. It is made in the same spirit in which the first paper was written.

It was, perhaps, to be expected that Duerden would not agree with my interpretation; but the excellent spirit of his article is commendable. I desire to discuss the matter in an equally fair manner, without any wish to belittle any work of investigation, to ignore a profound knowledge of the particular field of discussion, or to deny the possibility of other interpretations.

I still maintain the view that the resemblance to the Zoantheæ of certain rugose coral tips in dispute is not a structural resemblance, but an apparent likeness; that the important deductions about the origin of the cardinal fossula are largely based upon this supposed resemblance, and that the number of primary septa was four. If this appears to be obstinacy in the face of strong assertions to the contrary, I desire to submit the following argument.

I am not prepared to admit that the evidence afforded by the specimens of *Streptelasma profundum* examined by me is to be considered lightly. The forms examined were undoubtedly young specimens showing all the septa and having in the tip only four primary septa. In those specimens, which were much younger than the one figured by Duerden,[3] the cup was open to the bottom, thus allowing a complete view of the septa down to the tip—the septa not having reached the center in these specimens. If a section close to the tip of these specimens were made only four septa (protosepta) would be seen. Two specimens were examined by me, both of which showed unquestionably that only four protosepta were present in the youngest stage, and Professor Grabau assures me that several others of the same kind, though less perfect, are in the collection at Columbia University.

[1] *Annals and Magazine of Natural History*, Ser. 7, Vol. XVIII., September, 1906.

[2] See 'Johns Hopkins University Circular,' January, 1902.

[3] *Biol. Bull.*, June, 1905.

Unless Duerden can prove that all of these specimens are in some way defective and can show that the base of the third pair of 'so-called primary' septa (my first pair of secondary septa) were destroyed, so that in all cases they are shorter than the other (typical) primary septa (protosepta) he can not destroy the importance of this evidence. Those specimens begin as tetrameral corals, and continue so. Unless Duerden can satisfactorily show that this tetrameral character of the youngest stage is in these specimens a result of defective silicification his argument based on sections is incomplete. Duerden must show that it is possible for a hexameral coral tip to be changed by silicification, or otherwise, to a tetrameral one in more than one example.

I am not ready to concede that mere surface views are unreliable. I hold that the inside surface view of a young form is more reliable than sections made through the tips of adult individuals. In the absence of direct proof to the point and yet without intending to beg the question I would like to inquire how one can feel sure of one's section even in forms which are not accelerated? How much more uncertainty must the probability of acceleration add!

The fact that the so-called 'primary' septa (protosepta, Duerden) under dispute have positions both in locus and in sequence that coincide with those called for by Kunth's law, and that both of these facts point to their membership among the secondary septa (metasepta, Duerden), must again be urged, and the fact that the quadrant they occupy shows evidence of acceleration must be emphasized. This acceleration is shown in the case of *Streptelasma rectum* by the fact that these quadrants generally contain more secondary septa than the other two and by the further fact that tertiary septa (exosepta, Duerden) appear next to the counter septum before they appear in the other interspaces.

It must not be forgotten that *Streptelasma profundum* is an early form and devoid of specialized characters and that the forms studied were young ones; also that the forms studied by Duerden were not only specialized, but late in time (Devonic and Carbonic).

The reassertion that the order of development is that usually ascribed to zaphrentoid corals does not affect the explanation of the alar fossulæ as instances of retardation, nor does it affect the explanation of the cardinal fossula. The explanation of the cardinal fossula as caused by the siphonoglyphe (sulcar or sulcular) is certainly ingenious, but the assumption that the gonidial groove was present was made on the previous assumption that the Tetracoralla are related to the Zoantheæ, which in turn rests upon the doubtful ground of their primary hexamerism, which was assumed from a study of sections only.

What is to be offered as the explanation of the fossula of the counter septum region? And if it is to be that offered for the cardinal fossula—the presence of a gonidial groove—what is to be advanced in support of the zoanthidian relationship, since the chief structural peculiarity of the Zoantheæ is one gonidial groove?

When the correction of cardinal and counter septum is made the cardinal septum is seen to be the small one and this fact would apparently support Duerden's view that there was a gonidial groove in this region; but I have pointed out the necessity of finding another explanation for the fossula of the counter septum or else the alternative of acknowledging that both fossulæ are caused by siphonoglyphes, which would remove these forms yet further from their hypothetical zoanthidian relatives.

To my mind the inversion of the figures counts for very little. In Duerden's criticism at this point we find nothing that affects the argument—on either side, in fact. The dorso-ventral orientation is merely arbitrary.

Such unsatisfactory definitions for *ventral* are given for designating this aspect, that for determining which to call ventral and which dorsal we must decide whether, if there be two siphonoglyphes, one of these morphological structures is more pronounced than the other, or if there be only one, whether this, in colonial types, faces the proximal end of the colony.

It must be confessed that, so far as using the terms sulcus and sulculus for determining ventral or dorsal aspects is concerned the whole scheme is useless in so many cases that it fails of any importance. The terms are applicable only when these aspects, if they may be legitimately so called, are determined in some other way. Even though, as Haddon probably meant it should be used, the term *sulcus* be applied to that groove associated with the third pair of primary mesenteries, its use among fossil forms can hardly be said to be justified. Hence we may question the value of the term 'ventral stomodœal groove' in connection with the cardinal fossula. Indeed, we may go farther and question whether the Rugosa possessed gonidial grooves. If two grooves represented the primitive condition of the living Anthozoa we should find vestiges of a second in types with only one. The evidence, though negative, seems to point to a primitive Stomodæum without these grooves.

It is hard to see why the cardinal fossula necessitates the presence of a gonidial groove. It might have been due to the arrested development of the cardinal mesenteries (without that arrestation having been caused by a gonidial groove) combined with the other incompletely developed septa adjacent. Duerden admits, or rather independently asseverates, that such is the origin, but calls in the Siphonoglyphe to account for the small cardinal septum. What more likely than that the counter septum fossula is the result of arrested development of the corresponding mesenteries! On this wise all the fossulæ might be considered as old-age characters.

Before much can be asserted as to the order of development of the primary mesenteries in the Rugosa, specimens must be had which will indicate something about the sequence of the primary septa. It is not conclusive to reason from sections that do not inform us in this regard. In *Streptelasma profundum* the counter septum of the primary four seemed to reach farthest down into the calyx.

To summarize:

First, the argument from *L. proliferum* is not conclusive or final, since one can never be certain of having the lowest section. The statement can be extended to other forms studied by Duerden. Even *Streptelasma rectum* shows acceleration in the counter quadrants. In this form, a highly specialized type, the *tertiary* septa of the counter quadrants appear long before they do in the other quadrants, showing extreme acceleration in the counter quadrants. In an actual young specimen of *Streptelasma profundum*, in which the bottom of the corallum is shown, and the actual beginnings of the septa are visible, the four primary septa reach farther down than the secondary ones, and hence must be considered as having appeared before the secondary septa appeared. This shows the primary tetramerism of this type and is strong inferential evidence for all zaphrentoids.

Second, the inversion of figures counts for nothing. In referring to fossil forms of uncertain septal sequence and structural make-up the older terms are the more suitable.

Third, the hexameral arrangement of the septa in the Rugosa is not established, but rather is contradicted, by the evidence from the primitive members of the group. The primitive Rugosa appear to possess a pronounced quadripartite arrangement and a definite bilateral symmetry. Upon this symmetry and arrangement, by acceleration or otherwise, has been imposed a pseudohexameral arrangement, in instances, and a 'biradial symmetry.'

Fourth, this article really purposes to discuss the matter only and makes no pretense of ignoring other points of view, or of having settled the matter.

C. E. GORDON
MASSACHUSETTS AGRICULTURAL COLLEGE,
AMHERST, MASS.

UNIVERSITY REGISTRATION STATISTICS

TO THE EDITOR OF SCIENCE: As the figures of the University of Chicago were not received until the article on university registration statistics (SCIENCE, December 21, 1906) was in press, it was not possible to include an accompanying notice of changes in the fall registration. The facts of the case are these:

The enrollment figures of the university as of November 10 show a slight gain, from 2,130 to

2,179, in the fall courses within the quadrangles, and a considerable loss, from 562 to 247, in the courses outside the quadrangles. Owing to the increase in the summer session figures, however, there is a gain in the grand total. The change of courses given for teachers from the center of the city to the quadrangles, which took effect this fall, has lessened the number of students in such courses, but increased the efficiency of the work.

As for individual schools, there has been a gain in the academic department, especially in men, in law and in pedagogy, while there has been a slight loss in medicine, divinity and the graduate schools. As in the case of the University of Pennsylvania, a number of students enrolled in courses for teachers have been included in the Chicago figures who would be excluded in the Columbia or Harvard figures, but the time for making more definite inquiries was too short.

The following errors should also be noted: In the list of institutions mentioned on page 794, column two, line eleven, Stanford should be inserted between Kansas and Indiana; and in line fifteen Chicago should be omitted. On page 796, column one, line eighteen, Chicago should be inserted before Harvard. In the table, the number of men in the academic department of Princeton University should be 758, instead of 755. On page 794, column one, line twenty, insert, before Missouri, 'Syracuse (48.71%).' Rudolf Tombo, Jr.

ALCOHOL FROM CACTI

To the Editor of Science: In a letter entitled 'Alcohol from Cacti,' which appeared in the *Scientific American* for December 15, the author referring to the results obtained with this plant by a California chemist, states that "from five pounds of pulp he distilled, in a crude way, more than a gallon of alcohol, which was clear in color, and burned readily with a bright, warm glow."

At the time this article appeared we were hesitating about publishing the enclosed press bulletin for fear the theoretical estimates therein given would exceed the amount which it would be possible to obtain in practise.

Cactus will not average over 10 per cent. carbohydrates, and if, as is usually estimated, this yields one half its weight of 95 per cent. alcohol, it is not clear how it would be possible to obtain one gallon of alcohol from less than 140 pounds of this plant. If, however, the chemist referred to above can distil one gallon (seven pounds) from five pounds of cactus pulp, it would be interesting to know what the strength of his product is, and whether or not it was done with the assistance of a magician's wand.

R. F. Hare

Agricultural College,
New Mexico

THE PARTHENOGENESIS OF ENCYRTUS

At the time that my recent note on 'Polyembryony and Sex-determination' was written I had not seen Silvestri's latest communication. In a brief, preliminary paper[1] he presents the results of his studies on the early stages of the development of *Encyrtus* and among other details notes the fact that, as in *Litomastix,* there is parthenogenetic development, unfertilized eggs always producing males, fertilized ones only females. The maturation and early segmentation stages studied are identical in the two types.

Wm. A. Riley

SPECIAL ARTICLES

POLARIZATION AND INTERFERENCE PHENOMENA WITH WHITE LIGHT

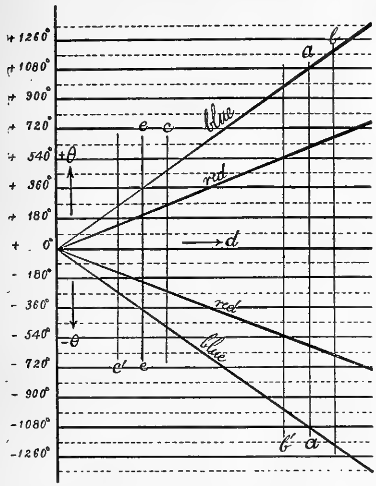
I have usually found great difficulty in endeavoring to explain the color phenomena obtained with white light in rotary polarization, in the behavior of thin plates with or without polarized light, and in interferences and diffractions generally, to an elementary class. The following diagram, therefore, which yields a large amount of information, may be of interest to the reader, although it contains nothing essentially novel. Note the occurrence of d/λ throughout.

Rotary Polarization.—If we write the rotation θ of the plane of polarization due to a thickness d of quartz cut perpendicularly to the axis,

$$\theta = \pi(1 - v'/v'') \cdot d/\lambda',$$

where v' and v'' are the velocities of right-handed and left-handed rays in the crystal

[1] Silvestri, F., 1906, 'Sviluppo dell *Ageniaspis* (*Encyrtus*) *fuscicollis* (Dalm.) Thoms.,' *Atti Acc. Lincei* (5), XV., pp. 650–658.



and λ' the wave-length or pitch of the right-handed screw, it is convenient to lay off θ in terms of d as in the chart. Two oblique lines in the positive field may then represent the amount of rotation corresponding to any two typical colors, as for instance, blue and red. The former having the smaller wave length will have the steeper slope. If the rotation is left handed, two other symmetrically oblique lines below the axis will represent this case. If the nicols are crossed the colors which fail to get through the analyzer must be rotated in multiples of π. Hence horizontal black bars intersect the whole field above and below the abscissa, to point out the colors blotted out. Colors which are rotated in odd multiples of $\pi/2$ get completely through, and these are shown by the dotted horizontal bars midway between the former. If the nicols are in parallel the black and dotted bars replace each other; or in general, if the analyzer is rotated θ to the right or to the left, the whole system of bars will move up or down, respectively, by the amount θ, as the dark band will pass through the spectrum of the right-handed plate from red to blue. The colors to be anticipated for any thickness of plate will then be given by drawing vertical lines as a, b, c, etc. Thus it is clear that only for relatively thin plates may vivid colors be expected, for here (as at c and e) there is but one or a few extinction bands in the spectrum. As the thickness increases the number of these bands increases until eventually the colors removed are practically the same as those retained and white light appears in all positions of the nicols. The spectra are channeled in the beautiful way seen with a column of quartz an inch or more long.

Furthermore, if a right and left bi-quartz be taken as in the saccharimeter the colors for the same d will be identical both for crossed and parallel nicols, but not otherwise. For the dark bands move from red to blue and from blue to red, respectively, in the two halves, on like rotation of the nicols. Moreover, if for a thickness d an additional rotation is imparted as by the sugar tube, d will pass to c in the right-handed plate but to c' in the left-handed plate. In relatively thin plates, however, the identity of color may be restored on rotating the analyzer to the right, for the dark band at c' below the abscissa moves in wave-lengths much more rapidly through the spectrum than at c above the abscissa; but the identical colors will even here not be quite the original color. For a relatively great thickness the identity can not be restored since b contains three and b' usually two extinction bands.

Finally the occurrence of successive orders of colors is suggested by the chart.

Thin Plates in Polarized Light.—The plates of thickness d are cut parallel to the axis, or the edge of the thin wedge is so ground and placed at 45 degrees to the plane of the polarizer. If we write

$$\phi = 2\pi(\mu_e - \mu_o) \cdot d/\lambda,$$

where ϕ is the phase difference of ordinary and extraordinary rays, λ the wave-length in air, μ_o and μ_e the respective indices of refraction, and if the nicols are crossed, the diagram as drawn will apply at once. Rotation of the analyzer over 90 degrees exchanges the black and dotted horizontal bars as before; but there is this essential difference, that the bars can not be moved continuously on rotating the nicols, since the intermediate colors are elliptically polarized. Colors are white for thick plates and one may note in addition that $\mu_e - \mu_o$ and d are reciprocally related.

Interference. Diffraction.—For the case of two slits, real or virtual, at a distance c apart and r from the screen, if d is the distance of a lateral dark band from the central fringe for light of wave length λ, we may write

$$n = (2c/r) \cdot d/\lambda,$$

where odd numbers for n determine the position of the successive minima. Hence if the phase difference in which the rays meet $\phi = \pi n$ be introduced,

$$\phi = (2\pi c/r) \cdot d/\lambda$$

and the chart is applicable at once, with the understanding, however, that the dark horizontal bars now denote maxima, the dotted bars minima. This, however, is not necessary, for the bars may be moved up by inserting in one of the rays a thickness of lamina t of the refracted index μ (compensator) by the amount $2\pi(\mu - 1)t/\lambda$, until a minimum replaces the central maximum. The compensator thus has the same effect as the rotation of the nicols in the first paragraph. The chart shows the lateral extent of spectra of different orders very clearly, the first having a breadth of $d = 0$ the other breadths being proportional to the intercepts of the successive bars between the oblique lines blue and red. The overlapping of these spectra is also well shown. The spectra must be less spread out as the slopes $2\pi c/r\lambda$ are steeper. If the slit of the spectroscope be placed at a given distance, as at $d = a$, for instance, the vertical section indicates the channeled spectra which will be observed and the dark lines may be sharp enough to suffice the standardizing the spectrum.

With certain well-known changes the same remarks apply for most cases of the diffraction of white light.

Colors of Thin Films. Ordinary Light.—If d denote the thickness of the film or a given section of the wedge of refractive index μ, λ the wave length of light in air, r the angle of refraction corresponding to the angle of incidence i we may write

$$\phi = 2\pi\mu \cos r \cdot d/\lambda$$

where minima correspond to even numbers of π. Hence the positive field of the chart applies at once. The equation as written is primarily useful for the wedge. What the diagram points out very well is the resolution of doublets in the spectra of very high order. If the incidence is normal and the wedge be of air as in the interferometer $\phi = 2\pi d/\lambda$. If the thickness is constant and the angle of incidence varies the oblique lines are still applicable if instead of d the quantity $\cos r$ be laid off as the abscissa. They are real between $r = 0$ and the angle of total reflection.

In practice the oblique lines may be drawn to the scale on a sheet of paper, the black bars on a sheet of celluloid capable of sliding up and down over the former and the vertical lines may be represented by threads movable to right and left over the celluloid. The whole is to be serviceably framed on a sheet of tin plate.

C. Barus

Brown University,
Providence, R. I.

THE CAUSES OF THE GLACIAL EPOCH[1]

Of the various hypotheses advanced to account for the occurrence of a protracted glaciation of a large portion of the earth's surface in post-tertiary times, none has thus far met with universal acceptance; partly because of the insufficiency of data regarding critical geographical areas, but mainly because of the feeling that although the causes discussed by Croll, Chamberlin and others are or may be true ones, they are inadequate to account, quantitatively, for all the facts observed. The paleontological evidence of the prevalence of temperate and even semi-tropic floras and faunas in the late Tertiaries within what are now arctic regions, gives such forcible evidence of the comparative uniformity of tem-

[1] Read at tenth session of the International Geological Congress, Mexico, September 6–14, 1906.

perature prevailing even in late preglacial times all over the globe, that we instinctively seek for some more cogent cause of the widespread glacial phenomena that followed so closely upon the periods of greatest mountain-making recorded in geological history.

It is not the purpose of this paper to discuss the glacial theories alluded to above in detail, that having been done by many able writers; but rather to call pointed attention to another hypothesis now before the scientific world for a number of years, and which seems to me to offer a complete solution of the problem, provided it can pass the criticism of physicists; for from the geological point of view it seems to suffer no valid[2] objections. This hypothesis or theory (for it seems rather to deserve the latter name from its comprehensiveness) first brought forward by Dr. Marsden Manson in 1891, has until lately suffered the initial fate of many others now generally accepted, viz., that of being 'todtgeschwiegen' at first, as being too much opposed to some generally accepted, but by no means proved dicta regarding earth-heat, and especially the time-limit of its influence upon terrestrial surface-temperatures. Physicists at one time claimed that the globe as a whole is more rigid than steel, since it must otherwise suffer sensible tidal deformation; but were met by the ocular proof, known to every geologist, that so far from being even moderately rigid, the crust as known to us is a mere congeries of fragments in unstable equilibrium and in more or less constant movement for readjustment.[3] We are now told that the transmission of earth-heat to the surface must have ceased in early geological time, because of the low conductivity of the rocks known to us. But the enormous effusions of molten rock even in late Tertiary times, followed by manifestations of vulcanism which, though now apparently in course of extinction, proves the continued existence of high temperatures not far below the surface crust, again invalidates the physicists' objection, because based on the arbitrary assumption that conduction was the main or only manner in which the interior heat could reach the surface.

It is evident that whatever may have been the original source of that heat, whether from cosmic contraction or planetesimal collision, it has existed and exists now, even though practically insensible at the earth's surface. Even if, as some believe, to-day's vulcanism were only 'skin-deep,' the evidence of former much greater heat is too strong to be set aside. That this heat must have been brought up from the depths of the earth-mass by water, aqueous vapor and other gases, as is now the case in volcanic eruptions, and is also being done by

[2] While it is evident that extended glaciation existed in the Permian era, the claim that it was as extensive and continuous as the Pleistocene glaciation does not seem to me to be well established. The Permian having been, as is strongly emphasized in Chamberlin and Salisbury's 'Handbook of Geology,' a time of extended deformation of the earth's crust, to which the deflection of ocean currents, causing glacial conditions, is ascribed by them, it seems quite reasonable to suppose that such deformations extended to the upraising of large areas, together with orogenic uplifts, of which no evidence now remains except the seaward borders of the glaciated areas, with their till and roches moutonnèes, such as we now see at the foot of glaciers. It is pertinent to inquire, in this connection, by what imaginable changes in land areas and ocean currents the north and south polar regions could *at this time* remain deglaciated and made to grow magnolias, figs and the like. The Arctic Ocean is now substantially closed to equatorial currents; the Antarctic is wide open; yet both are glaciated. What intermediate arrangement could give either or both a temperate climate, with or without more, or less, carbon dioxid?

The claim that aridity is shown by the prevalence of evaporation-deposits of rock-salt and gypsum, is hardly tenable alongside of that made for glaciation. The low temperature and abundant moisture required for glaciation do not seem compatible with arid heat. Evaporation due to currents of undersaturated air, such as always characterizes descending air currents, is very effective; and the arid conditions would hardly be expected to extend as far north as Stassfurt.

[3] See, for the latest discussion of this subject, the paper on 'The Geodetic Evidence of Isostasy,' etc., read before the Washington Academy of Sciences by John F. Hayford, May 18, 1906.

geysers, can not be questioned. Whatever part chemical action, the friction or collisions connected with faults, or that accompanying flexures of strata may have in generating heat, that heat-generation must have been much more active in times of higher original temperatures and active mountain-making. It seems as though, despite all contrary suggestions thus far made, a higher temperature at the earth's surface than exists at present, within the geological ages covered by the warm-temperate flora and fauna found also in the arctic regions, may fairly be presumed on physico-geological grounds alone. This reasonable assumption forms the basis of Manson's theory of the ice age.

It can not be doubtful that during any highly heated condition of the globe, of whatever origin, the bulk of the water now gathered in seas, lakes and rivers existed in the form of vapor, which as it ascended was condensed into a mass of clouds forming a thick spheroidal envelope all around. On the outside, upper surface of this cloud-sphere the sun exerted substantially the same zonal effects as it now does upon the earth's surface, modified mainly by the uniformity of the physical nature of the cloud-surface, as against the alternation of sea and land as they now exist, and which by their differences in the absorption and radiation of heat, in heat capacity, and in topographic features, modify profoundly the typical, regular zonal order of climates. The tropical belt with its strong ascending currents, low barometer, and high temperatures; the two adjoining arid belts with descending currents and high barometer, and the temperate zones to poleward of the same, with variable but generally low barometer, would be defined on the cloud-spheroid as they are now on the earth's surface, but with greater regularity, though perhaps less sharply. It is also clear that, though not directly influencing the temperature of the earth's surface, the solar radiation would act powerfully as a conservator of earth-heat, compensating to some extent the radiation into space from the cloud-surface, of the heat carried up by convection currents.

The general disposition of the rain-belts would also be substantially as it is now, but the amounts of rainfall would, in so thick a cloud-cover, undoubtedly be greater than at present. The isothermal spheroids or shells corresponding to our present temperatures would at first be at heights more considerable than at present; but as the heat carried up from the earth's surface was more and more lost by radiation into space from the exterior cloud-surface, the isothermal shells would gradually descend, and the temperature of the falling rains would become lower, so as under favorable conditions to fall as snow. It is clear that snowfall might occur at any period of the earth's evolution on high mountain ranges or plateaus, and there the accumulation of snow might at any period have formed nevees and glaciers with their well-known effects. The earlier glaciations observed, especially in the Permian, are, therefore, quite compatible with Manson's theory. Elevation as a cause of glaciation must, however, be accompanied by its necessary correlative factor, an abundant rainfall; a point frequently left out of consideration in this connection. Labrador is a conspicuous example of non-glaciation from low precipitation.

Owing to the higher radiating power of the earth-surface as compared with the ocean, as well as to its much lower specific heat, the earth must have cooled more rapidly than the oceans by radiation alone. In addition to this, the water flowing from it into the seas would carry off a large amount of heat. Even while the ocean still received heat from its bed, the land areas would be a cooling agency especially for the ocean depths, while the warm oceanic surface waters would be supplying abundant vapor for precipitation on the relatively colder land areas. The latter would finally fall to so low a temperature as to receive their precipitation in the form of snow, thus inaugurating the glacial period, during which the isothermal shell of say the freezing-point of water, and below, descended near to the earth's surface. As the ocean gradually also cooled and evaporation diminished, the protecting cloud-envelope became thinner, first in the tropics and the flanking belts of lesser rainfall (which later became the arid belts); and thus gradually the zonal solar régime was established.

Such are main features of Manson's theory, the details of which have been elaborated in his published treatise on the evolution of climates and other papers, and the substance of which will be presented to this body by the author himself. In my view it is not a conception to be lightly set aside, for whatever evidences of former glaciations may have been observed, there has not appeared in former geologic history anything resembling in magnitude the pleistocene glaciation, the scattered remnants of which are even now in gradual retreat under our eyes. The observed evidences of glaciation in former geologic ages do not appear to be of such extent, or to be accompanied by detrital deposits indicating a continental extent of glaciation; they are apparently such as might be produced at any time by either the upthrusting of mountain chains, or by wider, epeirogenic elevations of the surface. Thus far, it seems as though there had been but one distinctively glacial epoch of world-wide importance and extent; and that nearly contemporaneous with the appearance of man upon earth.

It has been asked how the early floras and faunas could have existed and developed under the perpetual cloud assumed by Manson's theory to have covered the earth prior to the establishment of the solar climate, toward the end of the glacial period. In answer to this it may be suggested, apart from the fact that even at the present time the average cloudiness of the sky is estimated at 60 per cent., that the earlier floras consisted almost exclusively of plants whose analogues or evolutional successors, such as ferns and horse-tail rushes, vegetate preferably in dense shade, even in cloudy climates; and the extreme succulence of the carboniferous flora is sometimes approached when in certain climates, under unusally rainy seasons, such plants grow to maturity almost without a ray of sunshine. The plants growing under the canopy of primæval forests, in perpetual twilight, show how easily vegetation adapts itself to such conditions. In later periods, as the cloud-envelope brightened, the higher orders of plants, now preferably basking in sunshine, had opportunity to develop to their present prominence. But it is notable that the present forms of peculiarly arid-region plants, which are specially adapted to hot sunshine and dry air, are absent from any of the fossil forms thus far reported. They clearly had no *raison d'être* until the cloud-veil was dissipated by the sun.[4]

There seems to be as little difficulty in assuming the animal creation to have been tolerant of, or adapted to, a sunless existence. Not to speak of our present nocturnal and deep-sea faunas, the adaptability of the pupil of the eye now existing provides all needful conditions so far as vision is concerned; and the great wide-open orbs of the ichthyosaurs suggest ready adaptation to dark days. Here

[4] Chamberlin (Science, October 26, 1906) claims that the existence of palisade cells in plants of paleozoic age proves the existence of arid-region plants at that time. But palisade cells as such depend much less upon climatic factors than upon leaf-texture and botanical relationship. It is only the presence of *several* tiers of such cells beneath the epidermis of the upper leaf-surface that constitutes such presumptive proof; witness the existence of abundant palisade cells in firm-leaved ferns that are at home in the deepest shade, right alongside of others which show no such tissue; as well as the abundant palisade tissue in the leaves of the shade-loving Pyrolaceæ and other ericaceous plants, of *Vinca minor, Myosotis palustris* and thousands of other shade plants. Moreover, *saline* soils cause xerophytic structure and growth; which, therefore, should not surprise us if found in coal plants. The very generally clayey (fire-clay) nature of the substrata of coal beds plainly suggest that the coal-forming flora was one of *swamp* plants, and not xerophytic or even upland, as suggested by Chamberlin. So far as I am aware, no plants showing the well-known extreme provisions against drying-out, such as we find in the cactus and others, have been found among the fossils of even the late Tertiary. On the other hand, the fauna of the Permian, belonging chiefly to the Brachiopod and Cephalopod orders, indicates a warm temperate or tropical, not a frigid temperature of the seas, such as is shown by the marine fauna of the Pleistocene glacial epoch.

as in the case of plants, the organisms specially adapted to continual sunshine—the desert fauna—seem to be absent from prepleistocene deposits. Horned toads, Gila monsters and animals of similar habits were not then in evidence, so far as the writer is aware.

In any case, the postulates for a sunless existence of prepleistocene beings are not greater, if as great, as those involved in Chamberlin's hypothesis of a materially greater, or less, content of carbonic dioxid in the atmosphere.

It does seem to the writer that unless it can be shown that the temperature prevailing at the beginning of the glacial epoch could not have been high enough to maintain a cloud envelope, Manson's theory as outlined above must be considered the most probable among those that have heretofore been suggested, as fulfilling both qualitatively and quantitatively the postulates of the great ice age; not excluding, of course, the probable influence of the agencies claimed by Arrhenius and Chamberlin as the chief ones, but which appear to the writer to be inadequate to account for the phenomena in actual evidence.

E. W. Hilgard
University of California,
January, 1907

CURRENT NOTES ON METEOROLOGY

WINDS ON THE PEAK OF TENERIFFE

The controversy regarding the direction of the upper winds in the vicinity of the Cape Verde and Canary Islands has prompted Hann to bring together (*Met. Zeitschr.*, Dec., 1906) the published observations of wind direction on the summit of the Peak of Teneriffe (Lat. 28° 49′ N.; Long. 16° W., altitude 12,172 feet). The conclusion is as follows: the S. W. and W. wind which is very often observed above 3,000 meters, even in summer, is certainly not a local wind, but belongs to the upper members of the general atmospheric circulation. The N. E. trade occasionally blows on the top of the peak, at least in summer. The mean direction of cirrus clouds in winter is W. by S. The N. W. winds observed by Hergesell in summer in the vicinity of the Canary Islands were probably connected with the then location of the subtropical high-pressure area of the North Atlantic Ocean. The map of isobars at 4,000 meters (Teisserenc de Bort) shows, in July, the center of the maximum somewhat N. W. of the Canaries, so that northerly winds at 4 km. above sea-level would not be contradictory to the pressure distribution. According to the interesting observations of temperature and humidity made by Hergesell in the free air above the anticyclone, the latter may extend to greater altitudes than has thus far been assumed. More constant equatorial currents are to be expected over the West Indies and Central America in the same latitudes.

CLIMATOLOGY OF THE UNITED STATES

'The Climatology of the United States,' by Professor A. J. Henry (Bull. Q, U. S. Weather Bureau, 4to, 1906, pp. 1012, Pls. 34, Figs. 7), is one of the most important publications of our Weather Bureau. The need of a compact summary of the essential climatological data for the United States has long been felt. Hitherto these tabulations have been scattered through various annual reports of the chief of the Weather Bureau. Since Loren Blodget's famous classic, 'The Climatology of the United States' (1857) there has been no attempt to collect into one volume, and to discuss, the mass of climatological material collected by our official and voluntary observers. The data in this volume cover, generally speaking, the period 1870–1893. There is a discussion, satisfactory on the whole, of the climates of the United States in general (84 pages), illustrated by a considerable number of maps. This is the portion of the book which will be most generally used, and it will serve its purpose well. A long series of tables follows, in such form that they can easily be referred to by those who wish detailed information. At the end, occupying the larger part of the volume, come condensed summaries for the different states. The advertised price of the book is $10, which is much too high if the volume is to find its way generally into our libraries, but it is to be hoped that all educational institutions will secure free copies.

THE ANTI-TRADE OVER THE ATLANTIC OCEAN

The 'Results of the Franco-American Expedition to explore the Atmosphere in the Tropics' are discussed by Professor A. L. Rotch in the *Proc. Amer. Acad. Arts and Sci.*, Vol. 42, No. 14, Dec., 1906. A summary of these results has already appeared in Science. This expedition, it will be remembered, was sent out in the summer of 1905, at the joint expense of Messrs. Teisserenc de Bort and Rotch, and made studies of the atmospheric conditions in and above the N. E. trade belt of the eastern North Atlantic, by means of small balloons and kites. The most important result of the summer's work was the establishment of the fact that 'the classic observations of the return trade, which were long ago made on the Peak of Teneriffe, indicate a general phenomenon, and agree with those obtained over the open ocean by the present expedition.' We note also a confirmation (p. 268) of the view that at sea cumulus clouds (noted at the edge of the N. E. trade in this case) are probably formed by the condensation of water vapor which is diffused upward from the ocean surface.

THE TSUKUBA OBSERVATORY

We have received the results of meteorological observations made on Mt. Tsukuba (Japan) during the year 1902. This mountain is about forty miles northeast of Tokio, and rises to 2,854 feet at its highest summit. Although the altitude is not great, the exposure is excellent. In addition to the summit station, there are also a base and an intermediate station. The observatory is well equipped with standard instruments. Observations are taken at 2, 6 and 10 A.M. and P.M. on the summit; at 0, 2, 4, 6, 8, 10 A.M. and P.M. at the intermediate station. At the base, observations are made weekly, when the thermograph and barograph sheets are changed. On the summit, hourly records can be obtained from the self-recording instruments. An annual publication is issued. On the title-page appears this statement: 'Herausgegeben von Hofmarschall-Amt. S. K. H. des Prinzen Yamashina.'

TUBERCULOSIS AMONG THE INDIANS OF ARIZONA AND NEW MEXICO

Under the above title Dr. I. W. Brewer, of Fort Huachuca, Ariz., has given the results of a study recently made by him, with the assistance of the medical officers at the Indian agencies and schools (*N. Y. Med. Journ.*, Nov. 17, 1906). The wide-spread prevalence of tuberculosis among these Indians emphasizes very forcibly the fact that the climate of Arizona and New Mexico, with all its sunshine and dryness, is not a specific. No climate *is* a specific. It is certainly of great benefit to those in the early stages of tuberculosis, but is of little value when a patient is improperly nourished and is surrounded by filth, or lives in poorly ventilated houses.

R. DeC. Ward

Harvard University

THE AMERICAN WOMEN'S TABLE AT NAPLES

The Naples Table Association for promoting Laboratory Research by Women wishes to call attention to the opportunities for research in zoology, botany and physiology provided by the foundation of this table.

The Zoological Station at Naples was opened by Professor Anton Dohrn in 1872 for the collection of biological material and for the study of all forms of plant and animal life. Under the personal direction of Professor Dohrn and his assistants the station has developed into an international institution for scientific research. Any government or association which pays five hundred dollars annually is assigned a table for research and is entitled to appoint to it qualified students, who are provided by the station with all materials, apparatus and assistance, free of cost. One table is sometimes used by four or five research students in the course of a year.

This association, which was formed in 1898 to promote scientific research among women, is maintained by annual subscriptions of fifty dollars each. For the year 1906–7 the following colleges, associations and individuals are contributors: Association of Collegiate Alumnæ, Barnard College, Bryn Mawr College, University of Chicago, Mass. Institute of Technology, Mount Holyoke College, Radcliffe College, Smith College, University of Pennsylvania, Vassar College, Wellesley College, Western Reserve University, Women's College in Brown University, Women's Advisory Committee of the Johns Hopkins Medical School, Woman's College of Baltimore, Miss Helen Collamore, Mrs. Alice Upton Pearmain, Mrs. J. M. Arms Sheldon, Mrs. Elizabeth A. Shepard, Mrs. Mary Thaw Thompson.

The year of the association begins in April, and all applications for the year 1907–8 should be sent to the secretary on or before March 1st, 1907. The appointments are made by the executive committee.

A prize of $1,000 has been offered periodically by the association for the best thesis written by a woman, on a scientific subject, embodying new observations and new conclusions based on an independent laboratory research in biological, chemical or physical science. The fourth prize will be awarded in April, 1909.

Application blanks, information in regard to the advantages at Naples for research and collection of material and circulars giving the conditions of the award of the prize will be furnished by the secretary.

Executive Committee: Florence M. Cushing, 8 Walnut Street, Boston, Mass., chairman; Mary E. Woolley, President of Mount Holyoke College; Ellen H. Richards, Massachusetts Institute of Technology; Alice Upton Pearmain, 388 Beacon Street, Boston, Mass.; Marion Talbot, Dean of Women, Chicago University; Elizabeth L. Clarke (Mrs. S. F.), Williamstown, Mass., treasurer; Ada Wing Mead (Mrs. A. D.), 283 Wayland Ave., Providence, R. I., secretary.

THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH

The Rockefeller Institute for Medical Research purposes to award for the year 1907–8 a limited number of scholarships and fellowships for work to be carried on in the laboratories of the institute in New York City, under the following conditions:

The scholarships and fellowships will be granted to assist investigations in experimental pathology, bacteriology, medical zoology, physiology and pharmacology and physiological and pathological chemistry.

They are open to men and women who are properly qualified to undertake research work in any of the above-mentioned subjects and are granted for one year.

The value of these scholarships and fellowships ranges from eight hundred to twelve hundred dollars each.

It is expected that holders of the scholarships and fellowships will devote their entire time to research.

Applications accompanied by proper credentials should be in the hands of the secretary of the Rockefeller Institute not later than April 1, 1907. The announcement of the appointments is made about May 15. The term of service begins preferably on October 1, but, by special arrangement, may be begun at another time.

L. Emmett Holt,
Secretary

14 West 55th Street,
New York City

SCIENTIFIC NOTES AND NEWS

The Rumford medal of the American Academy of Arts and Sciences 'for discoveries in light and heat,' has been awarded to Professor E. F. Nichols, of Columbia University.

The Berlin Academy of Sciences has conferred its Helmholtz medal on M. Henri Becquerel, Paris.

Dr. Eduard Zeller, the eminent student of the history of philosophy, has celebrated his ninety-third birthday.

Dr. H. H. Hildebrandsson, professor of meteorology and director of the Meteorological Institute of the University of Upsala, and Professor Knut Joh. Ångström, professor of physics, have been elected honorary members of the Royal Institution, London.

DR. T. W. RICHARDS, professor of chemistry at Harvard University, has received a fifth grant of $2,500 from the Carnegie Institution of Washington.

MR. A. F. BURGESS, secretary of the Association of Economic Entomologists, has tendered his resignation as state inspector of orchards and nurseries for Ohio, to go to Massachusetts to experiment with insecticides for the destruction of gypsy and brown-tail moths.

PROFESSOR HENRY F. OSBORN is expected to return about April 1 from Egypt, where, with the assistance of Messrs. Walter Granger and George Olsen, he has been making paleontological explorations in the Fayoum desert on behalf of the American Museum of Natural History.

PROFESSOR HAROLD HEATH, of Stanford University, has been given leave of absence for the second half year, and will go first to Naples. He will return by way of Japan and the Malay Archipelago.

WE learn from the New York *Evening Post* that an archeological expedition, under the direction of Professor John R. S. Sterrett, will start on March 9 for Asia Minor. Professor Sterrett has previously spent several years in exploration and excavation in the east. He will be accompanied by B. B. Charles, instructor in Semitics, A. T. Olmstead, formerly fellow in the American School at Jerusalem and now at Athens, C. O. Harris, late instructor in Latin, now in the American School at Athens, and J. E. Wrench, late fellow at the University of Wisconsin.

THE Society for Horticultural Science, of which Professor L. H. Bailey, of Cornell University, is president, and the American Pomological Society, of which L. A. Goodman, Kansas City, is president, will meet on the grounds of the Jamestown Exposition on September 25 and 26.

PROFESSOR ERNEST RUTHERFORD gave a lecture at Clark University on February 15 on 'Radium and Radioactive Substances.'

PROFESSOR W. T. COUNCILMAN, of the Harvard Medical School, delivered an address on January 23, before the Harvard Club of Worcester, on 'The New Medical School in its Relation to the University.' He also gave the opening address at the Tuberculosis Exhibit at Taunton on January 31. His subject was 'Tuberculosis: the nature of the disease and the modes of relief.'

THE annual address before the Society of the Sigma Xi was given at the University of Nebraska on February 15 by Dr. Charles Sedgwick Minot, of Harvard University, on 'The Biological Interpretation of Life.'

MR. W. BATESON gave, on February 11 and 15, two lectures on 'Mendelian Heredity and its Application to Man,' in the medical schools of Cambridge University.

THE Boston *Transcript* states that the fund which a special committee has been instrumental in assembling to be used for a memorial to the late Professor Shaler, of Harvard University, has been completed, and amounts to $30,000. The committee, of which Edward W. Atkinson is chairman, will meet to decide upon the exact form the memorial will take.

THE Paris Municipal Council has appropriated the sum of about $800 for a monument in honor of Pierre Curie, to be erected in the School of Physics and Industrial Chemistry.

THE Botanical Seminar of the University of Nebraska is to celebrate the two hundredth anniversary of the birth of Carl von Linne on the eleventh of May next, the day before the exact anniversary. Addresses are to be made by Dr. Clements, Dr. Pound and Dr. Bessey, in connection with the commemorative exercises.

DR. J. PÖSCHL, professor of physics at the Technological Institute at Graz, has died at the age of seventy-nine years.

THE deaths are also announced of Professor le Roux, formerly professor of physics at the Paris School of Pharmacy, and of Dr. Lyon, docent for analytical chemistry at Geneva.

THERE will be a civil service examination on March 20 and 21 for the position of scientific assistant in veterinary zoology in the Bureau of Animal Industry at a salary of $840.

The zoological and ethnical collections made recently in East Africa by Mr. Richard Tjäder have been acquired by the American Museum of Natural History.

We learn from English journals that the pearl oyster fisheries of the Mergui Archipelago, lying off the province of Tenasserim, Lower Burmah, are to be the object of an investigation on behalf of the Indian government, and for this purpose Mr. R. N. Rudmose Brown and Mr. J. J. Simpson left early last month for Rangoon. It is extremely probable that an examination of the ground may result in the discovery of new pearl banks, or at least the possibility of such banks being started. It is expected that the investigation, at least on its economic side, will be completed before the commencement of the southwest monsoon season in May.

The lichen collection of Dr. H. E. Hasse, of California, consisting of about 3,000 species and many duplicates, has been recently presented to the New York Botanical Garden by Mr. John I. Kane.

The New York Aquarium, situated in Battery Park, and conducted by the New York Zoological Society, with Dr. C. H. Townsend as director, is open free, every day in the year. It is closed on Monday forenoons except to school teachers with their classes, and to members of the New York Zoological Society. When a holiday occurs on Monday the public is admitted as on other days. The attendance for the ten years ending December 31, 1906, amounted to 17,103,328—an average of 4,685 visitors a day. The attendance for the year 1906 was 2,106,569—an average of 5,771 a day.

Five free lectures on popular scientific subjects, illustrated with lantern views, and open to the public, are being given in the Geological Lecture Room, University Museum, Harvard University, at 3:30 o'clock on Sunday afternoons, February 3 and 17, March 3, 17 and 31, 1907. The subjects treated in the lectures are related to exhibits in different parts of the Museum.

February 3—'Meteorites, their Fall from the Sky, their Composition and their Relation to the Rocks of the Earth's Interior,' Professor J. E. Wolff.

February 17—'The Ruins and the Ancient People of Yucatan, Mexico,' Dr. A. M. Tozzer.

March 3—'Why the Earth is believed to be Millions of Years Old,' Professor W. M. Davis.

March 17—'Tropical Plants,' illustrated by Museum Specimens and Pictures, Professor G. L. Goodale.

March 31—'Quartz, its Varieties, Origin, Characteristics and Uses,' Professor Charles Palache.

We learn from the *American Museum Journal* that the American Bison Society held its annual meeting at the Museum on Thursday, January 10. This society has for its object not only the prevention of the extermination of the bison, but also the encouragement of the raising of the animal as a commercial proposition. A generation ago the bison, or American buffalo, roamed over the western plains in vast herds, estimated to contain more than ten million individuals, while to-day, on account of the merciless and wanton slaughter practised in the early eighties, scarcely two thousand are known to be in existence. The society proposes to encourage the establishment of bison reservations in each state where climate and other conditions are favorable for the maintenance and increase of herds. For New York the proposition is that, as a beginning, the state set aside nine square miles in one of the reserved areas of the Adirondack region and appropriate $15,000 for the purchase and maintenance of a herd of fifteen bison. Dr. William T. Hornaday, director of the New York Zoological Park, is the president of the society.

We learn from the London *Times* that Mr. Francis Galton, F. R. S., has given a further sum of £1,000, which has enabled London University to revise and extend the scheme for the study of national eugenics founded under his previous benefaction, and will provide for the carrying on of the work of the eugenics laboratory for the next three years. Mr.

David Heron, M.A., has been appointed Galton research fellow in national eugenics, in succession to Mr. Edgar Schuster, M.A., resigned; Miss E. M. Elderton has been appointed Galton research scholar, and Miss Amy Barrington (mathematical tripos, Cambridge) computer. The work in this subject will be carried on under the supervision of Professor Karl Pearson, F.R.S., in consultation with Mr. Francis Galton. It is the intention of the founder that the laboratory shall act (1) as a storehouse for statistical material bearing on the mental and physical conditions in man and the relation of these conditions to inheritance and environment, (2) as a center for the publication or other form of distribution of information concerning national eugenics. Provision is made in association with the biometric laboratory at University College for training in statistical method and for assisting research workers in special eugenic problems. Short courses of instruction will be provided for those engaged in social, anthropometric or medical work and desirous of applying modern methods of analysis to the reduction of their observations. The laboratory, which is in connection with University College, is temporarily established at 88, Gower-street, W.C.

WE learn from the London *Times* that the highways committee of the London County Council has presented a report to the council with reference to the Greenwich Electricity Generating Station and the Royal Observatory. Last year the admiralty appointed a special committee to inquire into the working of the station. The special committee, which consisted of Lord Rosse, representing the Royal Observatory, Professor J. A. Ewing, representing the admiralty, and Sir Benjamin Baker, representing the council, have now issued their report, and the conclusions arrived at are contained in the following recommendations, in which are suggested certain modifications in the arrangements at the generating station, and as to the manner and times at which the first portion of this station shall be worked, so as to avoid any possible interference with the work of the observatory: (*a*) The question, both as regards effects of vibration and obstruction through chimneys or discharge from chimneys, to be further reviewed after, say, two years, by which time experience should be obtained with the second portion of the station at work. (*b*) The generating plant for the second portion to be turbines, which, as well as the dynamos, must be of a perfectly balanced type, such as has been proved by trial not to cause vibration. (*c*) An undertaking to be obtained that when the plant in the second portion is available for use, the reciprocating engines of the first portion shall not in ordinary circumstances be used after 10 P.M., and their use shall be restricted as far as possible after 8:30 P.M. (*d*) The two chimneys of the second portion, at present incomplete, to be not higher than 204 feet above Ordnance datum. (*e*) The discharge of gases both from these and from the existing chimneys not to be materially hotter than the discharge is now from the existing chimneys—namely, about 250 degrees F. (*f*) No further extension of the station to be made beyond the 20,000 kilowatts now contemplated in the equipment of the second portion.

UNIVERSITY AND EDUCATIONAL NEWS

BY the will of the late John A. Creighton, the sum of $900,000 is bequeathed to educational and charitable institutions, including $500,000 to Creighton University. The residue of the estate which is said to amount to more than $5,000,000 is to be distributed *pro rata* to the same institutions, whence it appears that Creighton University will receive in all the sum of about $2,500,000.

TEACHERS COLLEGE, Columbia University, has received from a source not stated, a gift of $400,000 for a building for its school of domestic economy.

THE Johns Hopkins University has received $150,000 from the estate of the late Charles L. Marburg, $100,000, of which goes to the hospital and $50,000 to the university.

AN unnamed donor has presented a new gymnasium to Syracuse University.

TEN research fellowships of the annual value of $500 each have been established in the engineering experiment station connected with the College of Engineering of the University of Illinois.

The University of Washington, Seattle, announces the establishment of five teaching fellowships in mathematics, each yielding annual stipends from $400 to $500. Fellowships are open to graduate students only.

The Hon. A. McRobert, Cawnpore, India, has founded a fellowship at Aberdeen University for cancer research. At Mr. McRobert's death the sum of $50,000 is to be available for this work, but that it may begin at once he has undertaken to provide $2,000 a year.

The will of the late Charles James Oldham, of Brighton, leaves to the University of Oxford and to the University of Cambridge the sum of £5,000 each, such sums to be invested. The incomes arising from such investments are to be applied to the founding of one annual prize or scholarship in the ancient classics, Greek and Latin, and one annual prize or scholarship in the knowledge of William Shakespeare's works, such prizes or scholarships to be called the 'Charles Oldham' prize or scholarship.

President Needham, of George Washington University, has announced that Van Ness Park, purchased two years ago as the new site of the university, had been sold to the United States government as the site for the new building of the International Bureau of American Republics; and that the university held an option on 'Oak Lawn,' a tract of land at the head of Connecticut Avenue, which could be bought for $800,000. He stated that $400,000 was already in hand, and that Theodore J. Mayer of Washington had offered to erect a building to cost $185,000, on condition that this site be selected.

Oberlin College will celebrate the seventy-fifth anniversary of its foundation from June 19 to 25.

Dr. Henry Pratt Judson, professor of political science in the University of Chicago, and since the death of Dr. Harper acting-president, was elected president of the university on February 20.

It is understood that the presidency of the University of Toronto has been offered to Dr. M. E. Sadler, professor of education at the University of Manchester, formerly director of special enquiries and reports in the British Education Department.

Mr. George H. Locke, of Ginn and Company, recently dean of the College of Education of the University of Chicago and editor of the *School Review,* has been appointed dean of the School for the Training of Teachers in the Macdonald College, founded by Sir William Macdonald and affiliated with McGill University, Montreal. The new building of the college, of which Dr. James W. Robertson is the director, will be opened in the autumn.

William H. Jackson, M.A. (Cambridge), lecturer at Manchester University, has been appointed professor of mathematics at Haverford College.

At Yale University, Dr. Charles H. Judd has been promoted to be professor of psychology, and Dr. F. P. Underhill to an assistant professorship of physiological chemistry. Dr. Ellsworth Huntington has been appointed instructor in geography and Dr. William E. Hocking, assistant professor of philosophy.

Dr. H. T. Barnes, associate professor of physics at McGill University, has been promoted to the chair of physics, vacant by the removal of Professor Ernest Rutherford to Manchester.

Dr. Carl M. Wiegand, of Cornell University, has been appointed associate professor of botany at Wellesley College.

Professor Georg Klebs, professor of botany at Halle, has been called to the chair at Heidelberg, vacant by the death of Professor E. Pfitzer.

Dr. E. D. Holzapfel, professor of zoology in the Technical Institute at Aachen, has been called to the University of Strasburg.

Dr. Ernst Meumann, of Königsberg, has been called to the chair of philosophy at Münster as successor to Professor Busse.

SCIENCE

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THE AMERICAN SOCIETY OF NATURALISTS COOPERATION IN SCIENCE[1]

As investigators in science a great burden of responsibility rests on us. What our sciences shall be in the middle of the century depends on how we build at the opening of the century. History shows this to be so. In the last century embryology attained its importance because of the activity of its founders, including Wolff, von Baer, Kowalevsky and Balfour, while modern cytology received its impetus from the labors of such men as Fol, Flemming, Hertwig and Mark. As we look to the work of these men, so the future investigators will look back to us with a true and final judgment and determine our place in the development of our subjects. Well were it for us if this decade, this year and this meeting were memorable for an increased devotion to the scientific interests of which we have become the trustees. To advance these interests we should do well to adopt principles which have worked successfully in other fields of activity. In the modern commercial world one of the most important principles is cooperation. Let us consider the development of cooperation in science to learn how it may be advantageously applied further among naturalists.

The ancient Greeks made investigation of nature primarily to illustrate their personal systems of philosophy. This form of investigation, unhappily not yet wholly obsolete, is manifestly incompatible with

[1] Annual address of president read before American Society of Naturalists, December 29, 1906.

cooperation, which thrives better the more objective the method employed. Although Aristotle had a love of discovery, he and his followers were so scattered that at this period there was little opportunity for cooperation. With the development of astronomy and mathematics in the middle ages the number of centers of research multiplied and we find evidence of jealous rivalry between astronomers: for it was customary to announce discoveries in cipher, pending their confirmation or further study. If, in the meantime, the discovery was announced by another, the first published the key to his cipher and maintained his priority of discovery. This cipher is, I presume, the ancestor of the modern preliminary notice.

In more recent time astronomy, the best organized of the sciences, has led the way in cooperation. She has been almost forced to do so by the expensiveness of the equipment of an observatory and by the magnitude of the tasks before her. Consider, for example, the work of mapping the heavens, a work requisite for the eventual determination of the movements of the stars, but of such proportions, however, that it could not be accomplished by one observatory alone, with the desirable thoroughness, inside of one or two centuries. In the year 1887 an international congress of astronomers was called at Paris to consider cooperation in making a star-atlas and the adaptation of photography to the work. Eighteen observatories entered into the plan. The position of thousands of stars had to be determined directly by the meridian circle. It was necessary, also, to take over 44,000 photographs of parts of the heavens; a work involving great technical refinements. The star-atlas is now nearing completion, sixteen volumes out of the twenty that are to appear having been already issued. This undertaking stands as the greatest example of cooperation in the history of science.

Other cooperative enterprises have been started by astronomers, such as making latitude determinations, and advancing solar investigations. Thus, last year, the International Union for Cooperation in Solar Research voted the following principles which, with certain changes, might well be adopted in other sciences:

1. Cooperation is desirable in the various branches of solar research [as enumerated]. 2. When an institution has collected and coordinated results from various sources, members of the union shall be requested to place their observations at the disposal of the said institution. 3. In the case of investigations which have not yet been thus collected and coordinated, special committees specially nominated by the union shall be charged with the work of preparing and carrying out the needful cooperation. 4. It is proposed forthwith to organize such cooperation in two branches of research: (*a*) the study of the spectra of sun-spots; (*b*) the study of the records, by means of the H and K light, of the phenomena of the solar atmosphere. 5. The committee lays special stress upon the fact that, notwithstanding the obvious utility of cooperation in certain cases, individual initiative is the chief factor in a very large number. It is as much the duty of the union to encourage original researches as to promote cooperation.

The foregoing account shows that astronomers have acquired the excellent habit of combining forces to carry through a large project.

In certain other sciences, also, cooperation has long been practised. Thus in biology the collections of expeditions are usually worked over by many investigators who publish together in one series. The publication of the results of the *Challenger* expedition is one of the greatest examples of such cooperative work. These fifty thick quarto volumes, containing altogether 30,000 pages of letter press and over 3,000 plates, have been the work of scores of hands and the distribution of the labor was international. What is true of the *Challenger* expedition is true also of a score of other large expeditions; indeed it is the custom of systematic workers in different groups of animals and plants to cooperate without pay with any naturalist who may send them species for identification.

The international congresses which in the past few years have multiplied so as now to include nearly all of the sciences have stimulated cooperative undertakings. The International Association of Academies has organized commissions in the geodetic and seismological survey of the earth and on terrestrial magnetism. In electricity there are international committees on electromagnetic units and standardization. At a recent international meteorological congress cooperative work was initiated for the construction of a cloud atlas, also for studies on solar radiation, and no aeronautics. The exchange between nations by telegraph of current meteorologic observations is being constantly extended. The chemists have an international committee on atomic weights. The botanists and the zoologists have each an active international committee on nomenclature. The International Congress of Experimental Psychology has commissioned individuals to make comprehensive reports on special subjects. All the foregoing examples of the international congresses (of which others might be cited) involve much cooperation between men of science, who not only travel far to attend them but also work together in committees to further the investigations of largest scope.

The national societies involve, likewise, even though less strikingly, the spirit of cooperation. At this season of the year we prepare our papers with much pains, we leave our Christmas firesides and we travel great distances, spending freely of time and money, to advance these meetings that we believe to be for the common good. We meet together both formally and informally and we exchange very freely our ideas and discoveries. These meetings illustrate in the most practical way the spirit of goodwill, reciprocity and cooperation of this holiday season. The only regret that we hear, the only limitation to satisfaction that we feel, is that the programs are too full to permit of sufficient discussion and that several programs of common interest are running simultaneously. This year, in the biological sciences at least, a distinct improvement has been made, in that the sections of the association have cooperated fully with the special societies by arranging joint programs. A further example of cooperation this year is the joint session of the zoologists and botanists for topics in heredity and plant and animal breeding, and there have been in recent years a few individual cases of participation by members of one society in the proceedings of the other. Such cooperation between biologists is so helpful that it leads us to inquire whether it ought not to be better organized.

Let us consider for a moment the relations that have existed between the botanists and zoologists. The early systematists, such as Ray and Linnæus, included both plants and animals in their studies. Later, as species multiplied, the systematists of the two realms divided sharply. To-day we see the division carried still further on the basis of materials studied; so that we now have entomologists—lepidopterologists, coleopterologists, etc., indeed—and conchologists, ichthyologists, helminthologists, and so on. These divisions are excusable only in systematic work. One can not but regret to see scientific men segregating themselves on the basis of materials studied. In other sciences it is not so. To be sure, the chemistry of organic compounds assumed such importance some years ago that university chairs in that subject were organized and even societies founded; but I think it is true that no further segregation on the basis of material will take place in chemistry and that even now the group of 'organic chemists' is dying out. The newer problems of physical chemistry know not the old boundaries. The sole unit of classification of workers is the problem or subject.

So might it be in biology. The whole realm of living matter is one and indivisible. The fundamental laws of action of protoplasm, no matter how diverse its form, are everywhere the same. A comparative study of these laws on all sorts of material is necessary in order that the primary and essential may be separated from the secondary and non-essential. For example, the cell-wall was long regarded by botanists as the essential part of the cell, until studies on animal tissues showed that it had a secondary significance and that nucleus and cytoplasm are of more general importance. No student of plant physiology can fail to recognize how much modern concepts in that science have been influenced by studies on animals and even on man, while, on the other hand, the young science of general physiology of animals has received its true direction and impetus from studies on plants. In still other subjects we must recognize the identity of phenomena in the two kingdoms—little longer to be kingdoms, I trust, but soon united states. In both, processes of cell-division are essentially alike. Maturation, fertilization and cleavage of the egg differ only in illuminating secondary details; the general embryological principles are the same; the form is similarly restored after injury. Not less strikingly alike are the laws of fluctuating variability—since the delicate test of statistics shows no essential difference in the variation surfaces of plants and animals. As for mutation and inheritance, the warnings of some zoologists that the recent discoveries in plants should not be hastily applied to animals have fallen on deaf ears, for the fact is startlingly manifest that in all organisms these functions are identical. Even as producers of obscure diseases, plants can claim no distinction from animals. Within the last few years the number of disease-producing Protista that have animal affinities has increased by leaps and bounds until the very name of bacteriology threatens to become extinct. Pathogenic micro-organisms, without regard to their situation in the 'realms,' now constitute the material of the former bacteriologist. From all sides come forceful facts, beating down the artificial barrier that systematists and anatomists have erected athwart the field of biology.

This barrier must go. Already there are comparative cytologists, students of growth and regeneration, biometricians, thermatologists and protistologists who have destroyed much of it. General physiologies are written that disregard the old boundaries. Societies are being founded which, like the American Breeders' Association and the Deutsches Gesellschaft für Züchtungskunde, ignore the conventional dividing lines. Botanists and zoologists have gladly cooperated in these undertakings, having forgotten all minor differences in the essential fact of being biologists. The International Conference of Plant Breeders, held last summer, had for its president a noted zoologist.

Colleagues, there are many biological matters which call for immediate cooperation. There is the matter of the regulation of changes in the nomenclature of our some half million of species. This nomenclature is a cooperative work of the first magnitude, but is there any other instance of so large a cooperative undertaking with so little central control? This nomenclature is made up of the decisions of an army of men and women of the most varied learning, judgment and experience. To qualify for the work of adding to or altering this nomenclature no notice is given, no examination as to fitness is passed, no license or certificate is obtained. Nowhere is a statement as to minimum training necessary. A decision once rendered in print is thereafter to be quoted forever. The decision may, indeed, be reviewed and set aside, but the reviewing judge may be inferior in age and experience to the reviewed. Despite the fact that many of the judges are men of great learning and conservatism, the result of this uncontrolled cooperation has been and is infinite confusion. Only a small proportion of us here have anything to do in making biological systematic nomenclature, but all of us have to use it. And we are tired of the reproaches of our nomenclatorial brethren that, in referring to some species, we have overlooked a (usually *his*) recent change of a name. As biologists, we are to blame not for having overlooked a change of name, but for permitting the names of species to be changed by the whim of any one. As biologists, we are responsible for systematic nomenclature. It will not suffice to disdain the species monger. We are responsible for his existence. We may scoff at the condition of our nomenclature, but we can not forget that we permit it to be what it is and that here in congress assembled we could to-day take steps toward putting it on a rational basis. A commission should be appointed composed of representatives of both sides of the house, which should work with other committees already in existence, and which should report on the best method of controlling, here in America, the naming of species and the changing of established specific and generic names. It seems to me clear that such a commission would seek in vain for a universal natural basis of species. Arbitrary rules must be made and enforced by outlawing new names not in accord with them. It is time to stop the changes and adjustments of names to meet different ideals. We have a nomenclature which, in most groups, accords closely enough with nature for our purpose. Let us henceforth arbitrarily protect such a nomenclature. Some steps have been taken in this direction. As indicated above, the international congresses of zoology and botany have established certain very general suggestions as to nomenclature. American ornithologists have gone further and laid down the rules under which new species shall be created. They have even published a list of specific names which are to be recognized. The international congress of botanists last year approved a series of *rules* and *recommendations*, enacting that 'a name contrary to a *rule* can not be kept up.' It passed a list of generic names, which, from long-established usage, are to be retained, though on the principle of priority they should be rejected. This action is important, in my opinion, because it affords a precedent for establishing generic names by vote of a congress. While an American *biological* commission on nomenclature is desirable, in order to begin the immediate consideration of reforms in our methods of naming species and to gain experience, we should work toward a permanent international committee on *biological* nomenclature.

Important though nomenclature is, it can not command the same interest as research. There are large undertakings in the field of general biology that require a more systematic cooperation than yet exists. The greatest of these is the experimental study of the factors of evolution—experimental evolution, in brief. The station established by the Carnegie Institution of Washington has done something to enter into cooperative relations with workers in this field both in America and abroad. These associated workers would doubtless be very glad to serve as a committee to report to a society of biologists.

The matter of the significance and control of sexual dimorphism is one of such moment that it might well be assigned to a special commission including both cytologists and experimental breeders. The subject of the physiology of ontogenesis, including experimental embryology, form regulation, and experimental morphology, is one in which American zoologists and botanists have made their country famous, yet the exploration of the subject has only been begun. The investigators should all come together from time to time to consider new lines of advance. As further examples of investigations needing cooperation I may mention the determination of biogeographic centers and the routes and means of dispersal; the basal instincts and reactions of organisms—but these will suffice as examples of subjects of common interest to all the biological societies; zoologists, botanists, bacteriologists, anatomists, physiologists, thermatologists and psychologists. Such subjects may not be left to the different societies separately. It is because none of the existing special societies can appropriately assume charge of these general biological topics that their interests have not been as much advanced as they ought to have been. Some attempt has been made to meet the need by occasionally arranging joint meetings between botanists and zoologists, and in the last two discussions of the naturalists a symposium has been held on some general biological topic. But it is clear that there should be a special society for the cultivation of these subjects.

The American Society of Naturalists was established in 1883 as an association of professional naturalists. The original call was signed by fourteen persons, all but one biologists. Although many geologists joined the society later, most of them subsequently withdrew to concentrate their interests on the Geological Society of America. Many other special societies have sprung from the loins of the Naturalists, but, for the most part, the individual biologists have clung loyally to the parent society. It has been suggested recently that the Society of Naturalists is an anachronism; that its interests are too diffuse; that we must concentrate now on the special societies; that, now her children are grown, the mother should die. With this view I do not agree. I have tried to show that there is not now less need but more of a synthesizing biological society with the following aims: To arrange for an annual discussion of some burning biological topic; to arrange with the special societies one session for technical papers of interest to both zoologists and botanists as well as biologists of other societies; to arrange, through the appointment of commissions from time to time, for cooperation in the control of biological nomenclature and for the cooperative study of certain large topics. Such commissions should be composed of those specially investigating those topics and should do what they can to encourage independent work also in these lines. They should report briefly each year to the society.

Next year the Society of Naturalists will celebrate the twenty-fifth anniversary of its birth. Can it do so more fittingly than by arranging a series of brief reviews of the progress in the past quarter of a century of the larger cooperative undertakings in biology, with suggestions as to their better organization?

C. B. Davenport

THE BIOLOGICAL SIGNIFICANCE AND CONTROL OF SEX[1]

THE NATURE AND SIGNIFICANCE OF SEXUAL DIFFERENTIATION IN PLANTS

You will pardon me if in discussing the subject which has been assigned I take my

[1] Five addresses given before the American Society of Naturalists at Columbia University, New York, December 28, 1906.

illustrations chiefly from the forms personally familiar to me. The researches of the past few years have demonstrated sexuality in the ascomycetes, the rusts and the yeasts, and some would see sexuality in slime molds and even in certain bacteria and blue-green algæ. At present one would hardly dare deny the possibility of sex in any group, however low in the scale of development. Processes so universally present among both plants and animals one would expect to have some significance in organic development and in the life of the individual. Yet very little is really known about the fundamental questions of sex. We do not know what constitutes a sexual process; what the real difference between male and female actually is; nor, finally, do we know what advantage, if any, the rather complicated sexual process has over other methods of reproduction. A mustering of the facts will enable one to say that apparently nothing is accomplished by sexuality that can not be equally well accomplished by purely non-sexual methods of reproduction.

The theory that the sexual union gives renewed vigor of growth would lead one to expect that forms that multiply exclusively by non-sexual methods would show signs of weakness and eventually die out. In fact, however, non-sexual forms are as vigorous as sexual ones. One could often wish that such were not the case when one finds cultures of sexual molds overcome in the struggle for existence by bacteria and non-sexual forms of *Penicillium*. It seems probable that a considerable number of forms among the flowering plants, of which the ubiquitous dandelion may serve as an example, have dispensed with the sexual method and reproduce parthenogenetically without signs of loss in vigor. Many examples could be given of higher plants which have been cultivated since historic times by non-sexual methods. During the last four years, the male and female races of a number of different species of the molds have been cultivated by means of non-sexual spores in separate test tubes where it is not possible for them to reproduce sexually. In two species 115 non-sexual generations have been reached without apparent change in vegetative or sexual vigor.

As to the variations which sexual reproduction is supposed to favor or to check by a blending of male and female characters, it can be shown by horticultural records that plants propagated by cuttings or by other non-sexual methods are less likely to vary than those grown from sexually formed seed. Yet distinct varieties do frequently arise non-sexually and are to be distinguished in no respect from varieties obtained from seed.

There is great difference of opinion as to what constitutes a sexual act. Nuclear union apparently takes place previous to formation of so-called apogamous embryos in certain ferns and would seem to take the place of the typical sexual process. If graft hybrids are possible, as seems probable from recent investigation, we have in this association of vegetative cells from two different individuals the blending of characters obtainable through the union of differentiated sexual cells. Nuclear fusions have been repeatedly observed in vegetative organs of higher plants. Moreover, by treatment with certain chemicals fusion nuclei may be obtained which divide with an increased number of chromosomes. When brought back to normal conditions the number is reduced to that usual to the plant. The phenomena have nothing to do with reproduction and may, therefore, be considered merely as a sacrilegious juggling on the part of the experimenter with the sacred x and $2x$—the gametophyte and sporophyte—rather than as giving any evidence of sexual character. The experiments are interesting, however, in showing the extreme plasticity of the plant cell.

There seem to be a number of different stages in a sexual act which may be passed through more or less independently of one another. Cell union in many forms is not at once followed by nuclear fusion. In the Desmids, for example, the nuclei do not fuse till the germination of the zygospores and in the rusts a whole life cycle is interpolated between the sexual union of cells and the fusion of nuclei. The fact that in certain hybrids the maternal and paternal chromosomes seem to retain their individuality throughout the whole plant up to the reduction division in the formation of the sexual cells, would indicate that delayed fusion of chromatic substance may be a common phenomenon, although generally less easily detected than in the rusts.

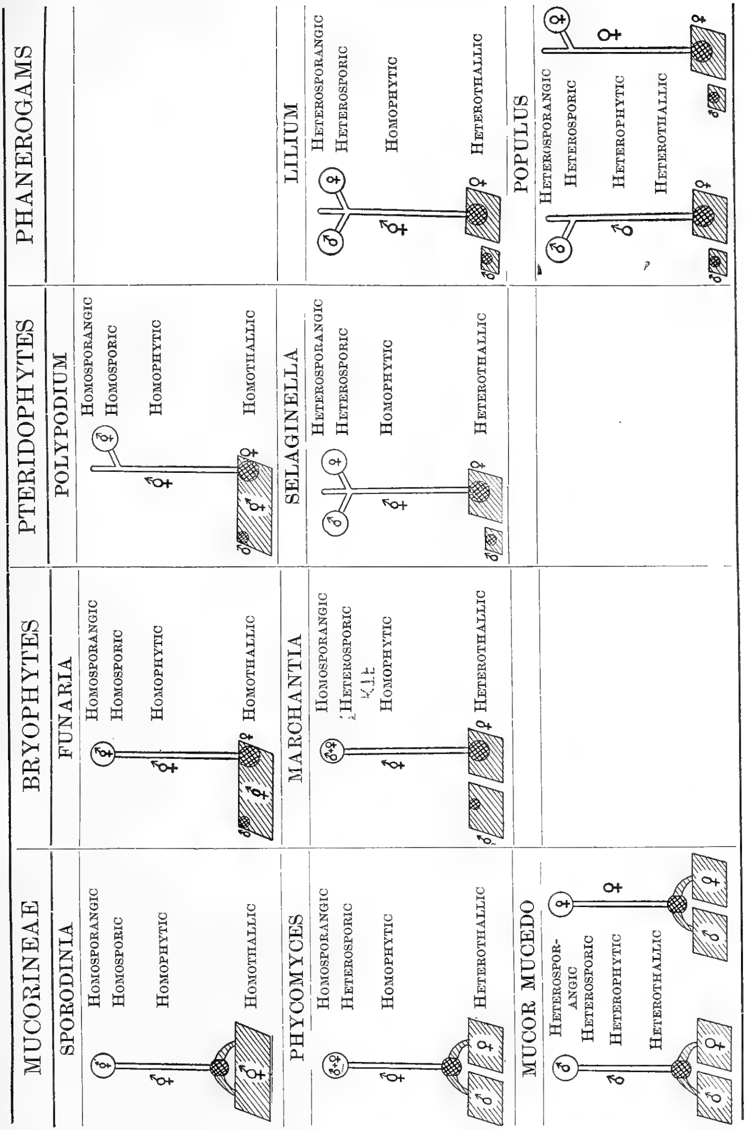
Not only may cell and nuclear unions occur independently of each other, but a distinct sexual reaction may take place leading to the formation and approximation of the sexual elements, though not to their union. In the mucors, when a sexual race of one species is grown between the male and female races of a different species, the sexual reaction between the races of opposite sex is shown by a white line due to the accumulation of imperfect hybrids. The reaction is sufficient only for the formation and mutual attraction of the conjugative branches. The second stage—the fusion of the sexual cells—occurs only when the opposite sexes belong to the same species and apparently can not take place between the opposite sexes of different species.

In treating the evolution of sex, it is customary to confine the attention to the progressive differentiation of the gametes alone. Differentiation of sex on separate individuals, whether or not accompanied by a differentiation in size of the gametes, would seem, however, to be the highest stage reached in the development of sex. The mucors may conveniently be taken as a basis of our discussion of this differentiation. There are comparatively few species in which the two sexes are united upon a single plant. If we assume this hermaphroditic group to be the more primitive, we have a progressive differentiation in two directions: first, to heterogamy, *i. e.*, to a constant dissimilarity in the gametes; secondly, to a constant dissimilarity in the sexual plants themselves. The large majority of the mucors have the sexes on separate individuals. Since the plants are capable of multiplication by non-sexual spores, a single sex may be indefinitely propagated non-sexually and the offspring thus obtained may be spoken of as male and female races.

It is difficult to conceive of a blending of characters in hermaphroditic species when the gametes may come from the same branch, even if they are in some species of different size. In diœcious species zygospores have been obtained from matings of male and female races which in one species came from as diverse regions as China and Cambridge, Mass. In such forms the environmental conditions under which the sexually opposite races have grown may be sufficiently different to furnish a basis for the advantages assumed to accrue from a blending of maternal and paternal characters in the offspring.

The mucors form the only group of the lower cryptogams in which the presence of sexual races has been demonstrated.

In plants a differentiation is more or less apparent into a stage bearing the gametes and a stage bearing the spores. The accompanying figures represent diagrammatically the sexual character of these two stages in certain groups of plants. The stage


MUCORINEAE
SPORODINIA
HOMOSPORANGIC
HOMOSPORIC
HOMOPHYTIC
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PHYCOMYCES
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HETEROTHALLIC
BRYOPHYTES
FUNARIA
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producing the gametes is the gametophyte. The stage arising from the germination of the zygote is the sporophyte. All forms which are hermaphroditic in the gametophyte are of necessity hermaphroditic also in the sporophyte. Every part of the mucor species *Sporodinia,* therefore (with perhaps exception of the sexual organs), contains both sexes. The same is true of some of the mosses and probably of all of the homosporous ferns. No representatives of this type exist among the flowering plants.

Phycomyces is diœcious in its gametophyte. The zygospore formed by conjugation of branches from male and female individuals produces at germination but a single kind of germ tube which gives rise to a sporangium containing both male and female spores. The sporophyte therefore is hermaphroditic. That the germ tube in fact contains both male and female characters may be proven by forcing it to grow directly out to a vegetative mycelium before the formation of spores. The growth thus obtained is distinctly different from either the male or female individuals characteristic of this species and shows its possession of both sexes by the fact that it produces both male and female spores as well as sexually formed zygospores.

The liverwort *Marchantia* resembles the mucors in that its gametophyte is multiplied by non-sexual reproductive bodies and in that the sex of the spores of the sporophyte is not apparent until after their germination. I have been able to show that this liverwort corresponds to the *Phycomyces* type and its sporophyte must be considered hermaphroditic since both male and female spores are found in a single sporangium. As with *Phycomyces,* the determination of sex does not take place in the zygote but a sporophytic interval is interpolated between the zygote and the germ sporangium where the segregation of sex finally occurs. The heterosporous ferns illustrated by *Selaginella* differ from *Phycomyces* and *Marchantia* chiefly in the reduction in size of the gametophyte and in the fact that male and female spores are produced in separate sporangia. All four types discussed are diœcious in gametophyte while hermaphroditic in sporophyte.

Mucor Mucedo has the sexes separated on different individuals as in *Phycomyces,* but two different kinds of germ tubes are formed by the germination of its zygospores. While some germ tubes are male and produce only male spores, others are female and produce only female spores in the germ sporangium. The sporophyte as well as the gametophyte therefore is unisexual. The same is true of the diœcious flowering plants represented by the poplar. There are no ferns of this type and none are known among the mosses, but the sexual differentiation in the latter group has been but little investigated.

Forms like the bacteria apparently have not developed sexuality, others seem to have lost it. The loss may be permanent as in the habitually parthenogenetic forms, or merely temporary. Thus the sexual races of one of the mucors has been rendered temporarily neutral by cultivation for a few non-sexual generations at unfavorable temperatures and neutral races of several species have been found in nature.

It is among the algæ and fungi that the influence of external factors upon the method of reproduction has been most carefully investigated. The limits within which growth is possible are further apart than those within which the formation of the reproductive bodies can take place. Plants will grow under conditions where they can not reproduce. Similarly the conditions for the formation of sexual and non-sexual reproductive bodies do not always coincide, the limits being narrower for the sexual than for the non-sexual spores. In many instances the influence of the external factors has been more or less definitely determined and by varying the cultural conditions one may obtain either form of fructification desired. Thus in the hermaphroditic mold, *Sporodinia,* one obtains exclusively non-sexual spores on a substratum deficient in nutrient but may obtain the zygospores by increasing the concentration of the nutrient.

Again, the conditions under which the male and those under which the female organs can form do not always coincide. We may illustrate this by the effect of the single factor light. The prothalli of homosporous ferns under minimum illumination may be brought to prolonged vegetative growth and thus the formation of both male and female organs be suppressed. When the amount of light is increased to a certain extent only male organs are produced from these sterile prothalli. To obtain female organs they must be exposed to a still greater illumination. Such suppression of the organs of a single sex on plants normally showing both sexes is not to be confused with sex determination in the offspring of diœcious plants.

There are a number of facts which are assumed to indicate that in all diœcious plants one sex is dominant and makes its appearance while the other remains latent. Thus a female plant is considered not pure female, but is supposed to contain the male character in a suppressed condition though not capable of being brought to light by simple changes in external conditions. Male and female willow plants are frequently found with flowers of the opposite sex, and as has been already explained, an hermaphroditic condition has been produced in *Phycomyces* which is normally diœcious. Moreover, by cultivating this hermaphroditic growth by transfers of the vegetative mycelium, it eventually loses its hermaphroditic character and in a few mycelial generations is transformed into a male or female growth indistinguishable from one of the sexual races normal to this species. We know too little about what the fundamental differences between male and female actually are to be able to conjecture in what way this suppression or elimination of one sex is accomplished.

Especially interesting in this connection is the diœcious plant *Lychnis dioica.* This wild pink, which has the sexes on separate individuals, is subject to the attacks of a smut fungus which is able to fruit only in the anthers or male organs. If it attacks a male plant it fruits in the male organs already present, coloring them violet. If a female plant is infected which normally never bears male organs, the growth of the parasite in some way stimulates its host to the production of male organs within which it may form its spores. This forms rather a striking example from the plant kingdom of doing good unto those that despitefully use you. Attempts have been made to artificially influence the sex in this plant, but entirely without success. It seems undoubted that in sporophytes of some plants the appearance of one sex can be suppressed in a similar fashion to that so well known for the prothalli of the homosporous ferns, but one is hardly warranted in assuming that in no forms the male and female individuals are ever sexually pure. The sex may perhaps be pure in the gametophyte while mixed in the sporophyte.

Some would question the sexual purity even of the gametes themselves. If the gametes contain but a single sex their development without conjugation in hermaphroditic forms should give rise to unisexual individuals—the male gamete to male and the female gamete to female individuals. Some experiments are in progress which it is hoped will throw some light upon the sexual condition of the gametes in certain of the molds.

It is only by the further accumulation of facts in various groups of plants and animals that we may at length be in position to determine what if any unifying principle there may be in this wide-spread phenomenon of sexuality.

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THE BIOLOGICAL SIGNIFICANCE OF SEXUAL DIFFERENTIATION—A ZOOLOGICAL POINT OF VIEW

The line of descent in multicellular animals is through a continuous or discontinuous series of sexual generations. In the latter case, there is alternation of generations, either of asexual and sexual or of parthenogenetic and sexual generations. So far as I know, the sexual generation is never absent in the first kind of alternation; there are, however, some parthenogenetic species in which males have never been found, though the structure of the females, or the natural history of the race, proves the former existence of males. There is only one feature common to all forms of sexual reproduction, and that is the union of ovum and spermatozoon to form a single cell, which has the capacity of developing into a new individual of the species. The biological significance of sex must, therefore, lie in the process of fertilization; and the interpretation of the fundamental significance of fertilization must be the answer to our problem.

Now fertilization is a more general phenomenon than sex itself, for it is characteristic of the Protozoa in the form of conjugation; and it appears to be a growing conviction among students of Protozoa that conjugation is universal in this group. Fertilization brings about biparental inheritance or amphimixis, and some have regarded this as its chief function, in view of the great importance of amphimixis for the process of evolution. But most zoologists regard amphimixis as a secondary function of fertilization, and find the chief significance of fertilization in the satisfaction of a periodic physiological need of the organism. The ovum usually requires fertilization as a stimulus to development; without it, in most animals, the processes of development either do not begin or soon cease. Observations on normal and artificial parthenogenesis demonstrate that it is not an indispensable requirement for development; however, in most parthenogenetic species fertilization-need arises in certain generations that alternate more or less regularly with the parthenogenetic ones; and those parthenogenetic species in which males are unknown have descended from sexual species, and moreover belong to specialized groups on one side of the main trend of evolution. Among Protozoa there seems to be a periodic need of fertilization to maintain the capacity of the species for reproduction.

We may then say, with the qualifications already indicated, that among animals at least the law of conjugation is as universal and imperative as the law of hunger. It is thus one of the most general of biological phenomena, with an element of obscurity in it that does not inhere in any other major problem of biology; for, as katabolism is combustion, the need of hunger to incite the individual to the taking of food is obvious; as the individual survives by adjustment to its environment, it must possess irritability and motility; but why the same food that satisfies for so long fails ultimately to support ebbing vitality among Protozoa, why the line of descent in Metazoa should pass through sexual generations —this is the mystery of physiology; and that salvation of the race should reside in the union of particular cellular individuals, has in it none of the obviousness of the individual's preservation by the taking of food.

The nature of the periodic need for fertilization has been differently conceived by different writers. Weismann believes that fertilization-need has arisen in the course of evolution in order to ensure the advantages of amphimixis to the race; it has no fundamental physiological basis. R. Hertwig propounds the hypothesis that there is an innate tendency to acceleration of the vital processes, due to the gradual adaptation of nucleus and cytoplasm in their copartnership, which proves gradually harmful and ultimately fatal. Fertilization checks the acceleration by introducing a foreign nucleus, unaccustomed to the protoplasmic milieu; but the new partners in the vital process gradually accelerate the speed until a second fertilization again checks the dangerous pace. Fertilization thus marks the return to a state of stability from a state of extreme cellular lability. Herbert Spencer, on the other hand, regards the vital processes as tending towards a state of equilibrium or fixed stability; fertilization restores the labile condition of the cell. According to Geddes and Thompson, fertilization may be compared to mutual digestion and may have arisen from a nutritive want. "With the differentiation of the elements on anabolic and katabolic lines, the nature of the fertilizing act becomes more definite. * * * The union of the two sets of products restores the normal balance and rhythm of cellular life."

The point of view that has been, perhaps, the most acceptable to zoologists was stated originally by Bütschli, Engelmann, Minot and Maupas, and was based primarily on the study of conjugation in infusoria. It was discovered that, during the series of asexual generations, there is a gradual diminution of vitality expressed in a reduced rate of division and in certain morphological changes that may be collectively designated senescent. Conjugation changes all this; senile processes cease, the division rate is restored. Thus conjugation apparently reverses the process of senescence, causes rejuvenescence. By an extension of this idea it was assumed that in Metazoa the fertilized ovum starts out charged with abundant vitality, which is, however, gradually exhausted, and the race is saved only by fertilization, which is here also interpreted, by a reckless transfer of terms, as rejuvenescence.

The majority of zoologists appear to be agreed that fertilization-need is a primordial physiological condition, more than a mere adaptation to ensure amphimixis; and though there are weighty authorities on the other side, this point of view appears to me to be right, even though the theories of the nature of the need and its satisfaction are inconsistent. The idea of Spencer is too indefinite to serve either for foundation of a more extensive theory, or as basis for observation and experiment. That of R. Hertwig is in opposition to so many known facts as to be untenable. The conception of rejuvenescence has a flavor of mysticism, and involves a confusion of ideas. It implies that the gametes are senescent before fertilization, but the only significance of the term senescent is in its application to the soma. That the germ-cells before fertilization are old, in the sense that tissue-cells become old, would be asserted by no one. They are, on the contrary, the spring of eternal youth, and all that can be asserted objectively is the necessity of fertilization for their continual functioning.

Even in the case of Protozoa there is no reason for assuming that the part principally concerned in conjugation, the nucleus, is itself old; the cell-body undoubtedly becomes old, and after conjugation it apparently resumes its vigor. But the latter process is more correctly interpreted as renewal instead of rejuvenescence; the old body is sloughed off a little at a time and gradually renewed after conjugation from the germ-plasm, because it can not be cast off an entire dying body as in Metazoa.

I believe that Weismann is correct in his contention that the conception of rejuvenescence has not a shadow of support among the Metazoa, indeed would never have been conceived from what we know about the Metazoa themselves; and also that the conception is baseless as applied to Protozoa. The conception of germ-plasm and soma is as necessary for Protozoa as Metazoa, and the conception of senescence is unmeaning as applied to germ-plasm. But Weismann's conclusion that the Protozoa are potentially immortal does not follow; the protozoan soma is no more immortal than that of the Metazoa, and it is as little subject to rejuvenescence.

No theory of sex can be consistent that divorces the physiological significance from the causes of sex-differentiation. In the physiological significance, that is, in the nature of the fertilization-need, we must find the primary cause of sexual differentiation. Richard Hertwig has been one of the very few to recognize this axiomatic principle; but he nevertheless states two hypotheses, one of the physiological significance of fertilization, the goal and ultimate attainment of sex-differentiation, and the other of the causes of sex-differentiation itself, and these have no logical connection. The main value of Geddes's and Thompson's otherwise vague and unsatisfactory theory of sex lies in their appreciation of the connection between the physiological significance and the causes of sex-differentiation.

In Metazoa, fertilization is always selective, *i. e.,* between unlike gametes. Ovum does not fertilize ovum, nor do spermatozoa conjugate. It is true that a phenomenon known as fertilization by the second polar globule has been described in a parthenogenetic egg, but it is altogether improbable that it has the physiological value of fertilization. In Protozoa, also, fertilization is often selective, *i. e.,* between differentiated gametes, and there are various degrees of differentiation from conditions essentially similar to the reproductive cells of Metazoa, to relatively slight unlikeness of gametes; and the latter grades into the conjugation of like gametes, which seems to be the primitive condition. It is almost universally believed that selective fertilization does not exist when the gametes are alike; any two gametes may unite. It, therefore, follows that the fertilization-need is the same in both gametes (even when they are differentiated). And from this idea arises the inconsistency between theories of the significance and the causes of sex differentiation; for if the gametes are in the same physiological condition, their differentiation, and sex differentiation itself, can only be devices to secure gametic union.

But there is an alternative point of view, viz., that fertilization may be always selective, even when there is no morphological gametic differentiation. I am convinced that only on such an assumption can a consistent theory of sex differentiation be constructed. If gametes be physiologically different, even when they are morphologically alike, then morphological differentiation of gametes follows naturally as an expression of these physiological differences, and sex-differentiation as a further stage in the same process of evolution.

Now Calkins has clearly demonstrated the probability that fertilization is selective even when the gametes are morphologically alike. He showed that, in *Paramœcium,* one of the ex-conjugants in each case has the greater vitality. He concludes: "This indicates that there is a physiological difference between *Paramœcium* gametes analogous to that existing between egg and spermatozoon."

It would seem that the view that species have periodic phases of inefficiency, corrected by fertilization, is well founded. But it is difficult to see how the union of two like inefficiencies may restore efficient functioning. I have much sympathy with Weismann's strictures on the hypothesis that the union of two senescent beings may produce one rejuvenated being. But if we conceive fertilization as always selective, *i. e.*, between physiologically differentiated gametes, then the fertilization-need must be different on the two sides; and this may be conceived in one of two ways: either the gametes represent plus and minus deviations, respectively, from the physiological mean, in which event fertilization might be supposed to be a reciprocal process; or one gamete may be supposed to act as stimulus and the other as the element stimulated, in which event fertilization would not be reciprocal, but one-sided. Now, fertilization in ciliate infusoria has always been supposed to be a reciprocal process, and the morphological phenomena are all in favor of this point of view; but Calkins's results indicate that only one of the ex-conjugants is benefited; the fertilization is one-sided physiologically.

In either event, union in the zygote would restore the physiological mean or condition of equilibrium, and the question would arise, how the differentiated conditions are subsequently produced. Nothing definite can be said about this at present; but it is obvious that the protoplasm does tend inevitably away from the condition of equilibrium towards one or the other differentiated condition; the direction of the tendency appears to be dependent on stimuli.

The objection may be raised that in some animals sex is certainly determined in the ovum at the time of fertilization, whereas, according to the point of view presented, fertilization is supposed to balance the physiologically differentiated conditions on which sex depends. Sexual determinateness of the fertilized ovum may, however, be interpreted to mean only that the sex-determining factors, primitively external, have been replaced by internal conditions in these cases. It is certainly not an illogical position that physiological neutrality in regard to sex may coexist along with internal conditions that absolutely restrict sexual differentiation to one direction.

In his thoughtful and suggestive paper on 'The Phenomena of Sex Differentiation,' Watasè comes to the conclusion that

> The organism is either a male or a female, not by the difference of primary sexual characters alone, but by the difference which saturates the whole of its entire structure. Such a difference is, however, neither absolute nor permanent. It is a temporary differentiation of protoplasm into one of two different directions, and sooner or later comes back to the original neutral or non-sexual state from which it started, thus manifesting the phenomenon characteristic of all protoplasmic irritability.

His point of view is instructive; there is a sexually indifferent stage of the organism corresponding to the period of union of the germ-nuclei; sexual differentiation is a phenomenon of irritability or response to stimulus, which lasts throughout the life history of the growing organism; 'and the recurrence of the irritable condition corresponds to the production of the unicellular embryo.' Sex differentiation is thus one of the phenomena of irritability, and it differs from other phenomena of this class only in the slowness of its rhythm.

Watasè's conclusions were based on the observations of Auerbach, himself and others, that the staining reactions of the egg and sperm nucleus are entirely different during the earlier stages of their existence, but that these differences disappear at the moment of fecundation. It has since been shown that the difference in staining reaction of the germ nuclei is probably of secondary significance only, but the view that a primary physiological difference between the germ-nuclei exists, is not necessarily excluded.

The question has arisen whether we are to deny the old biological conception of a sexually indifferent stage in the life history? It seems to me that this conception is as necessary and fundamental to-day as it ever appeared to be, and that we can not depart from it without involving ourselves in absolutely hopeless theoretical difficulties.

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SEX DETERMINATION IN RELATION TO FERTILIZATION AND PARTHENOGENESIS

IT is not an easy task to attempt a brief discussion of the relation of sex determination to fertilization and parthenogenesis; for the fact may as well be admitted at the start that we are not yet in a position to make any general statement as to what that relation is, and it is my impression that the subject is not yet ripe for discussion. We are not yet, I think, in a position to conclude with certainty in any single case that fertilization can be considered as a sex-determining factor, not even in the classical case of the bee. Even in cases which at first sight seem clearly to show that fertilization is such a factor, consideration will show that we can not, or at any rate have not, shut out the possibility that fertilization may be determined by sex rather than the reverse. There is the same uncertainty regarding the relation of sex production to parthenogenesis. There is no constant relation between these two processes, for the parthenogenetic eggs of a single individual may in the same species produce females only, males only, or both males and females. Both fertilization and parthenogenesis, in fact, present us with a series of relations to sex production in which the common factor, if there be such a factor, still eludes us.

There are two primary data which, I think, must be taken as our point of departure in any attempt to discuss these problems. The first is the long-known fact that in a few cases, of which the best known are those of *Dinophilus apatris* and *Hydatina senta*, the eggs are visibly distinguishable by their size as males and females, before fertilization or even maturation. Neither fertilization nor maturation, accordingly, can here be a sex-determining factor. We only know, if the results of Maupas and Nussbaum on *Hydatina* and the more recent ones of von Malsen on *Dinophilus* be well founded, that in these cases the ratio between male eggs and female eggs may be modified by conditions of temperature, or nutrition, or both, that affect the mother before the eggs are laid; but the true interpretation of this is still very far from clear. The second primary datum is that in many insects, and probably in many other air-breathing arthropods, the spermatozoa are predestined in the constitution of their nuclei, as males and females, or better, male-producing and female-producing forms, in equal numbers. Here, however, our actual knowledge ends, so far as fertilization is concerned. We do not know in any single case whether the predestination exists in both eggs and spermatozoa in the same species. Until we can be sure on this point it is almost idle to speculate on the subject; for if such a double predestination exists there must obviously be a selective fertilization, such that each form of egg is fertilized by the appropriate form of spermatozoon; and if this be so, sex is not determined by fertilization, but fertilization by sex. Until this question has been decided it will be hazardous to venture any conclusions as to the causal relation between sex and fertilization. We may, however, be quite certain that in parthenogenesis the egg alone is competent to determine sex; and for this reason one finds it difficult to avoid the feeling that this is likely to be true of eggs that are fertilized, even though we have direct evidence of this in only two or three cases. There is a certain amount of cytological evidence that such is actually the case, at least in the Hemiptera and Coleoptera, and probably in other forms that possess a pair of idiochromosomes or an odd chromosome. On the other hand, the strongest piece of evidence against this is, of course the longstanding one of the bee; yet even here one of several possibilities is that only those eggs are fertilized that are already predestined as females. This case, I believe, is not as yet closed.

For the foregoing reasons it seems to me that our best hope of a successful attack on the problem lies in the study of parthenogenesis; though we are here still confronted by too complicated and puzzling an array of facts to be at present surmounted by any one interpretation. The limitation of time forbids any adequate review of these facts, and I must limit myself to a single and, I fear, somewhat one-sided line of treatment. It seems to me that the most available stepping-stone towards the investigation of this problem is afforded by recently acquired evidence that sex production stands in some definite causal relation with the chromosomes and may be treated from the standpoint of the Mendelian phenomena, as interpreted by the Sutton-Boveri chromosome theory. It is certain that in many of the insects there is a particular pair of chromosomes that have a special and constant relation to sex production. There is perfectly clear evidence that the two members of this pair couple in synapsis and are disjoined in the reducing division. There is very strong, though indirect, evidence that one of them enters a male-producing spermatozoon, the other a female-producing one. A very definite material basis, therefore, exists for a treatment of the sex characters as if they were Mendelian alternates, sex determination, as opposed to sex heredity, being a matter of Mendelian dominance, more specifically of chromosome-dominance. I think that apart from the specific evidence in favor of this view a strong *a priori* argument in its favor is the approximate numerical equality of the sexes, which may be taken as the prevailing rule. The existence of a pair of chromosomes that are specifically related to sex production, and in respect to which the gametes of both sexes fall into two equal classes, gives a simple and natural basis for an equal production of males and females if we assume that these chromosomes embody respectively the male-producing and the female-producing factors. In other words, these two chromosomes may represent the hereditary bases of the male and female characters, respectively; and their relations to each other in respect to dominance may condition sex determination as opposed to sex heredity.

Let us briefly consider sex determination in parthenogenesis from this point of view. In the parthenogenetic egg sex might conceivably be determined either by elimination from the egg of the male or female element in maturation, or by conditions that affect the relations of dominance between the chromosomes. The hypothesis of elimination, which has been discussed especially by Castle and Doncaster, demands a reducing division, at least in case of the sex chromosomes; and such a division is not known to occur without at least a temporary reduction in the number of the chromosomes. This view may perhaps give a true explanation in certain cases; but formidable, if not fatal, difficulties stand in the way of its acceptance as a general principle of interpretation. In the case of the bee, for example, as Castle himself pointed out, if it be assumed that the female element is uniformly eliminated in the maturation of the parthenogenetic egg, the female element must be reintroduced by the spermatozoon; but the spermatozoa are produced by males that arise from parthenogenetic eggs, which by the hypothesis have eliminated the female element. Castle ingeniously endeavors to meet this difficulty by taking refuge in the conclusion of Petrunkewitsch that the testes are not formed from the egg proper but from a fusion-nucleus formed by union of two polar nuclei, in which the female element is present; but until decisive evidence is available that the testes really have such an origin in the male bee it seems to me impossible to regard the explanation with anything but skepticism. But better and more direct evidence than this, free from any hypothetical element, is afforded by the observations on aphids, recently brought forward by Miss Stevens. If her conclusions are well founded, as they seem to be, in these animals no process of synapsis or reduction occurs in any of the parthenogenetic eggs, whether they produce males or females; though the sexual eggs and the spermatocytes undergo reduction in typical fashion. The principle of elimination here appears to be ruled out of court as a sex-determining factor, and it only seems possible to explain the result by the assumption that throughout the summer broods there is a uniform dominance of the female element, and that males are produced from eggs in which a reversal of dominance takes place. That something of the kind occurs is indicated by the fact that both in aphids and in daphnids the same parthenogenetic mother may produce both male and female offspring; and in the daphnids the condition in which this occurs is shown by the recent experiments of Issakowitsch to arise in response to a change of the environment.

Whether similar considerations of dominance and recessiveness will afford a general explanation remains quite an open question, but they seem at least sufficiently plausible to be taken as a convenient working hypothesis. By its aid we can work out on paper a formal explanation of the mechanism of sex production that will include nearly all known cases, and will also include the determination of sex by external conditions (if it be admitted that such a process takes place). It would not, I think, be profitable to go into such speculative constructions in detail here. They are but fireside dreams which may serve a useful purpose in the safe seclusion of the study, but really belong in the same *limbo* with the so-called 'fool experiments' which all of us at times secretly practise. I dare say the general view that I have briefly sketched will appear to some as only a restatement of the problem done over into the Mendelian jargon. I venture to think, however, that it is a little more than this. A real advance has been made if it has become possible to connect sex production with a definite nuclear mechanism that gives us a tangible handle by which to take hold of the problem. But I hardly need add that this should not be considered as giving more than a tentative point of attack. It is entirely possible that we are on a wrong track, that the so-called sex chromosomes are only associated in a definite way with the sexual characters, and have in themselves no causative influence on sex production. The whole chromosome theory of heredity, for that matter, stands unproved before the judgment seat. I repeat, therefore, that the subject is not yet ripe for discussion; and what we need now is not more theory or discussion, but more observation and experiment. I believe that the chromosome theory as applied to the sex problem presents a sufficiently plausible face to be taken for a time as a guide to further examination of the facts. Perchance the true explanation may be found on the way, even should our working hypothesis prove a false leader.

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SEX-DETERMINING FACTORS IN PLANTS

IT is generally agreed that no true sex-determining factors for plants have as yet been recognized, and naturally a botanist would hardly choose the topic which has been assigned to me in this discussion. Claims are, however, constantly made that this or that environmental influence has been observed to modify slightly at least the percentage of the sexes in diœcious plants. The extensive literature of such experiments and observations was summarized very fully and critically by Strasburger in 1900, and I need only allude to it here.

A great variety of factors have been tested both singly and in combination, but without positive results. Conditions of nutrition, as to both kind and amount, have been exhaustively studied and reports of success in influencing the sex ratio by this or that fertilizer are constantly made, and quite as constantly fail of confirmation. Laurent has recently (1903) claimed that an excess of nitrogen or lime favors the development of males in spinach, hemp, etc., while potash and phosphoric acid favor the development of females, but his results are not convincing.

Temperature, light and moisture conditions, relative age of parents, relative maturity of pollen, early and late planting, pruning, etc., have all been more or less elaborately tested without achieving results.

Gallardo (1901) reported that wild female plants of *Dioscorea* and *Clematis,* when transplanted into the botanic garden at La Plata, became hermaphrodite the next year, and the year following returned again to the female condition. The experiment was repeated the following year, with the same results, but it is hardly clear just what factor or factors were here concerned, and it is certain that transplanting generally has no such effects.

That the sex of seed plants can be changed by environmental conditions is, however, further shown beyond the possibility of question by the case of the anther smut (*Ustilago violacea*) which infects the campion (*Lychnis dioica* L.). Here the fungus, when present in the female plants, regularly leads to the development of stamens and the suppression of the pistil. The capacity to develop stamens must, in this case, be assumed to be present in the female plant, and the fungus is able to induce the conditions necessary to their formation and the suppression of the pistils, and thus provide for the development of its own spores. Elaborate experimental attempts by Strasburger to duplicate on uninfected plants the effects produced by the parasite led, however, to no results.

In the absence of positive data as to sex-determining factors in plants it may be well to note briefly some of the more conspicuous facts as to sex differentiation with which the student of reproduction and heredity in plants is confronted.

In plants at least sex-determining factors are to be sharply distinguished from factors which lead to sexual as contrasted with asexual reproduction. We must be careful, in discussing the factors which may determine sex, to distinguish two questions: first, as to the causes which lead to the occurrence of sexual cell fusion as contrasted with asexual spore formation, and second, as to the cause or causes of the differences which exist between the egg and antherozoid cells which fuse and of the secondary sexual characters of the individuals which produce these cells. This distinction has not always been clear in the work of those who have studied the so-called cyclic alternation of parthenogenesis and sexual reproduction in animals.

The interesting results obtained by Klebs and others on a wide variety of fungi and algæ show conclusively that a very high degree of plasticity characterizes these plants as to the time of occurrence and relative predominance of asexual spore formation and sexual reproduction by cell copulations. Environmental stimuli and conditions of all sorts may determine whether the plant reproduces itself sexually or asexually or merely continues vegetative growth. We have in these simpler plants numerous cases like Penicillium in which sexual reproduction occurs only at very long intervals or not at all, while in many of the mildews, for example, a period of asexual spore formation is quite regularly followed by sexual reproduction. The same differences are abundantly illustrated in the algæ. It is quite possible, however, as I have elsewhere pointed out, that the very apparent influence of environmental conditions in determining whether sexual or asexual reproduction shall occur is, after all, only an accessory manifestation and that there is really a more fundamental series of cyclic changes in the organism which are primarily responsible for the result.

Functionally there can be little question that this cyclic succession of asexual spore formation followed later and under special conditions by sexual reproduction is equivalent to the alternation of parthenogenetic and sexual stages in the Aphides, etc. Asexual spores have the same advantages for the rapid spread and multiplication of the plant as parthenogenetic eggs have for the animal. In both cases environmental conditions seem to play a large part in determining which stage shall occur at all or predominate, and in these environmental conditions there are probably two sets of factors to be distinguished. First, the factors which lead to sexual as contrasted with asexual reproduction, and second, the factors which lead to the differentiation of the male and female organisms and sex cells.

It is quite clear in the case of the plants that reproduction by conjugation is by no means necessarily the fusion of visibly differentiated cells. It consists in its essential nature and in its most primitive occurrence merely in the union of two cells, whether like or unlike, in size, etc. The facts of heredity give, in general, no evidence that the gametes differ in any respect in their capacity to influence the offspring, and cytological data are in harmony with the assumption of the essential equivalence of the gametes even when their visible differentiation is most conspicuous.

In the simpler plants this functional equivalence is paralleled by an entire resemblance between the gametes in size, form, relative activity, etc. We may assume that there are inner and sexual differences between the gametes of *Sporodinia, Mesocarpus,* etc., but the assumption is based on no observable facts of structure and behavior and tends rather to mysticism than to an explanation of the phenomena of cell fusion. The changed relations of surface area and volume, and the known effects of an increase of the chromosome number on cells and nuclei may just as well be regarded as the first direct effects of conjugation, as the satisfaction of any mysterious sexual affinity due to some invisible sexual differentiation in the gametes.

The phenomenon of prepotency is also to be sharply distinguished from sexual differentiation. I do not refer to such prepotency as is characterized by plant and animal breeders as the power of certain individuals in a given cross to transmit their qualities to the offspring with the suppression of those of the other parent, but to the prepotency established by Darwin's classic experiments in cross pollination and whose correlative is the partial or complete self sterility observed in so many hermaphroditic flowers. This prepotency is a very widely spread phenomenon among plants and appears almost with the first development of gametic fusions. It is a common fact among algæ that even before there is any sexual differentiation between the gametes, the tendency to conjugation is very much stronger between swarm spores from mother cells of separated origin.

Sex differentiation itself seems to arise in entire independence of this prepotency as an expression of the universal tendency to specialization of function with differentiation of structure, on the one hand, toward increased motility with relatively reduced size, on the other, toward increased size and storage of metaplasmic materials as is so perfectly shown in the *Volvox* series and many other groups of algæ.

As to the stage in the life cycle at which sex differentiation occurs, plants show the widest possible variety. Marchal has recently shown that the spores from a single capsule in some of the diœcious mosses are of both sexes, but that the sex of each spore and of the protonema and moss plants which come from it is fixed. It is quite possible that here sex differentiation is accomplished in connection with the reduction divisions and spore formation—the same stage as that at which the differential distribution of chromosomes is described for certain insects, but far removed from egg formation and fertilization. The stage of sex differentiation in Equisetum may be the same as that in these mosses with diœcious gametophytes.

In the ferns and lycopods, however, sex differentiation is accomplished in the gametophyte. In the ferns the older and first formed regions of the prothallus produce antheridia and later on the newer growth produces archegonia. In the heterosporous fern allies Marsilia, Isoetes, etc., and in hermaphroditic seed plants, sex differentiation appears in the spore sacs and the asexual spores themselves. The gametophytes are thus unisexual and their sex is determined by the sex of the spores from which they come.

In diœcious seed plants sex differentiation apparently may occur in the egg so that the entire sporophyte is male or female for its lifetime, but the determination here is of a rather indefinite sort, since a large proportion of such diœcious plants have been reported as occasionally producing a few hermaphroditic flowers. The gametophytes in all seed plants are strictly and unchangeably unisexual.

The flowers of hermaphrodite or perfect shrubs and trees offer a most interesting case of successively repeated sex determinations in the life of the same individual. With each succeeding blooming period the stamens and pistils are differentiated anew in the flower buds out of what must be regarded as the strictly non-sexual new cell growth of the year. Such hermaphroditism, requiring the operation of sex-determining factors with each successive season, is sharply in contrast with that in animals in which the sex glands are fixed for life.

Polygamous and polygamodiœcious seed plants complicate the conditions still further, but it is sufficiently plain that no simple differential distribution of chromosomes on Mendelian principles at the period of chromosome reduction could in any way account for the conditions presented by the higher plants.

The existence of diœcious races with a widely varying percentage of the sexes is also against the assumption that sex is transmitted as a Mendelian character. Heyer found the hemp plants about Halle showed a ratio of 100 males to 114 females. Fisch at Erlangen found the ratio was 100 males to 154 females.

The conditions noted as to the stage at which sex differentiation occurs, etc., suggest at least the universal presence in plant cells of the potentiality for development in the direction of either sex when the necessary environmental conditions are given.

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SEX-DETERMINING FACTORS IN ANIMALS

There are few biological questions that appeal more directly to the human race than whether the sex of the child can be determined by the external conditions under which the parents live, or whether the conditions are internal and, therefore, beyond the power of control. This problem has been examined by the statistician, argued by philosophers, discussed by the naturalists and exploited by the quack. Theories of sex determination have flourished like weeds, and, while perennial, are apt to be like their prototypes, short lived. The history of these theories is, nevertheless, full of interest and not without significance. Even a brief survey will bring out the salient points.

Aristotle refuted the opinion of Anaxagoras that the male comes from the right side and the female from the left side of the father; that of Empedocles, who held that the temperature of the uterus influences the sex of the offspring; and that of Democritus, who suggested that the excess of the male or of the female element is the essential factor. Aristotle, in turn, held that the temperature of the germ-material determines the sex, for, he said, more males are born to young and to old parents than to those of middle life, because in youth the temperature of the body has not reached its maximum and in old age it has begun to abate. In recent years we find that one external factor above all others has been supposed to have an intimate relation to sex determination, namely, nutrition. An experiment of Landois in 1867 furnished the first erroneous evidence in favor of this view. He claimed that he could produce at will males or females of the butterfly *Vanessa* by regulating the amount of food. A similar conclusion was reached by Mrs. Treat and by Gentry in 1873. Riley, Bessels, Briggs, Andrews, Fletcher, and Kellogg and Bell have shown that no effects of this kind are produced by starving or by feeding. At most there occurs a greater mortality of the female caterpillars through starvation, leaving more males alive. The futility of the experiment is now manifest, since it has been shown that the reproductive organs of the male and female are already laid down when the caterpillar leaves the egg.

Equally inconclusive have been the experiments with the tadpole of the frog. The work of Born and of Yung has been upset by the experiments of Pflüger, Cuénot and of Richard Hertwig. The earlier observers failed to take into account the great mortality of the tadpoles kept under artificial conditions, hence a possible source of error is present in their results, and the conclusions are unsatisfactory so long as we do not know whether in the frog one sex is more susceptible than the other to unfavorable conditions. Aside from this possibility there seems to be something very peculiar about the proportions of the sexes in these amphibians.

Düsing has applied the statistical method of study to the proportion of males and females in the human race, and has reached the conclusion that the nutrition of the parents is an important factor in sex determination. His conclusion may be seriously questioned, because other statistics have given contradictory results (those of Punnett, for example), and because it is apparent that so many other external factors than food may be involved that the slight difference on which Düsing based his conclusion may be due to other conditions than nutrition. If, in reality, nutrition were a factor in sex determination in man we should expect to find, I think, a far greater disproportion of males to females in the offspring of the rich and of the poor than Düsing's statistics show. If further evidence is needed it is furnished by the recent experiments of Cuénot with rats, and of Schultze with mice. Even extreme conditions of starvation and of feeding produced no effects upon the birth rate of males to females.

If there were time we might pass in review the other external factors that have been supposed to account for sex determination. It must suffice to state that in no instance has a good case been made out for any one of them.

Our opinion in regard to the possible effects of external factors in sex determination has been influenced by our knowledge of the changes that take place in the life cycle of certain animals—changes that appear at first sight to be associated with sex, but in reality may relate to another phenomenon. In two, possibly in three, groups of animals a change from the parthenogenetic to the sexual mode of reproduction appears to be associated with changes in the environment. I may mention first the aphids, since I have studied the problem in this group. As is well known, parthenogenesis is the rule during the summer, but in the autumn the sexual forms appear. Cold does not bring about the result and it is almost certain that the change is incited by food conditions. The important fact to note is that, although an external factor causes the appearance of the males, it does so by introducing a new method of reproduction in which *both* males and sexual females appear.

A somewhat similar result has been found in Daphnia, where, also, according to the recent results of Issakowitsch, lack of food causes parthenogenetic reproduction to cease, and *both* males and sexual eggs to appear.

For the rotifer, *Hydatina senta,* Maupas has claimed that temperature regulates sex, while Nussbaum has tried to show that food is responsible for the result. Quite recently Punnett has discovered that the determining condition is not external at all, but that there are male strains and female strains, that give rise to their particular sex independently of the environment.

In the light of the evidence that we have at present it seems probable then that, in the higher animals at least, sex is determined by internal, not by external, factors. What the nature of the internal mechanism may be we do not know, but it is a curious and significant fact that in modern attempts to account for the nature of the change that takes place, the biologist finds himself trying once more to steer his course between the inevitable alternatives of preformation and epigenesis. The history of our science has shown, in fact, that preformation and epigenesis are two poles of thought between which speculation continually and necessarily vacillates.

One school, the preformationists, assumes that only the male or the female characters are carried by each egg or sperm, hence sex is preformed in the sense that its primordia are separated and come to lie in different germ cells.

The opposite school, that of epigenesis, assumes that all eggs and every sperm carry the potentialities of both sexes; there are no male and no female eggs, no male or female sperm, in the sense that each carries only one set of characters, but every germ cell is a sex-hybrid, and the fertilized egg is a double-barreled sex-hybrid. This view maintains that the sex of the embryo is determined by an internal condition that is present in the egg or sperm, which leads to the domination of one of the two possible alternatives. This is modern epigenesis as I understand it; predetermination, perhaps, but not preformation through the separation of contrasted characters. From this point of view we can imagine that sex-determination may be reached in more than one way. It may be due to conditions that are present in the sperm or in the egg, or as a result of the union of egg and sperm, for any internal, or even external, condition, that turns the balance one way or the other is a sex determinant.

It now appears probable that the problem of sex determination is to be sought in the same mechanism that accounts for alternative inheritance in general, Mendelian or otherwise. Strasburger, Bateson and Castle have pointed out the close parallel that seems to exist between the two cases. In Mendelian inheritance also we have to face the alternatives of preformation and epigenesis. The currently accepted interpretation of Mendelian inheritance is strictly one of preformation. Alternative characters are treated as entities in the germ cells that may be shuffled, but seldom get mixed. With each new deal the characters are separated, one germ cell getting one character and another the contrasted character.

If we take the opposite point of view, that of epigenetic development, the outcome, wherever alternative or contrasted characters are involved, is not due to separation, but to alternative dominance and recession, which need not give three types, but only two, if selective fertilization occurs, or, if only the egg or the sperm contains the internal factor that determines, that one or the other of the alternative sex characters shall predominate.

Which of these general points of view, preformation or epigenesis, we may think more profitable as a working hypothesis is, I believe, the question of the hour. My own preference—or prejudice, perhaps—is for the epigenetic interpretation, but the whole truth may lie somewhere between these two forms of thought that are the Scylla and Charybdis of biological speculation.

Thomas Hunt Morgan
Columbia University

SCIENTIFIC BOOKS

Abhandlungen ueber theoretische Physik. Von H. A. Lorentz. Leipsic, B. G. Teubner. 1906.

This work of Professor Lorentz is the result of a request of the publishers asking that a new edition of his articles on theoretical physics be prepared. The first part of the first volume appeared in 1906, and the complete work will consist of two volumes. The titles of the chapters in the first part of the first volume will be given below, together with some remarks indicating the nature and scope of the work.

Chapter I.—Some Considerations on the Principles of Dynamics in connection with Hertz's 'Prinzipien der Mechanik.' Starting with the principle upon which Hertz causes the whole science of dynamics to rest, viz., that a material system moves with constant velocity in a path of least curvature, Professor Lorentz investigates the question as to how far the method of Hertz is advantageous if one disregards Hertz's hypotheses concerning hidden motions and considers the system under the action of forces in the usual sense of the word.

After presenting in his own clear and concise way the fundamental notions of Hertz, the author shows how they lead to the general dynamical principles of d'Alembert, Hamilton and Jacobi, expressed in the familiar way; and concludes by discussing certain well-known propositions relating to holonomic systems.

Chapter II.—Ein allgemeiner Satz, die Bewegung einer reibenden Flüssigkeit betreffend, nebst einigen Anwendungen desselben. Proceeding from the fundamental equations of motion and the equation of continuity of a viscous incompressible fluid, the author derives a general reciprocal theorem expressing a relation between any two states of the fluid satisfying the fundamental equations of motion. This theorem is shown to be capable of very extensive application, and is shown in operation through the consideration of several problems.

Chapter III.—Ueber die Entstehung turbulenter Flüssigkeitsbewegungen und über den Einfluss dieser Bewegungen bei der Strömung durch Röhren. In this chapter is found a discussion of the motion of a viscous fluid when the velocity has reached a value which has been called by Osborne Reynolds the 'critical velocity.' After this velocity has been reached turbulent motion may arise. The general equations of motion for an incompressible viscous fluid furnish the starting point in this investigation. The question as to whether or not turbulent motion arises is referred to that of the stability or instability of the regular motion. Upon the regular motion small variations are imagined superimposed, the kinetic energy of which is found, and the time derivative of this quantity is used as a criterion of stability of the regular motion. Several particular cases which submit readily to extended discussion are then taken up, among them being the case of stationary motion in a cylindrical tube.

Chapter IV.—Les équations du movement des gaz et la propagation du son suivant la théorie cinétique des gaz. The discussion given in this chapter was apparently instigated by a criticism of Jochmann of the first paper of Clausius on the molecular theory of gases. In his criticism Jochmann remarked that it appeared difficult on the views of Clausius to account for the propagation of sound waves. Professor Lorentz points out that Jochmann's difficulty arose from his failing to note that the element of volume with which the mathematical physicist deals is not infinitesimal in the rigorous sense of the word. He then proceeds to a general discussion of the subject, following in the main the methods of Boltzmann.

Chapter V.—Ueber die Anwendung des Satzes vom Virial in der kinetischen Theorie der Gase. The equation of van der Waals, $(p+a/v^2)(v-b)=R(1+\alpha t)$, wherein p, v, t are, respectively, the pressure, volume and temperature of a gas and R, a, b are constants depending on the nature of the gas, is here the subject of discussion. This equation is derived from the so-called Principle of Virial first employed by Clausius. The principle is expressed by the equation $\Sigma(Xx+Yy+Zz)=-\Sigma\frac{1}{2}mu^2$, for the case of equilibrium, wherein X, Y, Z are the components of the total force acting on a molecule of mass m whose center of mass is given by the coordinates x, y, z, u is the velocity of the center of mass of the molecule, and the summations are extended to all the molecules.

Restricting himself to the case where the density is not too great, the author is led to the equation given by van der Waals. A similar restriction was tacitly made by van der Waals himself, who used, however, a somewhat different method in deriving his equation.

Chapter VI.—Ueber das Gleichgewicht der lebendigen Kraft unter Gasmolekülen. A criticism is made of an assumption made by Boltzmann in his treatment of the question of thermal equilibrium of multi-atomic gas molecules. Boltzmann's assumption is, that in a collision between two molecules, whose states of motion may be designated by A and B, if A' and B' are the new states assumed after impact, then, conversely, if the two molecules have the states of motion A' and B' they can after collision assume the states A and B. The author takes exception to this general assumption and proceeds to give his reasons therefor. Then follows a simple proof that in the case of mon-atomic gases the distribution of velocities given by Maxwell's law is the only possible one. Finally a discussion of the multi-atomic case is given, under a certain hypothesis, viz., that when the gas has assumed a stationary condition, for every group of molecules with a state of motion A there is another with the opposite state of motion A (—).

Chapter VII.—Ueber die Grösse von Gebieten in einer n-fachen Mannigfaltigkeit. Here is found a short mathematical discussion leading to results which are of importance in connection with their bearing on the kinetic theory of gases.

Chapter VIII.—Ueber die Entropie eines Gases.—In his treatment of the kinetic theory of gases Boltzmann was led to the consideration of a certain function, H, depending on the motion of the molecules, which in consequence of their collisions can only decrease. The author points out that for a stationary state the function $-H$ and the entropy stand in close connection. He then proceeds to show how in processes which progress infinitely slowly this function behaves in the same way as the entropy function.

Chapter IX.—Sur la théorie moléculaire des dissolutions diluées. On account of the simplicity of the laws of osmotic pressure and allied phenomena, Professor Lorentz has attempted with considerable success to derive them directly from kinetic theory without the aid of thermodynamics. The results of his investigations in this direction are the subject matter of this chapter.

Chapter X.—Bemerkungen zum Virialtheorem. This chapter is an article which appeared in the Boltzmann-Festschrift, 1904, p. 721. A method is described, very similar to the 'Virial' method of Clausius, which leads to the same results in the kinetic theory of gases. Application of the 'Virial' theorem is made to the case of the motion of an electron in the field of an electric doublet.

Chapter XI.—Ueber den zweiten Hauptsatz der Thermodynamik und dessen Beziehung zu den Molekulartheorien. This, the last chapter of the book, is by far the most extensive, comprehending the last one hundred pages. The author claims to give here nothing essentially new, apart from the method of presentation. The clearness with which the subject is treated, however, must appeal strongly to all readers.

Professor Lorentz begins at the beginning, and treats concisely, yet comprehensively, the fundamental principles of the subject, and arrives shortly at the expression of the second law through the entropy function. The discussion is limited practically throughout to the case of reversible processes. The thermodynamic relations of a general system of bodies are expressed through generalized parameters. The laws in connection with entropy, free energy and thermodynamic potential are derived, particular stress being laid upon the importance of the free energy principle in the solution of problems. Applications of this principle in the case of a perfect gas, and of liquid mixtures, are given in considerable detail; and the case of osmotic pressure is also worked out through this principle.

The general conditions for equilibrium among any number of different phases of a system are derived by the method of free energy, also by the method of thermodynamic potential. Attention is called to the importance of graphical methods in the treatment of various problems of phase relations.

Perhaps the most interesting part of this last chapter is the section dealing with problems of equilibrium by means of molecular theory. After some general remarks concerning the beautiful results obtained by treating the subject by the two methods, the thermodynamic and the molecular, the author makes the following cogent comment: "Bei dieser Sachlage kann mann, wie mir scheint, kaum daran zweifeln, dass die beiden Betrachtungsweisen, die thermodynamische und die molekulartheoretische, gleich berechtigt sind und dass wirkliche Wiedersprüche zwischen den beiden auf die Dauer nicht bestehen können; einer Meinung, in der wir durch die Tatsache bestärkt werden, dass mann wenigstens einen Teil der Folgerungen, die sich aus den thermodynamischen Sätzen ergeben, auch molekulartheoretisch, mit Hilfe gewisser Vorstellungen über den Mechanismus der Erscheinungen begrunden kann." A few examples showing the coincident results derived from the two methods are then given.

An extended discussion follows, showing how the second law of thermodynamics may be referred back to the principles of mechanics.

As is readily seen from the above brief survey, the various chapters of the book are not very intimately related; but at the same time the author in his arrangement of the subject matter has brought it into a form which approaches as near to continuity as the diverse nature of the topics treated will admit.

A. P. WILLS

SCIENTIFIC JOURNALS AND ARTICLES

The American Naturalist for February is an unusually good number. It opens with a description of 'An automatic Aerating device for Aquaria,' by Louis Murbach, which seems to be easily made and to work well. C. D. Durnford makes another interesting and (to most) convincing contribution to 'The Flying-fish Problem.' As he says, an extraordinary thing about the discussion is the unexplained power therein of the negative to quench the positive. It may also be noted that while the ordinary fish kicks about when laid on a vessel's deck that the flying-fish flaps its 'wings' with great rapidity, '*e pur si muove.*' L. B. Walton presents the first of a series of 'Contributions to Museum Technique. Cataloguing Museum Specimens.' It is greatly to be doubted if the detailed method advocated by Mr. Walton could be carried out in the average museum; it will be found practicable only where the staff is large, or the collections small. Also in a modified form (less sub-division and cross referencing) it has long been in use in various departments of the U. S. National Museum. James Murray describes 'Some South American Rotifers' including a few new species and varieties. J. S. Kingsley discusses 'Meristic Homologies in Vertebrates,' pointing out various difficulties in the way. To add another query to those propounded we would ask why there should not be an actual shifting of the pelvic girdle in long-bodied amphibians? We know that on one side the pelvis may be attached, say, to the twentieth vertebra and on the other to the twenty-first, and if half may shift why not the whole?

R. W. Shufeldt contributes an article on 'The Osteology of the Tubinares' with a scheme of classification. Oddly enough the first taxonomic character assigned, the posession of a large supraorbital, glandular fossa, is purely physiological and found in most sea fowl that dive. The gannets and cormorants which lack the supraorbital glands also lack nostrils and so have no need for them.

The American Museum Journal for February notices the unveiling of the busts of American men of science recently placed in the foyer; the 'Exhibition of the Progress of Science,' and the 'Expedition to the Desert of Fayoum, Egypt.' This region has yielded so much that American paleontologists will await with great interest the results of Professor Osborn's expedition. The number contains the table of contents for the Bulletin of the American Museum for 1906 and the lecture announcements for February and March.

The Museums Gazette of Great Britain for January has an article by Dr. A. B. Meyer on 'The Structure, Position and Illumination of Museum Cases.' As Dr. Meyer is the apostle of the iron case he naturally expresses himself in favor of that material. The last word on the subject is, however, yet to be said, and only in a few instances has any attention been paid to making cases and their contents harmonize. When the millenium comes and the wicked architect ceases from troubling and the weary curator has something to say about the construction of museums the halls will be left plain and finished when it is decided what is to go in them; then case, hall and specimens will be in accord. The balance of the number contains many notes and much information concerning museums in various parts of the world.

UNDER the title 'From Stone to Steel' the Horniman Museum, London, has issued a little handbook to the collections illustrating the ages of stone, bronze and iron. This, by H. S. Harrison, is a clear and concise statement of our knowledge of stone and bronze implements, will be most useful to curators and should be in demand by the public. It discusses the form and distribution of stone implements, their various uses; the different methods of hafting and the processes employed in their manufacture. The stone age in Europe is treated at length, with descriptions of the implements characteristic of different periods and the arts and manner of life of the men who used them. There are chapters on the Age of Copper and Bronze, The Early Iron Age and Stone and the Metals Outside Europe. Also there is a glossary of terms and a list of books and papers dealing with man's progress from stone to iron.

SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY. NORTHEASTERN SECTION

The seventy-fourth regular meeting of the Northeastern Section of the American Chemical Society was held in the Lowell Building of the Massachusetts Institute of Technology, on Friday, February 15, with President L. A. Olney in the chair. About seventy-five members were present.

Dr. David T. Day, of the United States Geological Survey at Washington, D. C., addressed the section upon 'The Conditions of Occurrence of Platinum.'

The speaker began with a reference to the remarkable increase in price of platinum—about tenfold increase within the last four or five years; this is not due to largely increased demands, but has been brought about mainly through the combination of large dealers and hoarding of the metal by merchants and others.

Platinum is always obtained by placer mining, and deposits of value are known to exist in the United States of Colombia, near Choco Bay, but the climatic conditions, and complications caused by government restrictions, render it impracticable to expect an immediate development of the deposits. Platinum was discovered on the Pacific coast of the United States at Pillar Point in 1850, but being much more difficult to recover from the sands, than is gold, in ordinary placer mining, development of these deposits was slow. Deposits have also been located in California in Ute County, along the Trinity River, at Monterey, and on the beach at Santa Barbara; beach deposits occur on the coast of Oregon and Washington; in British Columbia along the Tulameen River, is a valuable deposit well situated for placer mining.

Platinum occurs as an arsenide, sperrylite in many sulphide ores, such as those at Sudbury, Ontario. It also occurs native with gold, magnetite, chromite, serpentine and other minerals in the black sands; here it is in extremely fine grains, as a rule, but the character of the deposit can be easily distinguished by the microscopical appearance of the grains. The modern methods of concentrating the sands have now made a sure supply and no real famine of platinum exists; but no good substitute for it has yet been found in the connections of the incandescent light bulbs, nor, indeed, in any other industry. The present supply of the metal is probably 100,000 ounces per year, and the probable future demand is estimated at 200,000 ounces per year. If worked systematically, the known placer deposits could now supply 175,000 ounces without drawing on the sulphide or arsenide deposits, and it seems unlikely that these ores will be worked until the placer deposits are exhausted. Several valuable by-products are now being thrown away after the gold is taken out of the black sand; the magnetite content probably averages twelve per cent. and this is capable of yielding excellent iron and steel by smelting in the electric furnace.

The lecture was illustrated with lantern slides. In the discussion it was brought out that the value of the platinum in the black sands ranged from ten to fifty cents per ton. Professor Robert H. Richards contributed to the discussion a description of the black sands and the method of concentrating and collecting the fine platinum. The ordinary fire assay is practically useless where the value runs less than twenty cents per ton. But with the Wilfley table and magnetic separators, followed by an amalgamation process with mercury containing considerable sodium, the platinum and gold can be recovered. On removing the sodium by treatment with water, the platinum is practically all thrown down, leaving the gold behind in the amalgam. The cost of the recovery depends on the costs of transportation, excavation and handling the raw sand. The total cost will probably average at least fifteen cents per ton of sand.

A rising vote of thanks was accorded to the speaker, and the meeting adjourned.

FRANK H. THORP,
Secretary

THE ST. LOUIS CHEMICAL SOCIETY

AT the regular meeting of the society on February 11, Mr. R. S. Sherwin presented a paper entitled 'The Analysis of Fluorides, especially in regard to Calcium Fluoride.' The speaker dwelt chiefly on the difficulties connected with the determination of the fluorine, and the methods of overcoming the difficulties under various conditions. Mr. E. J. Ericson opened the discussion. He dealt chiefly with the determination of silica contained in fluorides.

C. J. BORGMEYER,
Corresponding Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 186th meeting of the society, on January 23, Mr. M. R. Campbell described informally some peculiar fine striations, resembling glacial striæ, on one of the bedding planes in the Portage flags in a quarry at Watkins, New York. The striæ are almost exactly parallel and absolutely straight for ten or fifteen feet, as exposed in the quarry. The surface of the rock shows the development of slaty cleavage normal to the direction of the striæ. The appearance of the striations indicates that they are the result of movement, either of a glacier or of the overlying rock mass. In the vicinity of Watkins the hills rise 300 or 400 feet high, and consequently this particular bed of rock is overlain by an average thickness of 200 or 300 feet of strata. The objection to the hypothesis of rock movement is that in places there are cross striæ at an angle of 20° or more from the principal lines. Seemingly this could not have been produced by movement in the rock mass, and the only other explanation that seems to apply is glacial movement, which, if true, would mean that a glacier invaded this region in late Devonian time. This hardly seems possible, and specimens of the striated rock were exhibited in the hope that suggestions would be offered regarding their mode of origin.

Mr. Fred E. Wright exhibited an interesting aggregate of artificial copper and silver crystals prepared in the geophysical laboratory and formed in the upper part of a steel bomb lined with a silver-plated copper tube. Into the bomb 25 c.c. of water, 1 gr. ammonium chloride and 1 gram of tremolite had been introduced and the whole heated in an electric resistance furnace at 500° C. (465–540) for three and one half days—the water being above the critical temperature and under a pressure of considerably over 200 atmospheres. The solution attacked the silver-copper tube near its base and redeposited the copper and silver in separate crystals in the upper and cooler part of the tube. In the lower, hotter part of the tube, particles of copper silver alloy and not separate crystals were observed. The inference was drawn that the aggregates of native copper and silver of the Lake Superior region, which are not alloyed, were formed at temperatures below 400°; a well-established fact deduced from abundant geologic evidence.

Regular Program

Artificial Magnesian-Pyroxenes and Amphiboles: Mr. FRED E. WRIGHT.

The results of an extended series of experiments on these minerals performed by Messrs. E. T. Allen, J. K. Clement and the speaker, in the geophysical laboratory of the Carnegie Institution of Washington, were described briefly, and obvious geologic conclusions deduced therefrom. Four forms of magnesium-metasilicate were prepared artificially, and found to correspond respectively to monoclinic pyroxene, to enstatite and to monoclinic and orthorhombic magnesian amphiboles. The monoclinic pyroxene was formed in a number of ways, and although it closely resembles other pyroxenes, both optically and crystallographically, it is easily recognized in the thin section by its polysynthetic twinning after the orthopinacoid and by the normal symmetric position of its plane of optic axes. Measurable crystals of the other forms were not produced, and their identification was accomplished by determining their physical properties—optical characteristics, specific gravity, hardness, etc.

The conditions of formation of each of these four forms were described, and attention called to the stability of the monoclinic pyroxene form at all temperatures—the other forms bearing monotropic relations to this form. Above 1,250° both artificial and natural enstatite change into the monoclinic pyroxene with evolution of heat. The stable form melts at 1,521°. The experiments show that temperature and viscosity are factors of prime importance in the formation of unstable bodies. Thus, from melts or from silicate solutions, the stable monoclinic form of magnesium metasilicate crystallizes at the highest temperature, enstatite next, and the amphiboles probably lowest of all. From thin solutions the stable form is obtained at still lower temperatures, 800° to 1,000°, while from aqueous solutions at 375° to 475° an amphibole results. The intergrowths of enstatite with the monoclinic pyroxene, and of the two amphiboles, which were obtained in close resemblance to those of nature, are cases of false equilibrium, and their occurrence establishes the fact that it can not be assumed that all rocks or mineral aggregates are systems in true equilibrium. The study of the enstatite from the Bishopville meteorite indicates that it must have cooled very rapidly from a high initial temperature, and there is evidence that the same is true of other meteorites. The conditions of formation of the four forms of magnesium metasilicate are in accord with the mode of occurrence of the natural minerals of similar composition.

Local Glaciation in Maine: Mr. Frederick G. Clapp.

In various parts of Maine there are evidences of local valley glaciers of post-Wisconsin or late Wisconsin age. These are most pronounced in the vicinity of Mt. Katahdin. Several valleys in the northern slopes of that mountain are shown by the position of moraines to have been occupied by glaciers, and more extensive evidences are found throughout the region southeast of the mountain. The indications consist chiefly in the existence of numerous moraines formed entirely of large granite boulders of the character of granite found only on Mount Katahdin. As the direction of glacial striæ in northeastern Maine is directly south, and as the granite moraines are found both south and southeast of the mountain, the material is shown to have been deposited by glaciers of post-Wisconsin or late Wisconsin age moving outward and south or southeastward from Mount Katahdin as a local center. Further indications are furnished by the depth of weathering in some of the more sandy moraines situated farther from the mountain. The glaciers extended at least twenty miles from the mountain. References were cited of descriptions of similar local glaciers in the White Mountains of New Hampshire, the Green Mountains of Vermont and the Katskill Mountains of New York.

The Late History of the Lower Colorado River: Mr. W. T. Lee.

The paper dealt mainly with the geographic and geologic features of the Colorado Valley from the mouth of the Grand Canyon to the Gulf of California. It was shown that the data, principally physiographic, warranted a subdivision of Tertiary and Quaternary time into epochs, the sequence of which is apparently clear, as announced by the speaker in the *Bulletin of the Geological Society of America* for 1906. Briefly stated, these epochs are as follows, given in order from youngest to oldest.

1. Sedimentation—formation of flood plains.
2. Erosion—during which the river cut through the Chemehuevis gravels.
3. Sedimentation—accumulation of about 700 feet of sands and gravels which extend from the Grand Canyon to the Gulf of California and are known as Chemehuevis gravels.
4. Erosion—diversion of the river from its former course in Detrital-Sacramento Valley to its present course west of the Black Range, and the cutting of Boulder, Black, Mohave and Aubrey canyons to a depth of 2,000 feet or more.
5. Sedimentation—wide-spread aggradation over the basin region. Deposition by the

river of 2,000 feet or more of sand and gravel, known as Temple Bar conglomerate.

6. Profound faulting and uplift of the plateau and erosion of Grand Canyon.

7. Erosion—during which moderately mature valleys were formed, the most conspicuous in western Arizona being Detrital-Sacramento Valley, presumably formed by the ancient Colorado.

8. Eruption of 3,000 feet or more of rhyolite and andesite.

9. Erosion—formation of Mohave peneplain.

10. Uplift and eruption of older andesite.

11. Erosion.

After a statement of the evidence upon which the subdivision is based, it was shown that the epochs might be correlated with those of neighboring regions. The more recent ones, as given above, were compared with those of Lake Bonneville and the older ones compared with the epochs of erosion, uplift and volcanic activity of the plateaus. The marine sediments of the Pacific Coast, while far from the region described, are divisible by unconformities and changes in fauna into formations representing epochs of uplift and volcanic activity alternating with epochs of quiescence similar to those of the Colorado River region, and may, upon further study, prove valuable for purposes of exact correlation.

In summing up the results of his studies, the speaker emphasizes the fact that in the Lower Colorado region, where fossiliferous strata are absent, so far as known, the sequence of Tertiary and Quaternary events can be established from physiographic evidence; that the epochs established on this basis are comparable with those of neighboring regions; and that certain lines of evidence, especially the one relating to mid-Tertiary peneplaination, give promise of definite correlation.

RALPH ARNOLD,
Secretary

DISCUSSION AND CORRESPONDENCE

FAKES AND THE PRESS

ON January 24 a paragraph, starting apparently from St. Louis, was disseminated throughout the country by the various press associations stating that a sudden diminution in the flow of oil in the wells of south Texas and Louisiana had taken place immediately after the earthquake at Jamaica, accompanied by a corresponding increase in the flow of the wells of northern Texas and Louisiana. Investigation has shown that this statement is a so-called 'fake,' namely, a lie perpetrated either as a joke or for the purpose of affecting business transactions in oil and land. The geologists whom the present writer consulted assured him at the beginning that the statement could not possibly be true, but he thought it worth while to endeavor to trace the matter back to its source. Of course the author is anonymous and unknown. Very few items in the daily newspapers appear as based upon the statements of responsible persons who are willing to vouch for their correctness. A newspaper is essentially a collection of the gossip and hasty impressions that have occurred during the day, set off in skilful headlines by the managing editor. The readers must therefore accept every statement with a grain of allowance. 'Newspaper science' has come to be a byword of reproach, and we have on several occasions in the last twenty years exposed fake tornadoes, meteors, lightning and grossly exaggerated earthquakes. In some cases like that of the present instance the newspaper report, by misleading investors, has a certain money value to the community, that is to say it can cause a loss, but no gain. It is analogous to a libel, but it is not clearly provided for by any law. It is a grave question whether Congress can not by some legal enactment check the publication of all items that convey erroneous impressions relative to matters in which the whole community is interested. The community has a right to protect itself from every species of crime. The law is made for the community, as well as for the individual. Can not some of our legal friends devise a law that will check the publication of fakes or condemn the fakist to the insane asylum, as being a joker dangerous to the community?

C. A.

GASTROLITHS

In a recent article entitled 'Gastroliths'[1] Dr. G. R. Wieland described some highly polished quartz pebbles, that I had shown to him, stating that they had been obtained in Colorado. This is an error. The pebbles came from the same locality as those obtained by him from Mr. Speers.[2] They were found in the Jurassic or Morrison formation of Montana near the Big Horn Mountains and were near, but not associated with, stegosauroid dinosaur bones. It is noteworthy that some of these jasper pebbles were dug from the clay in this polished condition, showing conclusively that they had been polished either before or during deposition. Others had been uncovered by erosion. Associated with the polished stones were many of less brilliant colors that were unpolished.

Mr. R. P. Whitfield informs me that he has seen quartz pebbles as highly polished as these at Spirit or Devil's Lake near Baraboo, Wis., which had been polished by the action of the wind. However, in a collection of wind-polished stones from New Jersey, preserved in the American Museum, all show faces and parallel angles that have been determined by the direction of the wind and position of the pebbles at different times. Some of these specimens are highly polished, but in no case showing the luster of the Montana specimens.

In the contents of chickens' gizzards I have found that pieces of glass subjected to its action for some time invariably have the edges rounded, while the faces are etched, entirely lacking the former polish. On the other hand, hard quartz pebbles found within the body cavity of a Moa show polish and considerable luster.

The unusually high polish of the Montana pebbles does not seem satisfactorily explained, either by the action of the wind or pressure of the clays. But, notwithstanding their proximity to scattered bones, there does not seem sufficient evidence to assume that these stones had been swallowed by dinosaurs as were the stomach stones of Plesiosaurs.[3] There is, however, an example nearly as well established for the herbivorous dinosaur Claosaurus of the Laramie formation.

In 1900, while collecting fossils in Weston County, Wyoming, which is a continuation of the Converse County beds, I found a Claosaur skeleton imbedded in a hard concretionary sandstone. In chipping off the surplus stone three rounded well-worn pebbles were found near the fore legs, embedded in the same matrix. These specimens were preserved and the occurrence made note of at once, for similar stones had not been seen anywhere in the deposit. These pebbles are rounded and vary in size, the largest measuring nearly three inches across. They resemble those found with plesiosaur remains and are polished to about the same degree.

It would be interesting to know what per cent. of acid is contained in the stomach of such birds as the Ostrich and Rhea.

Barnum Brown
American Museum of Natural History,
February 1, 1907

[1] Science, N. S., Vol. XXV., No. 628, pp. 66–67, January 11, 1907.

[2] *Ibid.*, N. S., Vol. XXIII., No. 595, pp. 819–821, May 25, 1906.

[3] *Ibid.*, N. S., Vol. XIX., No. 501, pp. 184–185, August 5, 1904.

SPECIAL ARTICLES

RECONNOISANCE OF A RECENTLY DISCOVERED QUATERNARY CAVE DEPOSIT NEAR AUBURN, CALIFORNIA

It was recently my good fortune to be sent by Professor J. C. Merriam to investigate a cave which had been brought to his notice through Dr. J. C. Hawver, of Auburn, California. Professor Merriam has since visited the cave and has kindly turned his notes over to me. In recognition of Dr. Hawver's vigorous prosecution of the work of cave exploration in this region we have named the cavern Hawver Cave in his honor.

Hawver Cave is situated about three miles due east of Auburn, Eldorado County, California. It is in one of several lenses of limestone in the Calaveras formation of that region. The trend of the lenses is north and south and the fissures in the limestone extend in the same direction. The entrance of the cave is on the top of the knoll a little south of the Middle Fork of the American River. Its elevation above sea level is 1,300 feet and its elevation above the river at this place is about 650 to 700 feet.

A perpendicular crevice gives access to the first part of the cave. The opening is partly filled with angular limestone fragments and red dirt to within about eight feet of the top. From the entrance the slopes extend down in a southerly direction for approximately forty feet. At this point two irregular, narrow openings give access to a well-like grotto twelve feet deep. From this grotto a small circular hole leads to the main portion of the cave. To reach this a rope is fastened in the grotto and lowered through the circular opening to a depth of twenty-two feet. The rope drops vertically, hanging free from the walls of the inner cave, and the lower end is immediately over a small subterranean lake. Near the end of the rope is a narrow tunnel about a foot above the water. This extends south for about six or eight feet, where another pool is encountered. Here a raft, consisting of an air mattress, is called into service, and paddling across the water for thirty feet a landing can be made on a mud-covered bank. From the south edge of the water and running in a southerly direction for approximately fifty feet is a tortuous series of narrow passages leading into grottos of varying dimensions; some of them are very large. These grottos and narrow passageways appear to be the extensions of an open fissure that has been widened by water and weathering. At one time the fissure was loosely filled with pieces of limestone and dirt, and the water running through has cemented it with a deposit of lime, forming a breccia which covers most of the irregularities of the fissure walls, and fills in small openings and grottos where the conditions are favorable. The distribution of the breccia indicates that the former filling of the present open spaces has sunk or been washed to a lower level now covered by water.

The cave is evidently at present in an active stage of growth, as there is a perceptible current in the water. The work of carving its walls and openings by taking the lime in solution and carrying it away is now going on. It differs in this respect from the Shasta caves which have been explored, in that the latter have attained their growth and the Quaternary openings have been sealed many years. The surface conditions above Hawver Cave by sinks and numerous small openings show where the old fissure was.

Since the recent removal from the cave of the fossil specimens now in the University Museum the water has risen to such a height that it is impossible to enter the grottos containing the bones. This rise in the water level has taken place since the last rains and demonstrates that the presence of pools in the cave is due to seepage and rivulets from the high ground above. It also shows how the fissure contents have been loosened by leaching at times of high water and allowed to sink to a lower level.

Fossil remains are numerous, considering the small space they occupy. The bones are imbedded in the breccia lining the walls and filling the small openings. High up on the roof blocks of stone have lodged and choked the fissure, and tumbled among the stones are limb bones and other skeletal elements of different animals, wedged in and cemented by a film of stalagmite. The bones are in a perfect state of preservation. In some specimens infiltration has taken place.

The remains have every appearance of being gradually accumulated in the fissure by falling and washing in from the surface, probably in part through the agency of rivulets. As has been noted from investigation in some of our northern caves, the animals probably used hollows or large crevices in the rocks as retreats and places in which to eat their prey. Numerous bones were accumulated in this manner, and were in an excellent situation to be entombed in the crevices of the limestone.

But few bones have been removed so far, though enough are known to indicate the age of the deposit and give promise of a rich fauna.

The most conspicuous of the remains are some excellently preserved *Megalonyx* bones; these consist of vertebræ, limb bones and a tooth. Also the remains of a cougar (*Felis hippolestes ?*) and of a horse (probably *Equus* *occidentalis*). Many rodent remains are present, notably those of *Aplodontia*.

The fauna, so far as known, differs from that of the Shasta caves in the absence of the peculiar goats, *Euceratherium* and *Preptoceras*, and of the deer. As far as our knowledge goes at present, the split bones so numerous in the northern caverns are relatively scarce in Hawver Cave. A fuller collection will throw more light on this point, and will give us a better knowledge of the relation of this fauna to that of other caves in this state.

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BERKELEY, CAL.

CURRENT NOTES ON LAND FORMS

CHANGES OF LEVEL IN YAKUTAT BAY

THE deformed shorelines of the Yakutat Bay region in Alaska as described by R. S. Tarr and L. Martin in 'Recent Changes of Level in the Yakutat Bay Region' (*Bull. Geol. Soc. Amer.*, XVII., 1906, 29–64) reveal recent and extraordinarily rapid and great changes in land level. The region is one whose general features were already known through the studies of Russell (1890) and Gilbert (1899), both of whom include mention of the precipitous shores in their general descriptions.

In September, 1899, three months after the Harriman expedition, of which Gilbert was a member, a series of violent earthquakes occurred in Alaska. The shocks are now found to have been associated with displacements that produced uplifts of from seven to ten feet on the southeast and of from forty to forty-seven feet on the northwest side of Yakutat Bay. The uplifts seem to have occurred within a little over two weeks and mainly on a single day, September 10, 1899. There were movements in other parts of the region besides Yakutat Bay, but to a less extent, and in some cases there was depression instead of elevation.

The physiographic effects of uplift are clearly preserved in the form of elevated beaches with fans and deltas of moderate size in bays, and of elevated narrow rock benches with sea caves and chasms cut in the headlands. Several new reefs and islands have appeared in consequence of the change of level. In some localities the elevated beaches are as clearly preserved as if they were merely exposed at low tide; elsewhere they have been partly dissected, the degree of preservation varying with height above present tide, position with respect to drainage from the land, and effectiveness of present wave attack. At almost every stream mouth there is an elevated fan or delta, its front nipped away by wave action after uplift and its top dissected by the now intrenched stream. In some cases the frontal nipping has been checked by new deltas built seaward from the new shoreline. The amount of land gained from the sea is very small in consequence of the former steep submarine slope imposed by previous glacial erosion upon mountain sides that may have been initially steep from faulting. It is evident that even the pre-earthquake stage of shore-line development was very little advanced, so small was the modification of land form along the line of sea action: the post-earthquake development is perceptible only on loose material; the rock slopes do not yet appear to be cut at the new water level.

I. B.

THE TIAN SHAN PLATEAU

RECENT explorations of the Tian Shan Mountains of north-central Asia have shown that a considerable part of their area consists of highlands of moderate relief, locally known as Syrt, at an altitude of 3,000 or 4,000 meters, above which various mountains rise and beneath which numerous valleys are deeply entrenched. The rocks of the highland are for the most part granites and other crystallines or deformed Paleozoic strata, across which the highland surface passes indifferently; but Tertiary deposits occur here and there, and the higher areas bear signs of glaciation. One of the first explorers to give an appreciative account of the plateau-like highlands was M. Friederichsen, now of Göttingen, who in 1902 accompanied Saposhnikof, botanist of Tomsk, into the district west of Khan Tengri, the great dominating summit of the region, and southeast of (Lake) Issik-kul, where the Syrt is well developed. In his 'Forschungsreise in den zentralen Tiën-schan und Dsungarischen Ala-tau' (a work of 311 pages constituting volume XX., *Mitt. Geogr. Gesellsch. Hamburg,* 1904; see pp. 87, 91, 121), he describes several gently rolling, grass-covered highlands, the flatness of which surprised him; but he gives no explicit explanation of their origin other than referring them to an 'apparently long period of continental development' (p. 157) without specifying the conditions which subsequently brought about their deep dissection. Similar surfaces of degradation, now slanting at such inclination

FIG. 1. The Ishigart range in the Tian Shan, from photograph by Friederichsen (*Pet. Mitt.*, 1906, Heft III.).

as to be sharply dissected by narrow valleys, are described (pp. 198, 206) by the same author in various mountains—or blocks, as one might say—of the Ala-tau, the northern outlying members of the Tian Shan system, but again without explicitly explaining why the earlier process of even degradation had been followed by sharp dissection (p. 216). In another article ('Beiträge zur Morphologie des centralen Tiën-Schan,' Verh. XIV., Deut. Geogr'tages, 1903, 35–42) Friedrichsen briefly considers the possibility of dislocation of the degraded surfaces, preliminary to their dissection.

In 1903, E. Huntington crossed the Tian Shan from Issik-kul to Kashgar, and described the highlands as parts of an uplifted and partly dissected peneplain ('A Geologic and Physiographic Reconnaissance in Central Turkestan,' in Pumpelly's 'Explorations in Turkestan,' Carnegie Inst., pub. No. 26, 1905, 159–216; see pp. 167, 171), an opinion in which the undersigned fully concurred ('A Journey Across Turkestan,' *Ibid.,* 23–119; see p. 73; also 'A flat-topped range in the Tian Shan,' Appalachia, X., 1904, 277–284). Parts of the same peneplain were believed to be recognizable in the even back-slope of several isolated ranges near Issik-kul, which were explained as tilted fault-blocks; and again farther north in the still low-lying Siberian steppe, degraded to small relief on crystalline and deformed stratified rocks in the neighborhood of Semipalatinsk.

W. M. D.

MERZBACHER'S TIAN SHAN EXPEDITION

A MUCH more detailed exploration of the Tian Shan, especially around Khan Tengri, was carried on by Merzbacher's expedition in 1902–3, of which the fuller reports are now publishing; but neither in his preliminary report (*Pet. Mitt. Erganz'hft,* 149, 1904; also 'The Central Tian Shan Mountains,' London and New York, 1905) nor in a recent descriptive article ('Der Tian-Schan oder das Himmelsgebirge,' *Zft. D. und O. Alpenver.,* XXXVII., 1906, 121–151; excellent photographs) does this explorer give particular account of the Syrt, not even to the remarkable slanting table-top of Mt. Catherine (Fig. 2); his attention being chiefly directed to the grand massif of Khan Tengri and its great system of radiating glaciers. However, his geologist, H. Keidel, in a report on the scien-

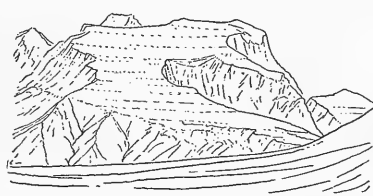

FIG. 2. Mt. Catherine (about 19,500 ft.), from photograph by Merzbacher, taken on Ak-bel Plateau (13,000 ft.), Tian Shan (The Centr. T. S. Mtns., plate opp. p. 177).

tific results of the expedition ('Geol. Übersicht über den Bau des zentralen Tian-Schan,' *Abh. k. Bayer. Akad. Wiss.,* II. Kl., XXIII., 1906, 91–192), makes it clear that the highlands of the Syrt, with their astonishing evenness of surface, are the result of Mesozoic (and early Tertiary ?) degradation of a previously deformed and folded mountain system; and that their present attitude and dissection are due to Tertiary faulting and vertical displacement, the surface margins and slope of the faulted blocks being indifferent to the general geological structure of the region.

As to the altitude of these surfaces of degradation before their sub-recent displacement, several opinions are held. Friederichsen suggests that they originated in interior basins, unrelated to the general baselevel of the ocean, and hence that their present altitude need not differ so very greatly from the altitude in which they were degraded (*Pet. Mitt.*, I., 1904, 272–273). Keidel explicitly states that it is as yet undetermined whether the present relief of the highland is due to elevation of the degraded masses or to sinking of the surrounding region. The undersigned has expressed the opinion that the even surfaces of degradation are uplifted members of a once far-extending peneplain, which bore residual mountains and mountain groups here and there; the best-finished part of the peneplain being seen still in or near its attitude of degradation in the neighborhood of Semipalatinsk, while other parts farther south have been faulted, uplifted and tilted in blocks of various areas, altitudes and attitudes ('The Bearing of Physiography upon Suess's Theories,' *Amer. Journ. Sci.*, XIX., 1905, 265–273). Hence, according to the second and third of these views, it would appear that the Tian Shan, like various other mountains, must be withdrawn from the class of forms whose present altitude, in relation to their surroundings, is due to crustal compression.

W. M. D.

THE SYSTEMATIC STUDY OF MOUNTAINS

The systematic study of mountain forms in accordance with the scheme of the physiographic cycle has presented difficulties, because it has so seldom been possible to reconstruct with any fair degree of success the forms initiated by deformation, on which the sequential forms are then to be developed in due order by the processes of erosion. Substantial relief from this difficulty is promised in all those cases, now increasing in number yearly, in which mountains are shown to be not in the original cycle initiated by disorderly compression and folding, but in a later cycle, initiated by uplift or by relatively simple block faulting, after more or less advanced peneplanation in a previous cycle. The systematic treatment of such mountains is relatively easy; for if they are not too much dissected the essential features of their initial forms may be easily determined and apprehended; and their sequential forms are in the main equally within the reach of explanatory description. If to this be added the recent increase in the understanding of high-mountain sculpture by glaciers, first clearly generalized by Richter and later presented in much detail for the Alps by Penck and Brückner, the possibility of developing a systematic method of mountain description does not seem so remote as it did a few years ago.

W. M. D.

SCIENTIFIC NOTES AND NEWS

Colonel W. C. Gorgas, chief sanitary officer of the Isthmian Canal Commission, has been appointed by President Roosevelt a member of the commission.

Professor Simon Newcomb has been elected honorary fellow of the Physical Society of London.

The steam yacht *Virginia*, on which Mr. Alexander Agassiz was making explorations in the West Indies, struck a submerged wreck on February 11, and was obliged to go to San Juan under sail, where it was placed in dry dock.

Professor T. W. Richards, of Harvard University, will sail for Germany on March 9, in accordance with the arrangements for an exchange of professors between Harvard University and the University of Berlin. Professor Richards will lecture on 'The Fundamental Constants of Physical Chemistry.' A laboratory has been fitted up for Professor Richards by Dr. Arthur Staehler, who last year worked with him at Harvard University.

MR. F. DARWIN has been nominated to represent Cambridge University at the celebration of the two hundredth anniversary of the birth of Linnæus, to be held at Upsala in May.

MR. H. F. NEWALL, of Trinity College, Cambridge, assistant director of the observatory, has been elected president of the Royal Astronomical Society, in succession to Mr. W. H. Maw.

THE Carnegie Institution has renewed its grant of $2,000 to Dr. A. A. Noyes, professor of physical chemistry at the Massachusetts Institute of Technology.

AT the celebration of Washington's Birthday at Lehigh University, the honorary degree of doctor of science was conferred on Arthur Arton Hamerschlag, director of the Carnegie Technical Schools of Pittsburg, Pa. The orator of the day was the Hon. W. U. Hensel, of Lancaster, Pa., ex-attorney general of Pennsylvania.

PROFESSOR G. CANTOR, of Halle, has been made an honorary member of the Russian Mathematical Society.

PROFESSOR H. M'LEOD, F.R.S., director of the Royal Society's catalogue of scientific papers, will receive the honorary degree of LL.D. from the University of St. Andrews on April 2.

THE administration of the new president of Brazil, Dr. Affonso August Moreira Penna, who was inaugurated on the fifteenth of November last, promises to open a new era for science in that country. For the portfolio of Industry, Highways and Public Works, he has called Dr. Miguel Calmon du Pin e Almeida, formerly secretary of agriculture of the state of Bahia where he fully justified his reputation as one of the most able and far-sighted of the younger generation of Brazilian engineers and administrators. One of the first acts of Dr. Calmon was to initiate a general geological investigation of the country under the direction of the well-known geologist Mr. Orville A. Derby who organized and for many years directed the geographical and geological survey of the state of Saò Paulo. Mr. Derby will be assisted by such native and foreign resident geologists as have made valuable contributions to the geology of Brazil. The new department is denominated *Serviço Geologico e Mineralogico do Brazil* and will be located in Rio de Janeiro. The leading feature of its program is the rapid reconnaissance of the general geology of the country with detailed investigation of the districts that on account of their mineral wealth, deficiencies of water supply or other reasons offer special scientific and economic interest for investigation. Owing to the lack of proper topographic maps, no systematic map work will for the present be attempted.

MR. W. R. BUTTENSHAW, a scientific assistant in the imperial department of agriculture for the British West Indies, has been appointed botanist in the agricultural department of India.

THE tenth lecture in the Harvey Society course will be given at the New York Academy of Medicine on March 8, at 8:30 P.M., by Dr. Friedrich Müller, professor of medicine at the University of Munich, Germany, on 'Neuroses of the Heart.' This is the last lecture of the present year's series. All interested are invited to be present.

PROFESSOR ANDREW C. LAWSON, of the University of California, presented an address before the Washington Academy of Sciences on January 31, 1907, on 'The Dominant Tectonic Lines of California.' It was discussed by Drs. G. F. Becker, Waldemar Lindgren and Ralph Arnold. Dr. Charles B. Davenport, director of the Laboratory for Experimental Evolution of the Carnegie Institution of Washington, Cold Spring Harbor, L. I., addressed the Washington Academy of Sciences on February 26, 1907, on 'Heredity and Mendel's Law.' The address was illustrated by lantern slides and was discussed by Professors O. F. Cook and W. J. Spillman.

PROFESSOR CHARLES PALACHE, of Harvard University, delivered in February a course of six lectures in the Geological Department of the University of Wisconsin on the subject, 'Recent Developments in the Study of Crystals.'

At a meeting of the American Ethnological Society at the American Museum of Natural History, on March 4, a public lecture was given by Dr. George A. Dorsey, of Chicago, on 'The Human Sacrifice Ceremony of the Pawnee.'

Mr. Frank M. Chapman, of the American Museum of Natural History, has given a course of lectures on 'Bird-lore' at Wellesley College.

The fourth of the series of public lectures on 'Problems of Insanity' was delivered at the Academy of Medicine, New York City, on March 2. Subject: 'The Development of the Legal Regulations concerning the Insane, with Suggestions for Reforms,' by Dr. Allan McLane Hamilton.

Professor F. R. Hutton, head of the department of mechanical engineering of Columbia University, will deliver the principal address at the anniversary exercises of the Clarkson School of Technology at Pottsdam, N. Y., in commemoration of the charter day, on March 19.

A meeting in memory of the late William Wells Newell, secretary of the American Folk-lore Society, will be held at the first parish church, Cambridge, on the afternoon of March 10. Among the speakers will be Colonel Thomas Wentworth Higginson, Professor Toy, of Harvard University, and Professor Boas, of Columbia University.

Professor Wilhelm von Bezold, director of the Royal Prussian Meteorological Institute, died on February 17 in his seventieth year.

The deaths are announced of Professor Nicholas Menschutkin, professor of chemistry at St. Petersburg; of Dr. Johann Rajewski, associate professor of mathematics at Lemberg, and of Dr. K. Harz, professor of botany at the Veterinary School at Munich.

The position of resident naturalist at the Station of the Marine Biological Association of San Diego will be vacant after June 1, 1907. A student well advanced toward the doctor's degree should, provided he has had considerable experience in marine biology, be competent for the place. Inquiries may be addressed to Professor Wm. E. Ritter, University of California, Berkeley, Calif.

The Treasury Department announces that an examination will be held at Washington on April 15 to examine candidates for assistant surgeonships in the U. S. Public Health and Marine-Hospital Service. Candidates must be between twenty-two and thirty years of age. The salary is $1,600, with quarters provided. On April 29 there will be an examination for assistant surgeons in the army, there being at present twenty-five vacancies.

There will be civil service examinations on March 13 and 14 for the position of engineering and hydrographic aid, at salaries ranging from $720 to $1,200 per annum, in the Reclamation Service and Geological Survey. It is probable that from eight to ten hydrographic aids will be required for service under the Geological Survey. From the eligible list secondary positions in the engineering and hydrographic corps will be filled, with a salary of $900 and upward. On the same day there will be an examination to fill during the coming season twenty to twenty-five vacancies in the position of topographic aid in the Geological Survey, and vacancies as they may occur in any branch of the service requiring similar qualifications. The salary of this position is $900 per annum.

Dr. W. Jarvis Barlow has presented to the Clinical Association of Los Angeles, for the use of the medical profession, a library building, the cost of which was $32,000, apart from the ground.

A laboratory for oceanographical research, organized through the efforts of Mr. W. S. Bruce, leader of the Scottish Antarctic Expedition, was formally opened at Edinburgh by the Prince of Monaco during a visit to the city in January. The laboratory is installed in a portion of the Surgeons' Hall.

The British Medical Journal states that preparations for the next International Medical Congress, which is to be held at Buda-Pesth in 1909, are already in active progress.

It has been decided that there shall be twenty sections, namely, anatomy, physiology, general pathology, therapeutics, internal medicine, surgery, obstetric medicine, ophthalmology, children's diseases, neurology, psychological medicine, dermatology, urology, laryngology, otology, stomatology, hygiene, forensic medicine, military hygiene, and industrial hygiene.

A COURSE of nine lectures upon science and travel has been arranged by the Field Museum of Natural History, Chicago, for Saturday afternoons in March and April, at three o'clock. These lectures will be illustrated by stereopticon views and will be given in Fullerton Hall, Art Institution, Michigan Avenue and Adams Street. The lectures are as follows:

March 2—'The Iron Ores of the Minnesota Ranges,' Professor C. W. Hall, University of Minnesota.

March 9—'Scientific Notes on the Russian Convict Island of Sakhalin,' Mr. Charles H. Hawes, M.A., University of Cambridge, England.

March 16—'Bird Husbandry,' Dr. N. Dearborn, Assistant Curator, Division of Ornithology, Field Museum of Natural History.

March 23—'The Blackfoot Indians,' Dr. Clark Wissler, Curator of Anthropology, American Museum of Natural History.

March 30—'A Superposed Vegetation—The Plant Rusts,' Professor J. C. Arthur, Purdue University, Lafayette, Indiana.

April 6—'The Monuments of a Prehistoric Race,' Mr. Frederick I. Monsen, New York.

April 13—'The Indiana of Nature—Its Evolution,' Professor W. S. Blatchley, State Geologist, Indiana.

April 20—'How some Insects have solved the Problem of Life,' Professor Herbert Osborn, Ohio State University.

April 27—'Physiography and Life in Western Norway,' Professor Mark S. W. Jefferson, State Normal College, Ypsilanti, Michigan.

QUOTING from the *Beilage zur allgemeinen Zeitung* for January 30, *Nature* states that an expedition is in course of being organized, under the auspices of the Royal Academy of Sciences of Berlin and the government of the Dutch East Indies, for the purpose of exploring Java in search of further remains of Pithecanthropus. The originator of the idea appears to be Frau Professor Selenka, widow of the late Professor Selenka, of Munich, who has already traveled extensively in Borneo for the purpose of collecting embryos of the orangutan. Dr. Elbert is attached to the expedition as geologist, whilst Dr. Maskowski, of Berlin, goes out as zoologist. A Dutch engineer, Mr. Oppenoorth, will have charge of the surveying and excavating operations. The Pleistocene volcanic breccia from which the original remains of Pithecanthropus were obtained by Professor Dubois at Trinil is believed to have a wide extent in the mountains of Java, reaching in some places to a height of 100 meters or more above sea-level, and it is proposed to examine this stratum thoroughly in a number of the more promising localities.

WE learn from *The American Museum Journal* that the museum has acquired by purchase a collection from the Samoan Islands. Among other things this contains a complete outfit for the manufacture of bark or 'tapa' cloth. In the manufacture of this cloth, single strips of bark from a species of mulberry are prepared by scraping and soaking in water, after which they are beaten out very thin by means of small wooden clubs. These thin sheets, while still wet, are laid one over another and the whole beaten together to form a large sheet of uniform thickness. Such bark cloth is in some respects a kind of paper, but it is serviceable as cloth since it is not easily damaged by water. The finished cloth is often ornamented by printing, or rather rubbing. For this purpose designs in relief are carved on wood or built up of palm-leaf cuttings, upon which the cloth is laid and rubbed with sticks of coloring matter, like crayon. This leaves an impression of the raised portion of the carving similar to that produced when a school-boy rubs the impression of a coin into the fly-leaf of his book. Aside from the tapa outfit, the collection contains several handsome pieces of finished cloth and a number of costumes, household utensils and other implements. It is proposed to install this collection together with other material in the museum as a special exhibit from one of the colonial possessions of the United States.

UNIVERSITY AND EDUCATIONAL NEWS

By the will of the late Mrs. Peter Redpath, of the Manor House, Chiselhurst, England, McGill University will receive $150,000 for the Peter Redpath Museum and the Peter Redpath Library. Of this sum $120,000 is for the library and $30,000 for the museum. The library will receive in addition the books of the Manor House Library. Both the library and the museum were the gift of Mr. Redpath.

It is announced that Mrs. Russell Sage has given an additional $50,000 for the school at Sag Harbor.

Yale University has received a bequest of $10,000 from Mr. John J. Abernethy for the establishment of a fellowship.

The New York *Evening Post* states that in a few weeks ground will be broken at Homewood, the new site of Johns Hopkins University, for the botanical gardens and laboratory, while the clearing away of the ground set apart for the houses of the faculty is being pushed rapidly. It is expected that the botanical gardens and laboratory will be in use by the close of the year.

The new building for the College of Physicians and Surgeons, San Francisco, was formally dedicated on January 18. Dr. Winslow Anderson, president of the institution, delivered the address.

New buildings are being erected for the Schools of Mines and Metallurgy of the Pennsylvania State College, of which Professor Wadsworth is dean. These buildings are to be thoroughly equipped and when finished will cover a ground space of some twenty-one thousand square feet.

According to the *British Medical Journal* a meeting of professors of the University of Warsaw, now closed, was recently held under the presidency of Professor Amalizki with the object of selecting a place for the establishment of a new Russian university. The choice appeared to be between Saratow and Woronesch. The former city was said to be prepared to contribute a million roubles, while the latter offered 950,000 roubles.

At the annual midwinter commencement of the University of Nebraska, held on the thirty-eighth anniversary of its charter day on February 15 thirty-four students were graduated. Nineteen of these were bachelors of arts, thirteen bachelors of science and two bachelors of law.

The summer school of New York University will offer a number of courses in advanced chemistry under Professor Arthur B. Lamb, director of the Havemeyer Laboratory, and Professor Martin A. Rosanoff. These courses will include a lecture course in physical chemistry, a laboratory course in physical chemistry, a laboratory course in electrochemistry, a laboratory course in organic synthesis, and a laboratory course in organic analysis. In addition to these courses the usual work in general chemistry, qualitative analysis and quantitative analysis will be offered.

General James A. Beaver, ex-governor of Pennsylvania, judge of the Superior Court and president of the board of trustees of the Pennsylvania State College, has been elected president of that institution. Dr. Judson P. Welsh, who was principal of the Bloomsburg State Normal School for many years, has been elected vice-president and business manager.

A chair of industrial chemistry has been established at the University of Kansas to be occupied by Professor Robert Duncan.

Dr. Shepherd Ivory Franz, psychologist to the Government Hospital for the Insane, has been appointed professor of experimental psychology in the George Washington University.

At the College of the City of New York George M. Hayes, of St. John's College, has been appointed tutor in mathematics, and the following tutors have been promoted to instructorships: Dr. Coffin, physics; Dr. Gutman, chemistry; Dr. Cohen, mathematics.

Professor Thomas Loveday, professor of philosophy at the South African College, Cape Town, has been appointed librarian to the University of Sheffield.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MARCH 15, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE NEW YORK MEETING OF SECTION C OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AND THE THIRTY-FIFTH GENERAL MEETING OF THE AMERICAN CHEMICAL SOCIETY—I.

THE meetings were held in the Havemeyer Chemical Laboratory of Columbia University. On Thursday morning, December 27, Section C was organized with Vice-President Clifford Richardson in the chair.

Dr. H. P. Talbot was elected councilor, Dr. J. H. Long, member of the general committee; Dr. Leonard P. Kinnicutt, member of the sectional committee, and Dr. C. E. Waters, press secretary.

Immediately following this, the opening meeting of the American Chemical Society was held with President W. F. Hillebrand in the chair. Two addresses were delivered: 'The Advisability of Restricting the Use of the Ionic Theory in Teaching Qualitative Analysis,' by H. L. Wells, and 'The Chemist and the Community,' by A. D. Little.

In the discussion which followed the second address reference was made to the establishment of the Perkin Chemical

Library, an account of which has already been given in these pages.

At the afternoon session C. F. Mabery, the retiring chairman of Section C, delivered an address on 'The Education of the Professional Chemist.' On Friday morning four of the sections of the Chemical Society held their meetings, at which the following papers were read:

INORGANIC CHEMISTRY

H. L. Wells, Chairman

The Solubility of Lime in Solutions of Nitric Acid: F. K. CAMERON and W. O. ROBINSON.

The complete solubility curves at 25° were given for that part of the system in which the solutions have an alkaline reaction. It was found that there was a curve for solutions in contact with calcium hydroxide as solid phase; a curve for solutions in contact with a series of solid solutions containing lime and nitric acid as solid phase; a curve for solutions where the solid phase had the composition $CaO.N_2O_5.3\frac{1}{2}H_2O$, and a curve when $Ca(NO_3)_2.4H_2O$ was the solid phase. The basic nitrate appeared to be identical with one formerly noted by Werner. It is efflorescent, loses water and breaks down on exposure to the air or more rapidly by washing with alcohol.

The Behavior of Nickel Sulphide toward Hydrochloric Acid and the Qualitative Separation of Nickel and Cobalt from Manganese and Zinc: H. C. COOPER.

Beryllium Nitrate: CHARLES L. PARSONS.

Normal beryllium nitrate, although an article of commerce, has no place in literature. It can only be made by evaporating a solution of beryllium hydroxide or basic carbonate in nitric acid to a thick, gummy and basic mass and dissolving same in a moderate excess of concentrated nitric acid from which it crystallizes as $Be(NO_3)_2 \cdot 4H_2O$ on cooling. The salt loses its nitric anhydride component easily. It melts in its own water of crystallization at 60.5°. It is soluble in alcohol and acetone.

The Instability of Certain Tungstates: R. C. WELLS.

By means of the electrical conductivity of their solutions it is possible to follow the transformations which para- and octa-tungstates undergo in water. The latter salts have the formula $M_6W_8O_{27}$ with more or less water of crystallization. The transformations take place more quickly at high temperatures, requiring only about twenty-five minutes to proceed half way at 80°. Not enough data are yet at hand to settle definitely the nature of the change.

Pentavalent Bismuth: VICTOR LENHER and E. B. HUTCHINS, JR.

In making a study of the higher valences of bismuth, it has been found that the tetroxide is fairly stable and that potassium bismuthate and bismuthic acid doubtless exist in a fair degree of purity. In studying the halogen derivatives, the pentoxide on treatment with hydrochloric acid at low temperatures, the treatment of the trichloride with chlorine in a Dewar bulk at liquid-air temperatures and the treatment of the trihalides with various perhalides of cesium yielded only derivatives of the trivalent type.

The Action of Thionyl and Sulphuryl Chlorides on Selenium and Selenium Dioxide: VICTOR LENHER and H. B. NORTH.

By the interaction of selenium dioxide and thionyl chloride, by the treatment of elementary selenium with thionyl chloride and by the action of sulphuryl chloride on elementary selenium, the tetrachloride of selenium is obtained. On the other hand, selenium dioxide can be sublimed in the vapor of sulphuryl chloride without any reaction taking place.

The Atomic Weight of Hydrogen: WM. A. NOYES.

This work, now in progress at the Bureau of Standards, is in part a repetition of the method used by the author some years ago, when he obtained for the ratio H:O, the values, 15.896 (H = 1) or 1.00654 (O = 16). When corrected for the gases occluded in the copper oxide used as the source of the oxygen, these values become 15.878 or 1.00765. In the recent work the oxygen is weighed as copper oxide, the increase in weight when the hydrogen is passed into the tube is also determined and, besides, the water is weighed separately. In other determinations the hydrogen is first weighed absorbed in palladium, and its weight checked by the increase in weight of the copper oxide tube, as well as by the weight of the water alone. The mean of a number of determinations gives a value which is somewhat higher than Morley's on the oxygen basis. Other experiments are in progress in which no copper oxide is used, the oxygen being passed directly into a tube containing palladium saturated with hydrogen. In this way as much as thirty-five to forty grams of water can be formed in a single determination, thus lowering the probable error.

The Selection of the Most Probable Value for the Atomic Weight of an Element: WM. A. NOYES.

A discussion of the principles which should govern the selection of the atomic weight of an element when a number of workers have determined it in different ways or by the same method. The later determinations should usually have greater weight than the older ones, and in many cases the latter should no longer be used in calculating the mean, especially when sources of error overlooked by the earlier investigators have been avoided in the later work.

The Separation of Calcium and Magnesium: NICHOLAS KNIGHT. (By title.)

Combinations of the Sesquioxides with the Acid Molybdates: R. D. HALL.

A study was made of the compounds containing the various sesquioxides in combination with the acid molybdates with a view of determining whether they were double molybdates or whether they were derivatives of a complex inorganic acid.

The methods employed were: (1) Preparation of other salts by double decomposition, (2) preparation of the free acid, (3) dialysis of the alkali salts. All favored the considering of these salts as derivations of a complex inorganic acid. The following nickel and cobalt derivations were obtained:

$$3K_2O \cdot NiO_2 \cdot 9MoO_3 \quad \text{and} \quad 3K_2O \cdot CoO_2 \cdot 9MoO_3,$$

analogous to the complex containing MnO_2 in union with MoO_3.

The Direct Quantitative Analysis of Mixtures of Bromides with Chlorides: LAUNCELOT W. ANDREWS.

On the Density Curve of Mixtures of Bromine and Chlorine: LAUNCELOT W. ANDREWS.

The Homogeneity of Tellurium: VICTOR LENHER.

In studying the action of tellurium and tellurium dioxide toward various reagents such as sulphur monochloride, phosphorus oxychloride, antimony pentachloride, etc., in addition to the formation of tellurium tetrachloride or double chlorides, mother liquors were obtained which were carefully tested for tellurium and found by experiment to contain tellurium with an atomic weight of 127.5. Fractional precipitation by means of ferrous salts from the chloride solution gave the figure 127.55. By the slow solution of tellurium in hydrochloric acid, in presence of air, a fraction was obtained, which gave 127.5 as the atomic weight of tellurium. Three series of atomic weight determinations of tellurium from Bohemia, Cripple Creek and from western copper ores were made. The double bromide of tellurium and potassium was heated in chlorine gas and the potassium chloride weighed. All of the tellurium from these various sources showed an atomic weight of 127.5 and no evidence of lack of homogeneity of tellurium has ever appeared to the author.

An Improved Method for the Qualitative Analysis of the Tin Group, including the Detection of some of the Rarer Elements: Arthur A. Noyes and Wm. C. Bray.

ORGANIC CHEMISTRY

A. S. Wheeler, Chairman

The Nitration of N-Substituted Anilines: J. Bishop Tingle and F. C. Blanck.

A continuation of the work already reported (*Amer. Chem. Jour.*, **36**, 305). The additional results obtained appear to warrant the statement of the following provisional rule: *In the nitration of N-substituted aniline derivatives, by means of a mixture of nitric and some other acid, the position of the first entering nitro group depends on two influences, viz., the nature of this N-substituting group and the strength of the acid, other than nitric, which is present, and these two factors may be so selected as to neutralize or reinforce each other.*

The Nitration of Benzene: J. Bishop Tingle and F. C. Blanck.

An investigation of the formation of dinitrobenzene in the presence of sulphuric acid and also of non-acidic or feebly acidic dehydrating agents. The results indicate that the formation of the dinitro derivative is preceded by that of mononitrobenzene. The use of sulphuric acid appears to give a larger yield of crude nitration products containing a lower percentage of *meta* compound and about the same percentage of mixed isomers soluble in alcohol. As nitric acid is a stronger acid than sulphuric, the result accords with the 'rule' deduced from the nitration experiments with N-substituted anilines and indicates that the rule is of general application.

Acyl Derivatives of Ortho- and Paraminophenols: J. Bishop Tingle and L. F. Williams.

An easy method is described for the preparation of ortho- and paraminophenol by which good yields are obtained. Many new acyl derivatives of these compounds are described. The results of the work may be summarized by saying that, of the thirty derivatives of this type now known, thirteen have been obtained for the first time by the authors, three have been prepared by newer and simpler methods, the melting point of one other has been corrected and the existence of two of the remainder has been shown to be extremely doubtful.

Condensation of 1, 3-Diketones with Secondary Amines: J. Bishop Tingle and L. F. Williams.

A number of condensation compounds of camphoroxalic acid and secondary amines have been prepared. Their existence tells strongly in favor of the enolic formula for camphoroxalic acid. The amines employed were acetylphenylhydrazine, dibenzylamine, diamylamine and diisoamylamine; similar products from dimethylamine and diethylamine have been previously obtained by the senior author, and, as a class, the substances are believed to be new. Condensation compounds, belonging to various types, of the above acid with the following primary amines have also been obtained: ethylamine, orthoaminophenol and paranitraniline. Camphoroxalic acid and triethylamine yield a salt.

The Relative Value of Calcium, Sodium, Sodamide and Sodium Ethylate in Promoting the Claisen Reaction: J. BISHOP TINGLE and ERNEST E. GORSLINE.

The experiments were carried out with camphor and various esters employing the condensing agents mentioned above. The effects of temperature, solvent and of certain other factors were studied. The best agent is sodium wire; calcium and sodamide are about equal in condensing power, but are decidedly inferior to sodium; sodium ethylate is the least active condensing material. The effect of the solvent is very marked, it is independent of the temperature and the question is raised as to the possibility of its playing a definite chemical part in the condensation, similar, perhaps, to its rôle in the formation of the Grignard reagent.

Preparation of Aniline Derivatives of Succinic and Phthalic Acids: J. BISHOP TINGLE and MARSHALL P. CRAM.

The current methods for the preparation of succinanil and of succinanilic acid have been somewhat modified. Succinanilide has been prepared by a new and simple method. A modified process for the preparation of phthalanil is described and also some experiments regarding the preparation and relative stability of phthalanilic acid and of phthalanilide.

Hydrazones of Aromatic Hydroxyketones; Alkali-Insoluble Phenols: HENRY A. TORREY and H. B. KIPPER.

It was found that many aromatic hydroxyketones, such as, pæonol, resodiacetophenone, dibenzohydroquinol and dibenzoresorcinol gave stable phenylhydrazones. These hydrazones, though from their method of preparation they would appear to have one or more free hydroxyl groups, are, nevertheless, insoluble in aqueous alkalies. It is possible that the insolubility of these compounds may be due to their having a quinoid structure, and that no hydroxyl group is actually present.

Quinazolinecarboxylic Acids: M. T. BOGERT, J. D. WIGGIN and J. E. SINCLAIR.

The authors converted 3-amino-1,4-xylene and 4-amino-1,3-xylene, into their acetyl derivatives, oxidized the latter to the corresponding phthalic acids, changed these aminophthalic acids into their acetanthranils, and prepared quinazolinecarboxylic acids from the latter by condensation with various primary amines.

Isomeric Oxygen and Nitrogen Ethers in the Quinazoline Group: M. T. BOGERT and H. A. SEIL.

The authors prepared various quinazoline ethers of the type -OR and -NR by alkylation of the hydroxyquinazolines and by other methods. They find that methyl iodide invariably gives the -NR compound, while ethyl iodide generally yields the oxygen ether.

4-Aminophthalic Acid and Some of its Derivatives: M. T. BOGERT and R. R. RENSHAW.

A description of the methods of preparation and properties of 4-aminophthalic acid and the following derivatives: the hydrochloride, a number of metallic salts, the anhydride, imide, monomethylaminoimide, the 4-monoamide of trimellitic acid, the dimethyl succinimido- and dimethyl phthalimidophthalate, and the 3,4-dimethyl ester of diphthalylurea.

Synthesis of 7-Nitroquinazolines from 4-Nitroorthotoluidine: M. T. BOGERT and W. KLABER.

4-nitroacetanthranil was condensed with various primary amines, both aromatic and aliphatic, thus producing the 7-nitro-2-methyl-4-ketodihydroquinazoline derivatives. The amines employed included amino-esters, nitriles and diamines.

The Synthesis of Naphthotetrazines from p-Diaminoterephthalic Acid and Related Compounds: M. T. Bogert and J. M. Nelson.

The diphenyluraminodiacetyl derivatives of *p*-diaminoterephthalic diethyl ester, and the corresponding diacetdianthranil were prepared. These were condensed with ammonia, and some of the aromatic and aliphatic primary amines, and the corresponding naphthotetrazines obtained.

Diphenylbrommethylamine: F. J. Moore.

The Basic Properties of Carbon: J. F. Norris.

The fact was brought out that certain derivatives of methyl alcohol show the properties of weak bases. Triphenylcarbinol is converted by concentrated aqueous solutions of hydrochloric, hydrobromic, and hydriodic acids into the corresponding salts. Tertiary butyl alcohol also shows this property. Tritolycarbinol is changed into tritolymethyl chloride by dilute hydrochloric acid, and readily forms a nitrate with nitric acid. The ethers of these carbinols are converted into the chlorides of the substituted methyl radicals by aqueous hydrochloric acid, and the ethers can be formed by the action of a few drops of dilute hydrochloric acid on an alcoholic solution of the carbinols.

INDUSTRIAL CHEMISTRY

A. D. Little, Chairman

When is Time of More Value than Refinement of Method? Mrs. Ellen H. Richards.

The purpose of the investigation was to ascertain the practicability of shortening and intensifying quantitative methods of investigation in sanitary and industrial chemistry so that they could be intelligently used by high school pupils. The results were promising enough to be extended by other laboratory workers, and the purpose of the paper was to urge more attention to ways of interesting the public in order that research may be more fully supported by non-scientific business men and state and city officials.

A Rapid Method for the Determination of Calcium in Water and its Significance in Connection with the Analysis of Water for Boiler Purposes: F. E. Hale.

Air Elutriation of Fine Powders: Allerton S. Cushman.

In order to study the influences of fineness of the particles on the rate of decomposition of rock powders when acted on by water, it has been necessary to devise an air separator for use in the laboratory. By means of a simple apparatus in which both blast and vacuum are employed, it has been possible to easily separate powders that have passed the finest meshed sieves obtainable into four or five graded sizes. A 200-mesh rock powder invariably contains a certain proportion of particles that may be called ultimately fine, having reached the mechanical limits of possible subdivision. If these smallest particles can be separated interesting investigations can be made on the influence of the maximum surface area for unit weight, when the powders are acted on by water.

The Relation between the Ultimate Composition and the Physical Properties of Portland Cement: Richard K. Meade.

The Available Hydrogen of Coal: S. W. Parr.

The fundamental proposition is based on the idea that the original substance from which coal has resulted was made up of substantially the same sort of material and the 'products of decomposition represent a progression which at any given stage in any case bear a constant likeness of composition at the same stage in another case.' By selecting, therefore, a proper unit of comparison a curve may be developed which shall show at any point of reference the amount of available hydrogen present. Tabulations of results from 180 coals widely distributed throughout the United States show an average as to accuracy approximately one third better than the results as derived from ultimate analysis.

The Testing of Copper and its By-products in American Refineries: GEORGE L. HEATH.

A proper analytical control of refining is attained by the determination of the quality of all supplies and fluxes, and by mechanical, electrical and chemical tests of the metal itself. The growing severity of commercial requirements in regard to the purity and electrical conductivity of a metal, whose properties are sensibly affected by less than 0.002 per cent. of some foreign elements, makes the knowledge and control of its impurities a problem of the highest importance for the present and future, to both producer and consumer.

A condensed account was given of the latest methods of analysis, and assaying in use at large smelting and custom laboratories, giving preference to those considered most accurate.

A determination may be required of one, or all, of the following elements; copper, gold, silver, oxygen, sulphur, selenium, tellurium, arsenic, antimony, tin, lead, iron, zinc, cobalt, nickel and manganese, and appropriate methods of analysis are indicated, in the original paper, for each natural group of foreign constituents. A short discussion is also given of the technical determination of the electrical resistance of copper, mentioning some causes of lack of agreement in commercial tests.

The Osage Orange: a Dye Wood: A. S. WHEELER and STROUD JORDAN.

The bright yellow coloring matter is extracted with alcohol, giving a beautiful green-red fluorescent solution. Textile fabrics are readily dyed. The dye is not a good indicator. Its constitution is under investigation.

The Determination of Arsenic and other Solid Constituents of Smelter Smoke, with a Study of the Effects of High Stacks and Large Condensing Flues: WM. D. HARKINS.

The amounts of materials of industrial value given out in the smoke are often enormous. The analysis of the smoke of one smelter shows an approximate daily output in the smoke of fifty-five thousand pounds of arsenic trioxide, three to four million pounds of sulphur dioxide, three hundred thousand pounds of sulphur trioxide, six thousand pounds of zinc, five thousand of copper, six thousand of lead, five thousand of antimony, etc. High stacks throw the smoke to a greater distance from the smelter than low stacks. Captain Taylor's pilot tube is the best instrument for velocity determinations.

Gases vs. Solids: an Investigation of the Injurious Ingredients of Smelter Smoke: W. C. EBAUGH.

The results of experiments upon alfalfa and sugar beet plants with (*a*) gaseous sulphur dioxide, (*b*) aqueous solutions of sulphur dioxide, and (*c*) 'flue dust' or fume in various forms, show that heretofore the amount of injury done by sulphur dioxide has been unduly emphasized, and that that due to the solid emanations of the smelters has been ignored. The effects of the investigations will be to modify the means adopted by smelting plants to eliminate the dangerous substances from their smoke.

Experiments on the Manufacture of Chlorates and Hypochlorites with a View to High Current Efficiency: A. G. BETTS and R. H. SHERRY.

A series of experiments was made using

different cathodes and also with the addition of various substances to the solutions. It was found that magnesium used as cathode gave a high current efficiency. Comparative results gave the current efficiency in a solution of potassium chloride to which sodium chromate had been added as 69.3 per cent., and in a potassium chloride solution with a magnesium cathode as 65 per cent. On a continuous run the efficiency of this cathode in making chlorate was 90 per cent. the first hour and 60 per cent. from the fourth through the eighth hour. In making hypochlorite with a 12.5 per cent. salt solution the efficiency was 96 per cent. for the first hour and 75 per cent. for the ninth hour. The loss of magnesium was very small. It was found that from one third to one half the salt could be converted and a current yield of from 60 per cent. to 70 per cent. could be obtained. In addition to this advantage, the power cost is low. The hypochlorite solution is quite clear and ready for use after dilution.

A Plea for the Systematic Study of American Gas Coals: H. B. HARROP.

Standard fields like the 'Pittsburg' are now working down to poor grades, while many areas now believed to be unsuitable may yet be proved satisfactory. Knowledge of location, extent and character of various coal deposits capable of supplying gas works and by-product oven plants is very meager both among gas engineers and commercial chemical engineers. Qualities of coal required for gas works and ovens discussed: gas yield, character of gas, impurities, quality of coke, tendency of ash to 'clinker,' and suitable carbonizing treatment. The author discusses, in the light of his own experiments, the merits and failings of the crucible volatile matter test and the practicability of a standard apparatus for determining the best carbonizing temperature, gas yield and character of gas. A committee is recommended to cooperate with the Coal Mapping Subcommittee of the American Gas Institute.

Viscosity and Lubrication: CHAS. F. MABERY and J. H. MATHEWS.

This paper presented results on the chemical composition, viscosity and durability tests of lubricating oils, manufactured from different petroleums, and the composition and viscosity of the principal constituents of commercial lubricating oils. The hydrocarbons examined and separated from the crude petroleum from different fields, as well as from the lubricating oils themselves, were of the series of hydrocarbons, C_nH_{2n+2}, C_nH_{2n}, C_nH_{2n-2}, C_nH_{2n-4}. The viscosity of these series varies regularly from the series C_nH_{2n+2}, which is very low, to the series C_nH_{2n-4}, which has a very high viscosity corresponding to the high viscosity of lubricating oils made from the heavy petroleum that is composed largely of the series, C_nH_{2n}, C_nH_{2n-2}, C_nH_{2n-4}. The durability tests corresponded to the variation in viscosity. Viscosity was determined in the apparatus of Ostwald, and calculated from his formula

$$n = n_0(td/t_0d_0).$$

Viscosity of these products was also determined by the Saybolt universal viscosimeter. The durability tests were made on a bearing under 500 revolutions carrying a load of 500 pounds, with a regular addition of the oil at the rate of six drops per minute, during the first two hours. During the tests readings of temperature were taken at intervals of five minutes and the friction was also determined from readings of weights on a balance taken at the same intervals. The coefficient of friction was calculated from the formula $F = WL/R$ where $W =$ dif. in wt. lbs.; $L =$ length of lever; $R =$ radius of axle. A comparison of various lubricating oils showed that viscosity alone can not be relied on as an accurate means for determining their commercial value in lubrication. This work is being extended, including the question as to the comparative value of straight hydrocarbon oils and compounded oils and any harmful effects of the latter due to corrosion.

Suspensions in Dilute Alkaline Solutions: W. R. WHITNEY and ALONZO STRAW.

The Annealing of Sterling Silver: W. H. WALKER.

The sterling silver of commerce is an alloy of 92.5 per cent. silver and 7.5 per cent. copper. The frequent annealing incident to the manufacture of silver-ware produces, when the article is polished, a black reflex or sheen called by the workmen 'fire surface.' This can be removed only by dissolving in nitric acid that portion of the ware affected. A study of this matter shows that, notwithstanding the literature on the subject to the contrary, the difficulty is caused by the formation of copper oxide in the alloy, and that it may be prevented by annealing under such conditions as will practically eliminate oxygen. A method based on the above observations is now in practical use in a number of establishments.

Fullers' Earth and its Application to the Bleaching of Oils: C. L. PARSONS.

A paper on the American occurrence, the properties and absorptive qualities of fullers' earth.

The Modern Canning Industry: Mr. MORRIS.

Denatured Alcohol and the New Law: C. E. MUNROE.

Rosin Size of High Free-Rosin Content: EDWARD F. MOODY.

Discussed the rosin sizing of paper, with particular reference to mill-made sizes, composed of completely saponified rosin, as compared with the more recent developments in high free-rosin sizes. The sizing effects of the former were shown to be due to the precipitated aluminum resinate, which, being somewhat soluble in water, is not a perfect water-proofing agent in paper; whereas, in the latter case, the insoluble rosin itself is the sizing agent, producing the desired results more economically. Also the causes of froth, imperfect sizing and other matters of interest to the paper industry were discussed.

Agricultural and Sanitary Chemistry—L. L. Van Slyke, chairman.

Food Adulterations: C. B. COCHRAN. (By title.)

India Beeswax: J. F. GEISLER.

The Efficiency of Certain Phosphates as Influenced by Liming and by the Variety of the Plant: H. J. WHEELER and G. E. ADAMS.

Field experiments with nine different phosphates have been in progress at the Rhode Island Agricultural Experiment Station since 1894.

In one series lime has been applied in small quantities and in the other it has been omitted. Many different varieties of plants have been employed. The results show marked benefit from liming in connection with superphosphates, and in a less degree with floats, bone meal, and basic slag meal. The soil is naturally practically devoid of carbonate of lime and when moistened, quickly and intensely reddens blue litmus paper. No doubt liming in excess would have the opposite effect on superphosphates. The cabbage and some other crops were practically a total failure where floats were used, yet certain other varieties of plants made much better use of them.

The most remarkable feature was the wonderful effect of liming in increasing the efficiency of the roasted Redondite (iron and aluminum phosphate). This material contains a total of 45 to 50 per cent. phosphorus pentoxid, of which 38 to 40 per cent. is 'reverted'; also 2.5 to 3.5 per cent. ferric oxid and 45 to 50 per cent. aluminum oxid. Liming was of no benefit and acted injuriously in connection with the finely ground unroasted material. The efficiency of the phosphates is influenced by the soil, the other manures, and by the variety of plant in a most marked degree, hence the danger of drawing conclusions from insufficient data is great.

The Functions and Agricultural Value of Certain Sodium Salts: H. J. Wheeler, B. L. Hartwell and F. R. Pember.

The investigation of this question has been in progress at the Rhode Island Agricultural Experiment Station since 1894.

Plants have been grown in the field during twelve years by the use of sodium carbonate and sodium chloride, by the corresponding potassium salts and by various mixtures of the sodium and potassium salts. Evidence was afforded of a liberation of potassium from the soil by the sodium salts. When the supply of potassium was limited sodium salts greatly increased the yields of the mangel wurzel, turnip, radish and certain other root crops; at the same time the percentage of phosphorus in the dry matter was usually increased. This increase was greater in the case of the carbonates than with the chlorids, however it seemed to be an incidental accompaniment of, rather than the cause of, the increased yield. Sodium did not appear to have been of special benefit because of its serving as a better carrier of nitrogen to the plant than potassium, neither was the benefit explainable by virtue of liberating lime or magnesia nor on account of changes in the relation of the two, removed by the plant.

Sand culture and water culture were both resorted to in order to eliminate factors arising in the field. The results thus far secured by water culture show benefit from sodium in the presence of small supplies of potassium, which is not obtained in the same degree when a like osmotic pressure is provided by the employment of salts of calcium. Sodium seems to enable a small supply of potassium to produce a larger crop by virtue of performing some part of the functions which potassium may perform if present in sufficient quantities. Sodium used in connection with potassium sometimes prevents plants from taking so great an excess of potassium from the soil as they would otherwise remove.

The Action of Acids on Casein when no Solution takes Place: L. L. Van Slyke and D. D. Van Slyke.

When casein is suspended in a solution of acid, the casein takes acid from the solution, the casein remaining solid under proper conditions of dilution of acid, temperature, etc. The amount of acid thus taken up by casein varies with the concentration of the acid, the temperature, the time of contact and the kind of acid. The phenomena do not indicate simple chemical combination of acid and casein, but correspond closely to the conditions described by van Bemmeln as applying to what he calls absorption compounds.

The Accuracy of Phenolphthalein as an Indicator in Determining the Acidity of Casein: L. L. Van Slyke and D. D. Van Slyke.

In determining the acidity of casein by titration with alkali, different indicators give different results. With phenolphthalein higher results are given than with litmus or methyl orange. The results with phenolphthalein have been chosen arbitrarily as being most accurate. Measurement of the conductivity of the solution was used. When a given amount of alkali solution is neutralized by addition of casein, the solution decreases in conductivity until the neutral point is reached, after which further additions of casein do not change the conductivity. The results thus obtained agree quite closely with those given by phenolphthalein.

The Sanitary Analysis of Water: L. P. KINNICUTT.

The Organic Components of the Soil in their Relation to some Properties of Roots: OSWALD SCHREINER and HOWARD S. REED.

The authors pointed out the toxic properties of soil extracts from unproductive soils and the effect of absorbing agents, organic substances and simple treatments, such as dilution with water, boiling, etc., in improving the conditions for plant growth. The oxidizing power of plant roots in connection with these toxic conditions, and the effect of fertilizers on these oxidizing properties were emphasized, as was also the production of toxic conditions by previous root growth.

Comparative Intensities of Colors used in Confectionery: EDWARD GUDEMAN.

Method of obtaining strength of colors without comparison with standard color, based on height of column through which diffused light will pass. Description of apparatus. Details and results withheld on account of other investigations on colors now being made.

The Changes in the Fat of Bread during Baking, and Methods for its Determination: J. S. CHAMBERLAIN.

The Unification of Reducing Sugar Methods: PERCY H. WALKER.

The Ripening of Strawberries, Raspberries, Blackberries, Gooseberries and Currants: W. D. BIGELOW and H. C. GORE.

The Preparation and Composition of Vinegar from Kieffer Pears: H. C. GORE.

The Preparation of Unfermented Apple Juice: H. C. GORE.

Absorption by Soils: F. K. CAMERON and H. E. PATTEN.

Various types of distribution curves and velocity equations were considered, and illustrated by examples from the literature and the authors' own experiments with dyestuffs, fertilizer salts and liquid manures. The causes for variations which soils present, from the ideal types, were pointed out, and the magnitude of absorption effects was shown to be much greater than is usually supposed.

A Comparison of Methods for the Determination of the Alkalinity of Ash: HERMANN C. LYTHGOE.

Varying results on alkalinity of ash are reported by different chemists owing to the use of different indicators. The results reported in the following table were obtained on one sample each of vinegar, lime juice and raspberry syrup. The difference in the results is due to the presence of phosphates.

ALKALINITY OF ASH. C.C. OF N/10 ACID REQUIRED TO NEUTRALIZE THE ASH OF 100 GRAMS OF SAMPLE.

Character of Sample	Indicator			
	Phenol-phthalein	Methyl Orange	Cochineal	Litmus
Vinegar	33.1	40.9	37.9	24.9
Lime juice	15.8	18.8	17.5	12.2
Raspberry syrup	15.5	20.3	17.1	11.0

The Possible Use of Corn Oil as an Adulterant of Lard and its Detection: WM. MCPHERSON and WARREN A. RUTH. (By title.)

The Influence of Climate on the Composition of Wheat: J. A. LECLERC.

The Work of the Association of Official Agricultural Chemists on the Analysis of Tanning Materials: F. P. Veitch.

Soil Acidity in its Relation to Lack of Available Phosphates: A. R. Whitson and C. W. Stoddart.

Experience has shown that a direct determination of the amount of essential elements present in a soil does not show its fertility, since it does not consider the degree of availability. It is unquestionably true, however, that the processes by which these elements become available are chemical, and depend upon the conditions existing in the soil. A determination of the conditions which influence the rate at which the elements become available and which affect the accumulation of the available material would enable us to diagnose the needs of the soil more quickly and surely than by direct field and pot experiments. Investigations of the fertilizer requirements of soils during the past two or three years have shown that whenever a soil is acid it needs phosphates.

Careful tests were made on nineteen soils, thirteen of them acid. All acid soils showed by field or plant-house tests the need of a phosphate fertilizer. Three of the soils, not acid, showed the need of phosphates, although rather slight in one case. The soils tested were mostly clays from as many different localities in Wisconsin as possible.

Although there is phosphoric acid present in these soils in sufficient quantity for many crops it is unavailable, and hence the soils need phosphate fertilizers. The soil acids probably act upon the readily available phosphates, such as the calcium phosphates, at a more rapid rate than the normal, neutral or alkaline soil moisture, and when once in solution these phosphates are readily washed out by heavy rains, or are fixed by iron and aluminium hydroxides—that is, are precipitated and rendered unavailable as insoluble iron and aluminium phosphates. When there is sufficient lime in the soil to maintain the phosphoric acid in the form of calcium phosphates, the plant is able to obtain enough phosphorus for its use, since calcium phosphate is soluble enough to supply the needs of a growing crop.

A Quick Method for the Determination of Moisture in Grain: E. Brown and J. W. T. Duvel.

By the use of the method and the apparatus outlined in this paper, the moisture content of any sample of grain or similar material, can be determined in from twenty to thirty minutes. The method consists primarily in heating the weighed sample of grain in about 100 c.c. of hydrocarbon oil, the moisture expelled being afterwards condensed and measured in a graduated flask reading to tenths per cent. A special apparatus is required for this method of making moisture determinations.

In the afternoon a number of the chemists were given a luncheon at the works of the Schultz Mineral Water Company, after which there was a choice of excursions to the Gas Works, the Pacific Borax Works and the New Jersey Zinc Works at Newark. The same evening there was a subscription dinner at the Waldorf Hotel.

On Saturday morning the annual meeting of the American Chemical Society was held. The secretary reported that the membership was now 3,079, a net gain of 160 over last year. There are, besides, 187 members who have been elected, but who have not yet qualified. Of the latter, 150 had applied during the last two months. New local sections have been established in Minnesota, Indiana, Louisiana and at the University of Illinois.

The report of the treasurer showed that

the finances of the society are in a satisfactory condition.

The report of the librarian was also read.

The editor reported that during 1906, 217 papers had been submitted for publication, of which 160 had been accepted without change, 25 after revision and 32 were not suitable for the *Chemical Journal*. Of these last, many were considered as better adapted for journals of a more general nature, and were rejected by the committee on publications solely for this reason. The editor also gave an account of the progress which had been made on the new journal, *Chemical Abstracts*. It is planned to make it fully as complete as the *Zentralblatt*, and two numbers will be issued each month. He urged that every member of the society try to encourage other chemists to join. To meet the cost of the new publication the dues have been raised to eight dollars, but it will not be possible to continue the publication if any considerable number of the members drop out on this account. In this connection he stated that the gratifying increase in membership during the last few months of the year was largely due to the new journal.

With a view to increasing the usefulness of *Chemical Abstracts* the council appointed a committee to consider the relationship of the society to other chemical organizations and to other chemical publications. It was also decided to add to the council two new members, both of whom were to be technical chemists.

The officers of the society for 1907 are:

President—M. T. Bogert.
Vice-Presidents—The presidents of the local sections.
Secretary and Editor—Wm. A. Noyes.
Treasurer—A. P. Hallock.
Librarian—E. G. Love.

J. H. Long, chairman of the committee on the quality of reagents, reported that the committee was in process of reorganization.

W. F. Hillebrand, chairman of the committee on uniformity of technical analysis, reported that there had been organized subcommittees on zinc ores, rosin and shellac and phosphate rock.

L. P. Kinnicutt, chairman, stated that the committee on water analysis had been reviewing and testing various methods of analysis.

J. H. Long, chairman of the committee on untaxed alcohol, requested that the committee be disbanded, as a satisfactory law had been passed by Congress.

The meeting of the Chemical Society was followed by a meeting of Section C, at which the following report and addresses were given:

G. C. Stone and W. George Waring: Report of the committee on the analysis of zinc ores. Gave the tabulated results of three series of analyses of zinc ores and said that a fourth sample would soon be sent out.

Cellulose: A. S. Wheeler.

Agricultural Chemistry in Relation to Research: L. L. Van Slyke.

At noon the meeting adjourned to the College of the City of New York, where two lectures were given:

The Electrical Industries at Niagara Falls: C. F. Chandler.

The Geology of Niagara Falls: John M. Clark.

After the addresses the chemists were entertained at luncheon in the gymnasium of the college. After this visits were paid to a brewery in the neighborhood, the Interborough Power Station and the American Museum of Natural History.

In the evening the retiring president, W. F. Hillebrand, delivered an address on 'The Present and Future of the American

Chemical Society.' This has been published in SCIENCE and in the *Journal* of the society. After the address the visiting members were given a complimentary smoker by the members of the club.

On Monday other meetings of sections were held.

PHYSICAL CHEMISTRY

Alexander Smith, Chairman

The Electrolytic Purification of Cerium: J. P. MAGNUSSON.

The Conductivity and Ionization of Salts in Aqueous Solution at High Temperatures: A. A. NOYES, W. D. COOLIDGE, A. C. MELCHER, H. C. COOPER and G. W. EASTMAN.

The Hydrolysis of Salts and the Ionization of Water from 0° to 306°: A. A. NOYES, Y. KATO, R. B. SOSMAN and C. W. KANOLT.

Photochemistry and the Phase Rule: WILDER D. BANCROFT.

Thermochemistry: JOSEPH W. RICHARDS.

Discussed the mechanism of endothermic reactions, taking as types of three common reactions the reduction of zinc oxide by carbon, the production of water gas by steam acting on carbon, and the reduction of iron oxides by carbon monoxide in the blast furnace. The analogy of these reactions to simple physical changes absorbing heat, was particularly dwelt upon.

The Final Disintegration Products of Uranium: B. B. BOLTWOOD.

The conclusion is reached that in unaltered, primary minerals from the same locality the amount of lead is proportional to the amount of uranium, and that in unaltered, primary minerals from different localities the amount of lead relative to uranium is greatest in minerals from the locality which is geologically the oldest. Hence lead seems to be the final disintegration product of uranium. The amounts of helium found in minerals containing both uranium and thorium are of about the order, and are not in excess of the quantities, to be expected from the assumption that helium is produced by the disintegration of uranium and its products only. From the composition of radioactive minerals it appears highly improbable that either lead or helium is a disintegration product of thorium.

Solution in a Dissolved Solid: CHARLES L. PARSONS.

A study of solutions of iodine in potassium iodide, of lead oxide in solution of lead acetate, of camphor in aqueous acetic acid, of iodine in aqueous acetic acid, etc. It is shown that some solids in solution form a mixed liquid which acts perfectly analogous toward some solutes, to the well-known cases of ternary mixtures where a solid or a liquid is dissolved in two mixed liquids. It was, furthermore, pointed out that many of the physical properties, generally accepted as abnormal, of ternary mixtures are in reality perfectly normal and that in many such cases polymerization is not at all necessary to qualitatively explain the supposed variation from the gas laws.

The Electromotive Force of the Oxyhydrogen Cell: G. N. LEWIS.

Measurements of free energy (the maximum work obtainable from a chemical process) not only are of great importance technically, but they also permit the chemist to predict in what direction and how far chemical reactions will progress. The free energy of many important oxidation processes can be calculated if the potential of the oxygen electrode is known, or the E.M.F. of the oxyhydrogen cell. This was determined in three ways: (1) from the decomposition pressure of silver oxide between 300° and 445°, (2) from measurements of the equilibrium in the Deacon process between 350° and 420°, and (3) from the decomposition pressure of mercuric oxide. These methods give concordant results which show that the E.M.F. of the oxyhydrogen cell is about 1.224 volts, a value which is higher by one tenth of a volt than the one hitherto accepted.

Calculation of Some Chemical Equilibria by the Nernst Formulas: K. G. FALK.

The equilibria for the reactions

$$2SO_2 + O_2 = 2SO_3, \quad H_2 + Cl_2 = 2HCl,$$
$$H_2 + Br_2 = 2HBr$$

are calculated for different temperatures by means of the formulas developed by Nernst, and which formed the subject of the Silliman lectures (delivered by Nernst) at Yale University in 1906. The calculated values are compared with those found experimentally.

Cadmium Sulphate and Mercurous Sulphate: G. A. HULETT.

The Quantitative Determination of Small Quantities of Arsenic by the Method of Marsh-Liebig as affected by Supertension and Potential Differences: WM. D. HARKINS.

The injurious action of iron and other metals in hindering the reduction of arsenic in the Marsh apparatus, can be prevented by using a temperature of 100° or by the addition of salts of tin, cadmium, lead or bismuth, using the proper conditions. The factors aiding in the reduction when a foreign metal is present or added are: (1) (most important) high superpotential; (2) distance from zinc in the scale of potentials; (3) degree of solubility of the salts formed. Two lengths of hard glass tube should be heated in two fire-brick furnaces to give complete decomposition of the arsine. The method is quantitative.

The Vapor Pressure of Aqueous Nitrate Solutions by the Air-bubbling Method: A. T. LINCOLN. (By title.)

Melting-point Determinations at Low Temperatures: LEO F. GUTTMANN.

A constantan-copper couple connected in series with a sensitive D'Arsonval galvanometer is used, and standardized by determining the known melting points of ice, chloroform, paste of solid carbon dioxide and alcohol, ether and the temperature of liquid air. From the data thus obtained a calibration curve is drawn up. To determine the melting point of a substance, the couple is immersed in the liquid contained in a glass tube, and the liquid frozen. By allowing the substance to warm up slowly, and watching the galvanometer deflections the melting point is ascertained. The melting points determined were those of some of the alkyl alcohols, esters, and iodides, the paraffins, acetaldehyde, ethyl methyl ketone, acetone, ethyl bromide and chloride, toluene, ethylbenzene and the xylenes.

The System Sodium Chloride, Sodium Sulphate, Calcium Sulphate and Water: F. K. CAMERON, J. M. BELL and W. O. ROBINSON.

The invariant points and boundaries for the system sodium chloride, sodium sulphate, calcium sulphate, and water were determined at 25° and the solid diagram constructed. It was shown that fields existed for solutions in contact with the following solid phases: NaCl, $Na_2SO_4 \cdot 10H_2O$, $2Na_2SO_4 \cdot 3CaSO_4$ (glauberite) and $CaSO_4 \cdot H_2O$. No anhydrite field exists.

The Colors of Colloidal Silver: WILDER D. BANCROFT. (By title.)

Studies in Catalysis; Amidine Formation: JULIUS STIEGLITZ.

The formation of amidines according to: $RC(:NH)OR' + NH_3 = RC(:NH)NH_2$

+ HOR′, is accelerated by the addition of an acid (HCl) or of an ammonium salt (NH_4Cl), simply because the reaction substance is not the whole imino ester but only its positive ions. A constant is obtained on the basis of the isothermal equation: $dx/dt = K \times C_{\text{pos. ester ions}} \times C_{NH_3}$, the velocity being always proportionate to the concentration of the positive ester ions, although they form only a very small but rigorously calculable part of the ester used. The results are in entire accord with the author's theory of the catalysis of esters, cane-sugar, etc., under the influence of acids.

Freezing Points of Mixtures of Sulphur and Iodine: ALEXANDER SMITH and C. M. CARSON. (By title.)

This report has been transmitted through Professor Charles L. Parsons, Secretary of Section C.

C. E. WATERS,
Press Secretary.

(*To be continued*)

THE ASSOCIATION OF AMERICAN GEOGRAPHERS

THE third annual meeting of this association was held December 31, 1906, and January 1, 1907, at the building of the American Geographical Society, in New York City. The president, Mr. Cyrus C. Adams, presided, and delivered the president's address upon the subject, 'Some Phases of Future Geographical Work in America.'

Professor I. C. Russell, member and councilor of the association, died during the year, and a memorial was read by Professor W. M. Davis. The Monday evening session was devoted to the following addresses: Mrs. Leonidas Hubbard, Jr., 'The Rapids of Labrador Rivers'; Dr. F. A. Cook, 'The Ascent of Mount McKinley'; Mr. Alfred H. Brooks, 'A Meeting with Captain Amundsen at Nome.'

In the general program the following papers were read by their authors:

William Churchill, 'Insularism, and the Nesiote Type'; A. L. Rotch, 'The Circulation and Temperature of the Atmosphere at Great Heights above the Tropical Atlantic'; Charles C. Adams, 'The Evolution of the Isle Royal Biotic Environment'; G. E. Condra, 'The Opening of the Indian Territory'; I. Bowman, 'The Deserts of Peru and Chili in South American History'; E. N. Transeau, 'The Need of Evaporation Data in Plant Geography'; William Libbey, 'Problems of the Panama Canal'; W J McGee, 'The Prospective Conquest of the Mississippi River'; Angelo Heilprin, 'Guiana and Venezuela as a Field for Geographical Exploration, with some Observations on a Recent Visit to the Essequibo Wilderness'; G. W. Littlehales, 'The Nature and Purpose of the Chart Publications of the Navy Department, and their Geographical Extent'; E. O. Hovey, 'The Isthmus of Tehuantepec'; Alfred H. Brooks, 'Railway Routes in Alaska'; H. L. Bridgeman, 'The International Polar Congress at Brussels'; E. Huntington, 'Influence of Change of Climate upon History'; R. DeC. Ward, 'The Meteorology of the North and South Polar Areas'; W. M. Davis, 'Place of Coastal Plains in Systematic Physiography'; W J McGee, 'The American Deserts and their Reclamation'; W. M. Davis, 'Geography as defined by Hettner'; F. P. Gulliver, 'The Orientation of Maps'; A. P. Brigham, 'Geography for College Entrance'; Collier Cobb, 'Hatteras Island and its Shifting Sands'; D. W. Johnson, 'The Texture of Topography'; W. M. Davis, 'The Eastern Slope of Mexico'; H. E. Merwin, 'Land Forms as Plant Controls'; Cleveland Abbe, 'A Study of Airy's Projection by Balance of Errors'; A. W. Grabau, 'Classification of Marine Life Districts.'

The annual dinner was held on Tuesday

evening at the Hotel Colonial. The next annual meeting will be held in Chicago.

Professor Angelo Heilprin is president for the year 1907. The complete list of officers has already been published in Science.

Albert Perry Brigham,
Secretary

SCIENTIFIC BOOKS

Petrogenesis. By Dr. C. Doelter. Braunschweig, Friedr. Vieweg and Son. 1906. Pp. xii + 261; 1 plate and 5 figures. 8vo. Paper. 7 M.

This is Volume XIII. of a series of monographs on natural history and mathematics entitled 'Die Wissenschaft.' In it Professor Doelter, of the University of Gratz, presents an outline of the knowledge and theories concerning rocks of all kinds. The comprehensive treatise is put in the form of fourteen chapters, the first ten of which deal with volcanic rocks, the last four with contact metamorphism, the formation of the crystalline schists, sediments, and chemical precipitates. One hundred and fifty pages are devoted to the problems of igneous rocks, fifty-two to those of the crystalline schists.

The method of treatment is to present the views of various geologists or petrographers on different topics, sometimes with comments and criticisms, sometimes without. The citations are many, but from the nature of the task, perhaps, they are not always complete as to number or entirely satisfactory as to substance in some instances. However, there is a great fund of information for the student and an abundance of valuable bibliographic references. The scope of the work is so large that it is not possible to review the book with the thoroughness it deserves without a very considerable expenditure of time and space. And it may be sufficient to point out the contents of the several chapters in a general manner.

The first is devoted to a consideration of the theories and observations concerning the interior of the earth and the problem of vulcanism. The physics of the interior of the earth is discussed and the observations of Barus and Tammann with reference to melting points and pressure are cited. The possible source of molten magmas, and the causes and mechanics of their eruption are considered, including the rôle of vapors and the temperature of lavas.

The second chapter treats of the forms of volcanic rocks as conditioned by their solidification on the surface of the earth or at some depth below it, and also the influence of vapors upon the crystallization. The discussion of laccolithic forms reveals a curious attitude toward the original definition of the term by Gilbert. In connection with the problem of intrusion the views of Kjerulf and of Michel-Lévy are presented, and those in opposition are credited chiefly to Brögger. Daly's theory is also stated.

The third chapter has to do with the structure (texture) of eruptive rocks. The porphyritic texture is discussed at length with special reference to Fouqué and Michel-Lévy's views and to those of Zirkel. A number of petrographers are cited in connection with the texture of phanerocrystalline rocks, chiefly Brögger, Lane, Teall and Vogt. Spherulitic texture receives considerable attention, the views of Rosenbusch and of others being noted. The question of the relation between age and texture is considered, and the textural and constitutional facies of rock bodies are described.

In the fourth chapter the relation between the mineral composition and the chemical composition of igneous rocks is discussed. The view of Lagorio, Vogt, Morozewicz and Iddings are commented on. The work of Osann in correlating chemical analyses of rocks is reviewed, and the methods of expressing the chemical composition of rocks by means of diagrams are described to some extent, especially those of Brögger and Becke.

The fifth chapter deals with the problem of the differentiation of magmas. Various theories are set forth briefly and commented on. The term is also applied to the crystallization of minerals from molten magmas. Experimental work on the crystallization of rock minerals in the laboratory is reviewed and Doelter's own observations on the precipitation of crystals in molten liquids are noted.

The question of the order of the eruption of different kinds of rocks at given centers of volcanic activity is treated in the sixth chapter. The diverse statements of petrographers as to the order in different regions are noted and the conclusion reached that there is no uniform order which obtains for all regions, but that the succession is highly complex.

The seventh chapter deals with inclusions in igneous rocks, their character and origin. Assimilation and corrosion are discussed in chapter eight. First the changes effected on minerals crystallized from magmas, and then the effects on rocks in contact with molten magmas. The laboratory production of rocks by experimental means is described in chapter nine, especially the researches of Fouqué and Michel-Lévy, and those of Morozewicz.

In the tenth chapter the solidification of volcanic magmas is discussed with reference to the minerals produced under different conditions, and also with respect to the order of their separation. Laws affecting the crystallization of compounds from solutions are reviewed, the question of eutectic mixtures is considered, also the effect of undercooling, the melting point of minerals, and the influence of pressure on separation. The production of tuffs and bombs is also discussed. In the eleventh chapter contact metamorphism is described and its causes and processes discussed.

The crystalline schists are treated in one chapter of fifty-two pages. Various theories as to their origin are reviewed, chiefly those based on specific researches upon special regions or particular occurrences. It will be enough to note the captions under which the subject is treated: Eruptive Gneiss, Gneiss as Altered Granite, Diagenesis, Regional Metamorphism, The Chemical Composition of the Rocks, Alteration by Means of Water, Alteration by Means of High Temperature, Injection Hypothesis, Dynamic Metamorphism, Chemical Reactions in Solids, Plasticity of Rocks, Lateral Pressure, Connection between Metamorphism and Dislocation, Law of the Change of Volume, Mineral Composition of Crystalline Schists, Structure and Texture of Schistose Rocks, Origin of Schistosity, Zones of Metamorphism, Objections to a General Application of Dynamic Metamorphism, Formation of Crystalline Schists by Contact Metamorphism, Comparison of Contact Metamorphism and Dynamic Metamorphism.

The thirteenth chapter treats of sediments, their kinds and modes of formation. The major portion of the chapter is devoted to limestone and the formation of dolomite. The last chapter contains a discussion of the production of chemical precipitates, the deposition of rock salt, gypsum and anhydrite. The works of Bischof, van't Hoff and of Ochsenius are those chiefly cited.

J. P. Iddings

The Origin and Structure of the Roxbury Conglomerate. By George Rogers Mansfield. Pp. 180, 7 pls. Cambridge, Mass., November, 1906. Bull. of the Museum of Comparative Zoology at Harvard College, Vol. XLIX. Geol. Series, Vol. VIII., No. 4.

This paper, after a rather lengthy introduction, begins with a forty-five-page chapter on the origin of conglomerates, in which are discussed the various types of conglomerate. The discussion is thorough, and the results, drawn principally from the conclusions and opinions of previous writers in different countries, are tabulated. This digest is of general interest, and is a valuable contribution to the subject.

The remainder of the work deals principally with the three Carboniferous basins of Massachusetts and Rhode Island—the Boston Basin, which includes the Roxbury conglomerate; the Norfolk Basin, and the Narragansett Basin. The text is essentially a critical review of previous publications on these areas, especially of the Boston Basin, which has been studied in great detail by Professor W. O. Crosby. Professor Crosby's work in this region has extended over a period of at least thirty years, and no less than sixteen of his publications are included in the author's bibliography. In view of this fact it is noteworthy that, in the final paragraph of acknowledgments, Professor Crosby is not mentioned among those personally consulted, although he could have been reached by the author by a half hour's ride. This circumstance is in keeping with the general tone of the text, which leads one to believe that Crosby's work is to be proved faulty. Such a tone is unfortunate, since, with but three or four exceptions, the author is obliged to agree with all of Crosby's conclusions. Of the exceptions noted, one, on page 214, is a resurrection of a controversy between Crosby and Burr, in which the former's contention that certain melaphyr areas were effusive, was disputed by the latter. Crosby's convincing answer to Burr's paper was published in the *American Geologist* (Vol. XXVII., p. 324, 1901), and, so far as known, has met with no reply; but still the present author believes that Burr's conclusions are in many instances correct. Regarding the other exceptions, on pages 206 and 220, the author forms a different *opinion*, but in neither case proves himself correct nor Crosby wrong. In the second case the author states that he has not visited the outcrops which furnish Crosby's evidence. In his concluding chapter he states that, while Crosby attributed to the Roxbury conglomerate a marine origin, 'the evidence, largely negative and unsatisfactory, favors non-marine origin.' This and the conclusion that glaciers may have furnished material to torrents by which it was deposited either upon the land or in lakes are possible, though rather uncertain additions to the previous knowledge of the geology of the Boston Basin. The author has evidently accomplished a great deal of work in the time at his disposal, but it is unfortunate that so criticizing a tone should be employed by a young man in reviewing the work of so experienced and painstaking a geologist as Professor Crosby. It must, however, be exceedingly gratifying to the latter to see that his work, much of it done many years ago, when knowledge of geologic structure was far less advanced than at present, has stood so successfully all critical tests of recent years, and remains still *the* authority.

S. L. W.

A Text-book of Sanitary and Applied Chemistry. By E. H. S. BAILEY. New York, The Macmillan Company. Price, $1.40.

The author states that the object of this work is to furnish a text-book upon applied chemistry that is suitable for use by those students who have had a good high school course in general chemistry. Scattered material is also presented from government reports and other sources which has been added with the intention of making the book useful for reference. Part I. is entitled 'Sanitary Chemistry' and deals with: The Atmosphere, Fuels, Heating and Ventilation, Lighting, Water and its Purification, Sewage and its Disposal, Soap, Disinfectants, etc.

Under Part II. is considered the 'Chemistry of Foods.' Stress is laid upon the vitiated character of 'ground air,' and it is further noted how small is the attention given by the general public to the question of good ventilation, the reason being that the ills arising from a poor air supply are slow in making their appearance.

Under each heading there are experiments introduced with the view to fix in the student's mind the important points covered by the general text. In some cases, however, it is difficult to see the value of these experiments. Thus, those under Water are qualitative only. Surely the results of a water analysis can scarcely be of service unless stated in a quantitative form. The field covered by the work is so very great that it is hardly to be expected that thoroughness can be attained in a book of 345 small pages. We find, for instance, Mineral Waters, Potable Waters, Drinking Water and Disease and the Purification of Water Supplies covered in twenty-three pages, while only seven are given to Sewage Disposal.

There are many things in the book which will interest the student reader, but he must remember that it is essentially elementary. Doubtless the author intended that it should be so considered.

X.

SCIENTIFIC JOURNALS AND ARTICLES

The Psychological Clinic, a journal for the study and treatment of mental retardation and deviation, by Professor Lightner Witmer, of the University of Pennsylvania, is announced. It will be published monthly, beginning in March, except during the summer months, each volume containing about 300 pages. "*The Psychological Clinic* will be devoted primarily to the study and treatment of mentally and morally deficient children, but this will not preclude the consideration of other types deviating from the normal child, nor yet of that hypothetical type, the so-called normal child. The methods of clinical psychology are necessarily invoked wherever the status of an individual consciousness is determined by observation and experiment, and pedagogical treatment applied to affect a change, *i. e.,* the development of such individual mind. Whether the subject be a child or an adult, the result of examination and treatment may be conducted and expressed in the terms of the clinical method. Thus, the phenomena of adolescence, of criminality and insanity, are best investigated by the clinical method. The neurologist and psychiatrist are just awakening to a realization of the psychological and pedagogical significance of the treatment usually prescribed in cases of hysteria, psychasthenia, aphasia and allied mental disorders."

SOCIETIES AND ACADEMIES

THE AMERICAN MATHEMATICAL SOCIETY

THE one hundred and thirty-second meeting of the society was held at Columbia University on Saturday, February 23. President H. S. White and Vice-President P. F. Smith occupied the chair at the two sessions. Thirty-three members were present. The following new members were admitted to the society: Professor T. M. Focke, Case School of Applied Science; Dr. D. C. Gillespie, Cornell University; Professor C. C. Grove, Hamilton College; Professor T. W. Palmer, University of Alabama; Professor N. A. Pattillo, Randolph-Macon Woman's College; Mr. F. D. Posey, University of Chicago; Miss Gertrude Smith, Vassar College; Dr. A. L. Underhill, Princeton University. Ten applications for membership were received.

Professor J. H. Tanner was elected treasurer of the society, to succeed Dr. W. S. Dennett, who retires after seven years' service. Professor E. H. Moore, who has been editor-in-chief of the *Transactions* since the founding of that journal in 1899, retires from the editorial committee at the completion of the present volume. The vacancy in the committee was filled by the election of Professor Maxime Bôcher. Appropriate resolutions expressing the society's grateful appreciation of the services of these retiring officers were adopted.

A standing finance committee, consisting of the treasurer, Professor Brown, and Dr. Dennett, was established to have charge of the investment of the life-membership and surplus funds of the society. For the better regulation of the presentation of papers, it was ordered that papers should hereafter be read in the order and at the session announced on the printed program, except that papers whose reading is postponed may be read at the close of a session. Papers not on the printed program, but accepted for presentation, will be read at the close of a session after the printed list is exhausted. Abstracts of papers will hereafter be included in the printed program if furnished at least three weeks in advance of the meeting.

The Annual Register of the society, containing the list of officers and members, constitution and by-laws, reports of officers, catalogue of the library, etc., has recently been issued. Copies can be obtained from the secretary.

The following papers were read at this meeting:

R. D. CARMICHAEL: 'On dividing an angle into parts having the ratio of any given straight lines.'

R. D. CARMICHAEL: 'A table of multiple perfect numbers.'

G. A. MILLER: 'The groups generated by three operators each of which is the product of the other two.'

R. P. STEPHENS: 'On a quintic with three parallel tangents in any direction.'

E. B. WILSON: 'On the revolutions of a dark body about the sun.'

C. N. MOORE: 'On the introduction of convergence factors into summable series and summable integrals.'

G. A. BLISS: 'The construction of a field of extremals about a given point.'

R. G. D. RICHARDSON: 'Differentiation and integration of definite integrals.'

E. R. HEDRICK: 'On a final form of the theorem of uniform continuity.'

R. D. CARMICHAEL: 'On the classification of quartic curves possessing fourfold symmetry with respect to a point.'

The San Francisco Section met on the same day at Stanford University. The Chicago Section will meet at the University of Chicago on Saturday, March 30. At the next meeting of the society, on Saturday, April 27, Professor W. F. Osgood will deliver his presidential address, postponed from the annual meeting. The subject of the address is 'The Calculus in Colleges and Technical Schools.'

F. N. COLE,
Secretary

SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE

THE nineteenth meeting of the Society for Experimental Biology and Medicine was held in Schermerhorn Hall, Columbia University, in New York City, on Wednesday evening, December 19, 1906, The president, Simon Flexner, was in the chair.

Members present.—Auer, Beebe, Burton-Opitz, Calkins, Davenport, Emerson, Ewing, Flexner, Foster, Gies, Hatcher, Lusk, Mandel (A. R.), Meltzer, Meyer, Morgan, Noguchi, Norris, Sherman, Shaffer, Torrey, Wolf, Yatsu.

Members elected.—Alexis Carrel, Winfield S. Hall, William Ophüls, H. Gideon Wells.

Abstracts of Original Communications[1]

An Experiment on the Localization Problem in the Egg of Cerebratulus: NAOHIDÉ YATSU.

The author found that the third cleavage does not always separate the entodermic stuff from the ectodermic, so that the embryo from the animal half sometimes invaginates and sometimes does not. But in shifting the third cleavage plane to the equator by compressing the egg immediately after the first division (in doing this, the second cleavage is suppressed until pressure is relieved, the third cleavage of the normal egg appearing next to the first) and in separating the animal half from the vegetative, the former always gave rise to an embryo without gut, anenteron. From this it may be concluded that in the egg of *Cerebratulus lacteus,* a little before or at the time of the third cleavage, the entodermic basis extends farther above than that of *Cerebratulus marginatus.*

[1] The abstracts presented in this account of the proceedings have been greatly condensed from abstracts prepared by the authors themselves. The latter abstracts of the communications may be found in number two of volume four of the society's proceedings.

Experiments upon the Total Metabolism of Iron and Calcium in Man: H. C. SHERMAN.

Each of the experiments was of three days duration and the same healthy man served as subject, throughout. On a diet of crackers and milk which furnished 0.0057 gram iron and 2.65 grams calcium oxide (Exp. I.), there was equilibrium with respect to iron, and a storage of calcium. When the diet consisted of crackers and egg-white with 0.0065 gram iron and 0.14 gram lime (Exp. II.), or of crackers alone with 0.0071 gram iron and 0.13 gram lime (Exp. III.) there were losses of both iron and calcium. These losses occurred through the intestine, but were evidently not due to intestinal putrefaction, since the ratio of sulphur in ethereal to that in simple sulphates in the urine was determined in Exp. III. and found to be as 1 : 25. The results appear to confirm the suggestion of Von Wendt that a deficiency of calcium in the diet may lead to a loss of iron as well as of calcium from the body. There was a slight tendency toward diarrhœa in each of the periods in which loss of iron and calcium occurred. The iron requirement evidently varied greatly, the average daily output for three experiments being 5.5, 8.7 and 12.6 milligrams, respectively. The lime requirement was found in further experiments (IV. and V.) to be about 0.75 gram of calcium oxide per day.

The Cause of the Treppe: FREDERIC S. LEE.

The treppe is usually ascribed to increased irritability caused by activity. The cause of the increased irritability has remained obscure. In studying the depressing action on muscle of its fatigue substances the author often observed augmentation of activity instead of depression. A more careful investigation of this phenomenon shows that it may be produced by all of the three recognized fatigue substances, namely, carbon doxide, mono-potassium phosphate and paralactic acid. When a muscle is irrigated with an indifferent fluid containing one of these substances in small quantity, and compared with its mate irrigated only by the indifferent fluid, a fatigue record being made from both, more intense contractions frequently occur in the poisoned muscle at the beginning of the experiment, and may last until exhaustion sets in. When a fatigue record is being made from a muscle with the circulation intact, intravenous injection of a fatigue substance causes augmentation of contraction. The author concludes that the treppe is due to the augmenting action of fatigue substances in small quantities —the same substances which in larger quantities cause depression or fatigue.

An excellent mode of demonstrating the augmenting action of CO_2 in the cat is to record the contractions of the tibialis anticus in the living animal, and while the record is being made, to clamp the trachea. A marked treppe follows.

If two corresponding muscles be compared, one with the circulation intact, and the other with its arteries ligated, the latter muscle performs more intense contractions and exhibits a more rapidly developing treppe, owing to the accumulation of fatigue substances.

The chemical theory of the treppe is able to explain several other known phenomena. The author has experimented on both frogs and cats. The augmenting action of the fatigue substances seems to be observed even when curare is employed.

The Influence of the Red Corpuscles upon the Viscosity of the Blood: Russell Burton-Opitz.

After determining the coefficient for fresh ox serum at 37° C., the serum was gradually concentrated by the addition of definite quantities of red blood corpuscles (washed). The viscosity of the 'blood' was tested after each addition of corpuscles.

The following data may serve as examples:

	Spec. Grav.	No. Red Corpuscles.	Viscosity Coefficient.
Serum	1.0248		2397.7
S + 30 c.c. corp. ..	1.0382	4,000,000	1442.9
S + 30 c.c. corp. ..	1.0467	4,700,000	1009.3
S + 30 c.c. corp. ..	1.0524	5,700,000	851.6

Thus, the increase in the number of red corpuscles caused a corresponding increase in the viscosity. It is also obvious that the red corpuscles constitute the principal factor in determining the viscosity of the blood.

A New Recording Stromuhr, with Demonstration: Russell Burton-Opitz.

The cylinder of this stromuhr is placed horizontally and carries below its floor a valve, by means of which the inflowing blood can be diverted alternately into the right or left half of the instrument. The piston within the cylinder moves back and forth, therefore, in a horizontal direction and records its movements by means of a pulley arrangement and a writing lever upon the smoked paper of a kymograph.

On account of its great sensitiveness, and the possibility of low adjustment, this stromuhr is especially fitted for measuring the blood flow in the veins.

The instrument has been used by the author in testing possible vaso-motor reactions in the pulmonary circuit. It was connected with the vein draining the middle lobe of the left lung. The nerves in the vicinity of the ganglion stellatum were stimulated. So far the experiments have given negative results.

The Influence of Gelatin upon the Viscosity of the Blood: Russell Burton-Opitz.

Solutions of gelatin (1000 : 50) were introduced intravenously after the normal viscosity of the blood had been determined. It was found that the injections resulted in a very prompt increase in the viscosity. The following data may serve as examples:

Specific Gravity.		Viscosity.	
Before Inj.	After Inj.	Before Inj.	After Inj.
1.0565	1.0543	836	772

The Hemolytic Effects of Organ and Tumor Extracts: Richard Weil (by invitation).

The author has found that the cause of the variability in the hemolytic effect of organ extracts, which has been noted by previous observers, is the varying admixture of blood. Kidneys prepared bloodlessly, by perfusion with salt solution, are hemolytic only in very low dilution, and after a long latent interval. Kidneys suffused with blood are, as a rule, very much more active; occasionally less so. The effects of blood have been analyzed by the separate addition of serum, emulsions of white cells (from artificial abscesses), and of red cells after washing, to the bloodless kidney extract. In each case it was found that hemolysis was inhibited. The question, therefore, arises, why are kidneys that have been suffused with blood as a rule more actively hemolytic than the bloodless organs? If their extracts are centrifuged, and all the solid particles, including the red cells, removed, it is found that the extracts are still deeply stained by hemoglobin. This is due to the destruction and solution of red cells, which is inseparable from the process of preparing the extract. The next step, therefore, was to determine the effect of adding red cell constituents to the bloodless organ extracts. This was prepared by adding red cells to distilled water, and then bringing the solution to the strength of normal salt solution. Such a solution adds very markedly to the hemolytic power of the organ extract. Its manner of action seems to resemble that of complement, inasmuch as it is capable of breaking up the red cells only after a preliminary treatment with the organ extract.

Tumors were investigated in the same manner as the kidneys. It was found that the non-necrotic tumors are somewhat more hemolytic than are the kidneys, owing possibly to their blood content. They act, however, in other ways precisely like the latter, their action being diminished by the addition of serum and of white cells, and being increased by the red cell extract.

Necrotic areas of tumors are extremely hemolytic, even up to dilutions of two in four hundred. This hemolytic activity is not affected by the addition of the blood components.

The Enzymotic Properties of Diplococcus intracellularis: SIMON FLEXNER.

The brief vitality of many of the cultures of *Diplococcus intracellularis* is a point of differential importance. Many strains, grown on a favorable medium, unless transplanted to a fresh medium, do not survive beyond two or three days. Cultures three days old show marked degenerations and the latter increase rapidly with age until, at the end of five or six days, or even earlier, no normal cocci persist. As degeneration progresses, loss of staining power and disintegration ensue, until finally, staining is lost and a formless detritus remains.

The changes in the diplococcus are associated with the action of an enzyme which brings about the disintegration. This enzyme does not exhibit the usual properties of a proteolytic ferment: it does not liquefy gelatin or coagulated serum. The degree of rapidity of its action varies with its concentration: at least a heavy suspension of the cocci in salt solution, kept at 37° C., undergoes dissolution more rapidly and completely than a weaker suspension. The vitality of the cultures is associated with the degree of autolytic alterations in the suspensions: cocci in the weak suspensions survive longer than in the stronger ones. At lower temperatures—2° C.—disintegration of the cocci either does not take place at all or progresses much more slowly. Under the latter conditions more cocci survive in the strong than in the weak concentrations, although even here the vitality is a brief one.

The enzyme of the diplococcus acts energetically upon other bacteria, bringing about their dissolution. It acts upon *B. typhosus, B. coli communis, B. pyocyaneus, B. anthracis, M. catarrhalis,* and to a less degree and more slowly upon *Staphylococcus aureus.*

On the Supposed Existence of Efferent Fibers from the Diabetic Center to the Liver: J. J. R. MACLEOD and C. E. BRIGGS.

The authors have found that no hyperglycemia is produced by stimulation of the splanchnic nerves, or of the spinal cord below the cervical region. In the cervical region, on the other hand, stimulation produces hyperglycemia except when oxygen is very freely delivered into the trachea. By such administration it has been shown by Hirsch that the blood remains arterial even after the respiratory movements have been inhibited by curare. When the cervical spinal cord is stimulated, and especially when it is cut, the respiratory movements are very considerably interfered with so that a partial asphyxia is produced which may be the cause of the hyperglycemia.

The fact that stimulation of the cervical cord causes glycosuria can not, therefore, be taken as a proof of the existence of efferent fibers which control the glycogenic function of the liver. Dyspnea may be the cause of the hyperglycemia in these cases.

Regarding the other evidence, which is supposed to point to the existence of such fibers, the authors stated that in all the experiments on which it is based (viz., cutting the splanchnics, or sympathetic chain, or certain roots, or the spinal cord) there must have been induced by the operation, a great fall of blood pressure which, in the cases of dogs with vagal glycosuria, Macleod and Dolley have shown usually to cause a marked depression in the reducing power of the urine.

Conclusion.—When every precaution is taken to prevent asphyxia the authors have been unable, so far, to demonstrate the existence of any efferent fibers whose stimulation causes hyperglycemia.

William J. Gies,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 424th meeting was held January 26, 1907, with President Stejneger in the chair.

Mr. H. W. Clark described some observations on *Riccia lutescens* and *R. natans*. Among the many examples of adaptation in plants to alternating conditions of drouth and moisture, conditions which prevail most strikingly in temporary ponds, perhaps the most remarkable is that furnished by *Riccia lutescens*.

One of the first things to catch the eye of a visitor to the shallow woodland ponds of the northern states is a fleet of little fronds floating on the surface roughly resembling small green butterflies in general appearance. The plants would probably be found to be actively dividing into equal parts by a sort of decay along a median groove, a form of reproduction strikingly resembling that of single cells and lower organisms. Each frond has its under surface beset with long rhizoid-like scales which act as balancing organisms, so that if a frond is overturned it immediately rights itself. According to descriptions in books the plant just noticed is *Riccia* (*Ricciocarpus*) *natans*.

A visit to the same pond during the dry season would reveal numerous orbicular or elongate thalli creeping on the moist ground. These would be determined at once according to the literature as *R. lutescens*.

A third visit to the same pond some time after the rains had filled it would result in the discovery of the lately creeping fronds lifted to the surface, and in all stages of change to the floating phase. Experiments with the plants at the proper season—slowly drying basins containing floating phases, or slowly raising the water surface on the creeping phases—will result in the change of one phase to the other. These experiments might not be perfectly satisfactory, however, if attempted much out of season, for the plant has long been accustomed to making these changes at certain seasons, and would probably yield to treatment more readily at such times. About ten years ago, while attempting the experiments indicated with these plants during the spring, they fruited abundantly.

In view of these facts, one might naturally assume that *R. lutescens* and *R. natans* are different phases of the same species; this conclusion has indeed been darkly hinted at, and finally distinctly stated by one author.[1]

A visit to the marshy edge of one of our lakes or permanent ponds, however, will reveal the real *R. natans* a smaller plant than *R. lutescens*, with deep purple tinge about the edge and in the balancing scales. The balancing scales themselves are much more fully developed in *R. natans* and form a more conspicuous part of the plant as a whole.

[1] Charles E. Lewis, *Bot. Gazette*, XLI., No. 2, pp. 109–138, where a graphic account of all these changes are described and illustrated in full, and the genus is pretty thoroughly revised.

As it is, the floating phase of *Riccia lutescens* has always been identified with *R. natans* and is probably always labeled so in collections. Which species is really *R. natans* can probably only be told by examination of the type specimens. Indeed, it is likely that there are many more specimens of the floating phase of *R. lutescens* labeled *R. natans* than there are of the real *R. natans*, as the former is a much more conspicuous plant, and the latter is for various reasons much more readily overlooked. There are two distinct species in question, and each has two distinct phases.

The following brief diagnosis is, therefore, appended:

Riccia lutescens Schw., floating phase.

Thallus bright green, usually semi-orbicular to orbicular, its approximation to a complete circle being dependent on presence or absence of agitations of water-surface, waves tending to separate thalli into segments. Upper surface deeply furrowed, the main furrow functioning as the dividing line of thalli in vegetative reproduction.

Differing from *R. natans*, floating phase, in larger size, brighter green color, relatively less fully developed balancing scales, and from the fact that it is confined exclusively to temporary waters, while *R. natans* is found only in permanent waters.

Dr. W. H. Dall read 'Notes on Some Cretaceous Volutidæ,' describing the development from the Turonian upward of a group of Volutidæ which seems to have been derived from some type like *Piestochilus* Meek, to have attained a large size and striking characteristics and to have culminated in the upper Senonian or Ripley horizon in various parts of the world. The group is found represented in India, the Tyrol, North Germany and various parts of North America, represented in each region by analogous types, while each regional group has a certain local facies of its own. In North America two lines of descent were noted, represented by the genera *Volutoderma* and *Volutomorpha*, neither of which is represented in any horizon subsequent to the Ripley, though a distinct genus appears in the Tertiary of northwest America which has been more or less confused with them.

Dr. O. F. Cook read a paper on 'Parthenogenesis and Alternation of Generations in the Parasitic Hymenoptera.' The discoveries regarding the embryology of the parasitic hymenoptera presented by Dr. Howard at a former meeting of the society[2] are not merely a contribution to the study of sex-determination. They have brought to light an entirely new form of alternation of generations among the insects. Polyembryony is a botanical term which fits these zoological facts but poorly, and conceals a very close analogy with the typical and original instance of alternation of generations, that of the tunicates, described by Chamisso in 1819.

Polyembryony is the production or inclusion of two or more embryos in one set of seed-coats, instead of the usually single embryo. But it does not follow that the multiple embryos of plants are produced by vegetative propagation of the original egg or embryo, as are the chains of larvæ in tunicates and in these parasitic hymenoptera. Nor does the body of the host in which these chains of insect larvæ are formed correspond to the embryo-sac and seed-coats which enclose the multiple embryos of plants. The multiple plant embryos arise, as far as known, from the presence of two or more ova in the embryo-sacs, or from the intrusion of buds from the nucellar tissue. The polyembryony of plants has no necessary relation to sex-determination, for bisexual or hermaphrodite plants are reproduced from multiple embryos. To multiply sexually differentiated plants by vegetative propagation does not affect their sexual status, and the indications are that this is true also of animals.

Alternation of generations, that is, the alternation between vegetative propagation and sexual reproduction, has long been known among the hymenoptera. The form of alternation recently discovered in the parasitic families differs from the other in that the vegetative propagation takes place at a different period in the life history, in the early

[2] SCIENCE, December 21, 1906, 810, 'Polyembryony and the Fixing of Sex.'

larval phase, rather than in the late larval or the adult stage, as in the gall-forming hymenoptera and the plant-lice. It may be that this new method of vegetative propagation of the larvæ of parasitic insects should receive a distinct name. To call it polyembryony is to mislead all who know what is meant by polyembryony in the original application of this term among plants.

By recognizing the analogy with the previously known instances of alternation of generations among insects the new discoveries become much more interesting than when interpreted as polyembryony. These chains of larvæ correspond to the series of so-called parthenogenetic females of the gall-insects and the plant-lice, which are propagated by vegetative budding. If the fact that all the individuals of the same larva-chain are of the same sex represents a general principle of sex-determination, we must expect to find that this is also true of all the other series produced by vegetative budding. Not only will all the sexual offspring of an individual bud-propagated plant-louse be of one sex, but all the offspring which arise from each original egg of a sexual female. This would mean that what we now call parthenogenetic females are not really females, but represent the two sexes, both temporarily propagated by budding instead of by sexual reproduction. In support of this interpretation we have the fact that even the wingless, larviform, bud-producing plant-lice which are classified as the same species may show two distinct forms, as in the cotton aphis. It now becomes justifiable to suspect that these two forms may represent males and females, and that there may be explained in this way a larval dimorphism which previously appeared altogether mysterious. Adult female plant-lice are wingless, as in all the related families, but the members of these bud-propagating generations are often winged. This has made it necessary to believe that larval females might be winged while the adult females were always wingless, a unique and highly anomalous assumption which the present considerations may enable us to avoid. It is possible that the bud-producing winged insects may prove, after all, to represent the male sex, in spite of their apparent parturition.

One more confusion of terms remains to be noted. The so-called parthenogenesis of these bud-producing plant-lice is entirely distinct from the parthenogenesis of male hymenoptera from unfertilized eggs. If bud-propagation of insects is to be reckoned as parthenogenesis, then the hymenoptera have two forms of parthenogenesis, one a method of sex-determination, the other a method of vegetative multiplication. The hemiptera are now known to have a different method of sex-determination, by means of specialized chromosomes and two or more kinds of spermatozoa, so that the plant-lice should not be expected to agree with the parasitic wasps in sex-determination, even though the methods and results of vegetative propagation should prove to be entirely analogous in the two groups.

Mr. W. F. Wight read the last paper, entitled 'The History of the Cowpea and Its Introduction into America.' This will be published by the U. S. Department of Agriculture.

M. C. Marsh,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

REVERSION INDUCED BY CROSS BREEDING

Dr. Castle[1] in an explanation for reversion, thinks that the wild agouti color has been introduced through the red parent (when crossed with black); and bearing on this is Dr. Davenport's explanation of reversion to Gallus bankiva color, in a cross with white and black poultry, their difference being that Dr. Castle ascribes the reversion of wild color as being added to, and Dr. Davenport as being taken from.

The latter, in his address at the New York meeting, December 28, explains the phenomena of wild color as being due to the absorption of black by the white, leaving the wild color clean. Perhaps both are right in their respective examples.

We wish to tell of some cases, wherein neither of these explanations can be applied as a cause for the reversions.

[1] Science, January 25, p. 151.

Mr. John Chalfant, of Union, Nebr., owns two sows, that from a mating with the same male farrowed seven and eight pigs, all of which bore the seven longitudinal stripes belonging to the young of the wild breeds. The parents—both sire and dams—were three fourths Berkshire and one fourth Poland China, of pure ancestry; and were all black with white points the same as the pure animals of both breeds. When these fifteen striped pigs were ten days old, we predicted to Mr. Chalfant that they would lose their stripes under three months of age, as do our wild pigs (*Scrofa*) at Bear Creek. This proved to be true. Mr. Chalfant was kind enough—at our suggestion—to mate a pair from these reversion litters; and has recently reported the progeny. We quote him verbatim:

> One of the young sows that was striped has farrowed seven pigs, as follows: two black and white spotted, with sandy stripes running lengthwise; one black, with sandy stripes; the other five—nearly black, but with solid, sandy tinge. * * * Surely this must be a reversion of a long way back.

We have here, in this one litter, near and remote reversions; representing three stages, viz., the spotted pigs, representing the Poland China of thirty years ago; the black, with sandy tinge, showing the primitive Berkshire breed of sixty years; and the longitudinal, sandy stripes are a wiping out of all color characters accumulated during domestication.

Agreeing with this is a statement of Mr. John P. Ray, of New York, in conversation with the writer. Mr. Ray originated the Silver Laced Wyandotte twenty-five years ago. Twenty-one years later, he purchased a first-prize cock that had been bred away from his own strain fifteen years. All the progeny from this cock were high-scoring and uniform; but in the product of his daughters, mated with the original strain, were nine pure whites among a hundred chicks.

Another exaggerated reversion occurred at Bear Creek, in pure Short Horn cattle. Two white calves were born, having red ears like the wild cattle of Chillingham. Their sire was a red bull and a first-strain cross between English and Kentucky families. Their dams were both of straight Kentucky strains and both red.

The patience could be overtried with instances of similar phenomena.

In the pig reversions there was a hybridizing not of color, but of chemical. The Wyandottes were of two pure strains in the same breed, and if we may judge from the reputation of their breeders, they were identical in the spots and barrings of every feather.

In the example of the 'wild-color' calves, the parents were identically red in color, were pure registered Short Horns; but three-quarter *strain* hybrids.

We can not believe that this can be accounted for, either by 'adding to' or by 'absorption'; and if your readers will kindly allow, we will propose a solution.

Surmising that heredity in the chromosome is chemic, or enzymic, and in normal line breeding this chemical is stable and repeats itself, but when precipitated by a dynamic reagent the recently acquired less stable hereditary compounds are thrown down, leaving only the old type in many instances. In other cases a new chemical arrangement is produced, as in Burbank's creations and our own swine hybrid monstrosities.

Both Castle's and Davenport's explanations seem logical to their examples, as neither experiment had reached the 'breaking up' stage, wherein we appear to get precipitated reversion.

Castle states that 'ordinary black individuals, while homozygous, are not pure in the sense that they contain no other pigment but black'; and if we understand him correctly, red or brown lies latent. If there were only color reversion, this might apply; but there are also some wonderful morphological reversions to ancestral types, that can only be interpreted as the result of a powerful detergent acting upon the later accumulations of the chromosome.

We are now experimenting with the view of learning rules of hybrid stages of reversion, whereby we can 'break up' or destroy recently acquired characters at will, when these late characters have proven undesirable.

Castle's 'fixation' is good, and the theory may accomplish much, if he can avoid a precipitate. We hope that when he finds his discoveries are being used, 'the true scientific spirit' will yet allow him to continue his experiments and his philosophy.

Q. I. and J. P. Simpson
Bear Creek Farm

RELATIONS OF SALARY TO TITLE IN AMERICAN UNIVERSITIES

Having just read the interesting discussion in Science of February 15, under the above title, I venture to suggest that there is yet more to be said.

University men possess different kinds of value, *e. g.*: (1) Some are principally of value to the student body in attendance on their classes. (2) Some are valuable more particularly to picked individuals in the university and out of it. Such may be said in a certain sense to have as many students as those whose classes are thronged, but they are in many places. (3) Some will be exceedingly valuable to posterity, but their work is comparatively useless to the present generation, because it has not learned to value or use it, or because it will only reach its greatest significance and utility after it has been carried on for two or three generations.

From the standpoint of the state, all these classes of men are of value and should be supported. If there is any difference in their value, no doubt the pioneers, those of the third class, are the most valuable; but it requires very little reflection to see that these, from a psychological necessity, will be the *least* valued by presidents, trustees and the community at large. To properly estimate the value and importance of an 'infant industry' in the intellectual field requires imagination of such quality that those doing the work do not always possess it, and outsiders almost never.

It is quite possible to argue that the concern of the university is only with the students in attendance, so that all values must be determined by the standard applicable to the first of the above classes. This notion, however, is surely passing away, and with it the possibility of correctly estimating the money value of university men. The larger outlook also serves to convince us that the actual worth of certain professors, having in view their total influence upon contemporaries and posterity, exceeds any sum that can be thought of as payment. On the other hand, the *needs* of the great and the small are not so very diverse.

There is one kind of payment which should no doubt differ greatly according to the character of the man and his work. This is for the support of the work itself. One man may need expensive apparatus, or journeys to distant lands, while others may have no use for these things. This is not necessarily dependent in any way on the eminence of the man himself, but rather on the character of his labors; only, of course, he should be able enough to use well the means provided.

T. D. A. Cockerell
University of Colorado,
Boulder, Colo.,
February 18, 1907

SPECIAL ARTICLES

RIVER CAPTURE IN THE TALLULAH DISTRICT, GEORGIA

The head-waters of the Savannah River have been frequently referred to as an example of drainage transferred from the gulf system to the Atlantic through the process of stream capture. Dr. C. Willard Hayes, in his paper on 'The Southern Appalachians,' published as a National Geographic Monograph, cited this case as an instance of recent capture and ascribed the falls on the Tallulah River (one of the head-waters of the Savannah) to the fact that the newly acquired drainage had not been in possession of the captor sufficiently long for the falls to be worn down to grade. In a paper entitled 'Drainage Modifications' (*Jour. Geol.,* 1896) Mr. M. R. Campbell notes this capture under the heading 'remote changes shown in the streams of the Atlantic slope.' Mr. Chas. T. Simpson (Science, 1900), in discussing 'The Evidence of the Unionidae regarding the Former Courses of the Tennessee and other Southern Rivers,' reports the finding of mollusks similar to the Tennessee and Coosa River forms in the

Chattahoochee and Savannah Rivers, and mollusks of the Savannah drainage in the Chattahoochee River. From this he was led to support the theory of a capture of the upper Chattahoochee drainage by the Savannah River, since the diversion in question would effect the transferrence of fresh-water faunas from one stream to the other. A paper on 'The Geology of the Tallulah Gorge' (*Am. Geol.,* 1901), by Mr. S. P. Jones, discusses the general geology of the region, mentions the possibility of capture already suggested by Hayes and Campbell, but reaches no conclusion in regard to the matter. Other references to this capture are found in the literature, but need not be repeated here.

Believing that the upper Savannah would probably show evidence of river capture, and desiring to make a test of certain principles which had been applied in the case of the supposed capture of the upper Tennessee River with only negative results, the writer spent a month in the Tallulah district during the spring of 1905, studying the geologic and physiographic features of the district with special reference to the supposed changes in drainage. The evidence in favor of capture appeared to be quite conclusive. A detailed discussion of the various elements of the problem will be found in the *Proceedings* of the Boston Society of Natural History, Vol. 33, No. 5, but it is desired to outline certain phases of the subject here.

The results of this study appear to justify the following conclusions:

1. The upper Savannah (Chattooga) River formerly flowed southwest through the Chattahoochee River into the Gulf of Mexico, but was diverted to the Atlantic drainage by a process of stream capture, as already announced by Hayes and Campbell.

2. The capture furnishes an example of what may be termed 'remote capture,' having occurred so long ago that much of the direct evidence has been obliterated.

3. The capture seems to have resulted from the advantage gained by the Atlantic drainage over the gulf drainage, owing to the shorter course to the sea which streams of the former system enjoyed, although the process may have been aided by crustal warping.

4. The place of the capture was near the junction of the Tallulah River with the Chattooga, and probably just below that junction.

5. The falls of the Tallulah River, while initially caused by the capture, exist to-day because of a hard rock barrier crossed by the river, but not yet worn down by it.

6. The similar falls which must have existed on the Chattooga River have been obliterated by that stream, since the great lapse of time since the capture has been ample for it to grade its course in the less resistant rock over which it runs.

The Tallulah district is crossed from northeast to southwest by the southwestern extension of the Blue Ridge escarpment, which is from 500 to 600 feet high at this point. The escarpment connects a higher with a lower peneplain, both of which are eroded on folded crystallines, the higher one sloping gently toward the west, the lower one gently toward the southeast. The streams on the higher level flow in fairly mature valleys cut but slightly below the general surface. In like manner the streams on the lower level flow in broad shallow valleys; but their upper branches are working actively headward into the higher level, thus pushing the escarpment backward to the northwest, and capturing additional drainage areas from the rivers flowing on the surface well above them.

In striking contrast to these two classes of streams, on the lower and upper levels, respectively, is a drainage system cut down *into* the upper level. This is the upper Tugaloo-Chattooga River, which flows in a deep gorge cut 500 feet or more below the upper level, until it breaks from its gorge at the face of the escarpment and flows out over the lower level in a fairly mature valley. Nothing could be more striking than the contrast between this young stream with its picturesque, steep-sided chasm, cut *in* the upper level, and the mature streams in broad open valleys flowing *on* the upper and lower levels. The Tallulah River first flows through low mountains on the upper level, but when near its junction with the Chattooga suddenly plunges down into a gorge known locally as the 'grand chasm,' and so joins the drainage system, which is cut so deeply into the upper level. The rapid descent into the chasm is made by a series of falls and rapids, known collectively as the Tallulah Falls.

In connection with this peculiar feature of the drainage, is a striking drainage pattern. The Chattooga River flows almost due southwest until it is joined by the Tallulah, and then turns abruptly at a right angle and under the names Tugaloo and Savannah Rivers flows almost due southeast into the Atlantic. Near the point where this sharp bend or elbow occurs, a stream on the upper level, Deep Creek, takes its rise and continues the southwest line begun by the Chattooga, until, under the names Soque and Chattahoochee Rivers, it reaches the gulf.

It is this assemblage of features which has suggested that the Chattooga River formerly flowed southwest into the gulf drainage, being continuous with and forming the upper part of the Chattahoochee; that one of the headwater branches of the Tugaloo-Savannah system succeeded in eating headward into the escarpment so far that it undermined the valley of the Chattooga-Chattahoochee some 500 or 600 feet above, thus diverting the upper part of that system into the Atlantic drainage, and causing it to flow down to the lower level and so on southeast to the ocean. As a result of this capture falls and rapids would be established where the descent from the higher, older course down to the lower, new course was made. These falls would be gradually worn back, leaving a young valley or gorge below them, in the vicinity of the sharp bend or 'elbow of capture' where the stream changed from the southwest to the southeast course. From the elbow of capture the remaining portion of the beheaded stream would continue its southwest course, flowing in a more mature valley on the upper level.

The features in the Tallulah district are so striking and so typical that it seems difficult to account for them on any other basis than the theory of capture. Certainly the conditions are eminently favorable for capture; and when one sees the streams on the lower level actively engaged in gnawing headward into the steep escarpment and thus undermining the upper level, he feels a growing conviction that the streams on the upper level are in imminent danger of diversion. So when he witnesses those features which must necessarily follow capture, he is not at all surprised, but adopts the theory of capture as a matter of course. There are certain features of the Tallulah district which have caused some doubt as to the efficiency of river capture, and which, therefore, deserve special attention. It would appear that if the capture is so recent that the stream has not yet had time to wear back the falls and rapids produced by capture, then there should be falls in the Chattooga River as well as in the Tallulah. The former is no larger a stream than the latter, and so far as volume goes, no better fitted to grade its course. Recency of capture, therefore, does not seem competent to explain the falls in the Tallulah, when there are no corresponding falls in the Chattooga. Mr. Jones concluded that there was no difference in the resistance of the rocks over which the two streams ran, and was, therefore, led to doubt the fact of capture, and to suggest some other alternative. Professor Davis believed that the topographic features indicated capture, and in order to account for the falls in the Tallulah and their absence in the Chattooga, suggested that the former courses of the rivers might have been such that the Chattooga was captured first, and the Tallulah not until a later period, thus giving more time for the reducing of the falls in one case than in the other.

After careful investigation it appears that the difficulty lies in the interpretation of the character of the rocks over which the two streams run. Instead of sameness of character, there is seen to be the most significant difference in composition and ability to withstand erosion and weathering. The Chattooga River flows over a mica schist, which in all parts of the region is seen to offer little opposition to stream erosion. Occasional more quartzose bands have determined the location of minor falls or rapids along the smaller branch streams, but even these small branches of the deeply incised drainage (except in the case of the very smallest examples), have eroded deep valleys in the mica schist. On the other hand, the Tallulah River is found to cross a resistant barrier of dense, hard quartzite near its junction with the Chattooga. This rock is wholly distinct from the mica schist, contains little else than fine quartz grains firmly cemented into a massive bluish-gray quartzite. It is less apt to be sheared than the other rocks of the region, having to a great degree withstood the crushing incident to regional metamorphism; it weathers with difficulty, and wherever present forms a serious obstacle to stream erosion, even a small layer of it determining the site of falls on some of the branch streams. A great thickness of this resistant rock is crossed by the lower course of the Tallulah. A traverse of several miles along the lower course of the Chattooga resulted in the finding of nothing but mica schist in the ledges and in the boulders in the stream bed, so it seems apparent that this stream has not encountered any of this hard rock in its down-cutting, but is located wholly on the weaker mica schist.

The presence of the falls in the Tallulah River and their absence in the Chattooga now become perfectly intelligible. When the capture occurred falls were produced which at once began to be worn back. After retreating to the junction of the Tallulah and Chattooga Rivers two series of falls were formed, one retreating up the course of the former river, and one up the course of the latter. The falls in the Chattooga were worn back to grade during the long period of time which has elapsed since the capture, for the weak mica schist offered no great obstacle to the river's attempt to grade its course. The falls in the Tallulah have not been thus worn back, since the hard-rock barrier with which that stream had to contend has made the process of grading a very slow one, a retreat of but a few miles having been accomplished thus far, and the work still continuing very slowly under the extremely unfavorable conditions which prevail.

There are many direct evidences of river capture which have been obliterated during the long period of erosion since the capture occurred. The former channel of the Chattooga across the divide to the southwest cannot be seen; stream dissection has been extensive and all traces of that channel itself have been removed. The valley of the Chattooga has been so widened that no bench or terrace remains to indicate the abrupt change from the former mature upland valley to the deeply incised gorge. The lack of adjustment between the shrunken beheaded stream and the broad valley made to accommodate the former larger river, has given place to a newly established adjustment. The evidences of *recent* capture are lacking, and everything points to a great lapse of time since the capture, permitting the effacement of the more temporary effects of capture, and a considerable degree of adjustment to the new order of things. The evidences of remote capture, however, are none the less conclusive.

The theory of capture has been supported on the basis of certain facts in the distribution of the fresh-water faunas. It appears that a few shells from the Chattahoochee and more western drainage basins are found in the Savannah River, while a number of forms from the Savannah system are found in the Chattahoochee system. It is argued that these forms must have passed between the two systems at the time of capture.

That the fauna of the upper Chattahoochee (the Chattooga River) might be transferred into the Savannah River by the capture would appear quite possible. It is much more difficult to account for the transfer in the opposite direction, however, if we limit ourselves to river capture as the means. Yet the main transfer is supposed to have been in that direction. That shells could have passed from the lower level of the Tugaloo up 500 or 600 feet over falls and rapids to the higher level of the Chattahoochee, does not seem probable. It is possible, of course, that the headwater portion of the capturing stream may have been a more even slope instead of a series of falls, but the evidence of other streams working headward into the escarpment suggests that the capture was most probably initiated by a series of more or less prominent cataracts. These would effectually prevent transfer through the water itself. Furthermore, there is reason to believe that the transfer of water may have been nearly or quite completed before an actual surface valley connection was formed, since (as Lane has pointed out) leakage through the rocks from the higher to the lower level will go on for a long time, possibly increasing until all of the water from the higher level passes underground to the lower stream, leaving a dry channel for some distance below the point of capture in the upper valley. Under these circumstances it would be difficult to conceive of any transfer of faunas from the lower to the higher stream which was dependent upon direct fresh-water communication. On the other hand, there are so many means for the dispersal of fresh-water shells, and the evidence in other localities is so conclusive that they have been dispersed by such means, that we may reasonably suppose shells from either of the two systems might be transferred to the other independently of the capture. For this reason I do not believe that the distribution of the shells can be urged as a proof of capture, although the fact of capture is well attested by other lines of evidence.

The conclusion in favor of the theory of capture is further confirmed by the presence of old river gravels along the former southwestward course of the Chattooga. In view of the fact that the capture occurred at a remote period, we should not expect to find the former channel preserved, nor to find the gravels deposited by the river along that channel in their proper place. The stream dissection which we have already seen destroyed all traces of the former channel, would also wash the gravels down the slopes of the growing ravines and valleys, in many cases removing them altogether, but possibly leaving remnants in specially favored spots on the slopes and in the valleys. A careful search revealed the presence of these gravels, usually as scattered pebbles and boulders on the hillsides, but occasionally as considerable patches in the bottoms of small branch valleys. There was no mistaking their character. The present stream-borne material, even in the largest of these branches, is quite angular and evidently of local origin. The gravels are beautifully rounded quartz pebbles, cobbles, and boulders, somewhat roughened where exposed to the weather for a long time, but perfectly smooth where recently unearthed. In one place they were so abundant that a farmer had made numerous large piles of them in an ineffectual attempt to clear a small plot of ground for agricultural purposes. Their occurrence, together with the unequivocal topographic evidence, would seem to remove the question of capture from the realm of theory, and place it definitely in the realm of known facts.

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REASONS FOR BELIEVING IN AN ETHER

Many scientific men who are not physicists feel an ill-defined distrust of some of the more or less complex conceptions of modern physics. They feel that the physicist has, perhaps, allowed his imagination to carry him too far and has not stopped often enough to reexamine the foundations of his faith.

Perhaps the most fundamental conception exciting some such distrust from the outside is that of the æther which is assumed to fill all space. The non-physicist who has read of the oft-repeated but entirely unsuccessful attempts to detect the 'ether wind' due to the earth's movement through space, and of the negative results of all 'direct' experiments on the æther, begins to feel that the builders of physical theory are perhaps unreasonably tenacious of an idea which could, perhaps, best be dispensed with.

It may not be out of place, therefore, to state as briefly and clearly as possible several reasons for belief in an ether, reasons sufficient because based directly on observation or experiment.

The most important evidence is the simple fact that *the velocity of light does not depend on the velocity of the source.* This is shown by the normal apparent shape of the orbits of binary stars, which, it is easy to see, would otherwise appear distorted. For, if the orbital velocity of one member of a binary star affected the velocity of its light, it would affect the time required for the light to reach us and would, therefore, change the apparent position of the star at any instant. The resulting distortion, which in some cases would be great, has been carefully looked for but has been found absent.

Thus it is a fact founded on observation that the velocity of light is independent of the source. The meaning of this fact can be made clear in the following way. Suppose an observer imprisoned in a windowless box which is thrown at random into space. Such an observer, meeting no experimental difficulties, could discover and accurately define his speed through space by simply measuring the velocity of light in various directions within his enclosure, and, this, be it noted, without any reference whatever to any outside body. In general, he would find that light travels faster across his box in one direction than it does in another, for, as we have seen, the real velocity of light in space is not affected by the motion of the light source which he carries, and hence a change in his motion would change the apparent velocity of light within his box.

Thus space possesses what we might call a 'positional' property, by means of which the magnitude of any motion can be defined without reference to any body in the universe, and this motion is what physicists call 'motion with respect to the ether.'

There is an entirely different experimental truth which leads to the same conclusion as the above.

It is a generally accepted truth that two similarly charged bodies when moved side by side have, superimposed upon their mutual repulsion, an attraction which depends upon the fact that when moving they act like two parallel electric currents. This follows from Roland's classical experiment.

Now if the two charges, stationary with respect to each other, are considered alone in space, it is evident that they furnish our imprisoned experimenter another means of finding his motion relative to space, for the strength of the above-mentioned attraction depends only upon this absolute motion. This leads to the same conclusion as formerly, that space has a 'positional' property.

Other evidence of a similar kind might be given, but the above is sufficient to make it clear that at least so far as it represents this positional property of space the conception of an ether is thoroughly legitimate.

A being from a planet which possessed no atmosphere, if he came to earth, might first become conscious of our atmosphere through feeling it set in motion relatively to himself when he moved. In a somewhat similar way the ether manifests itself, since we know it through its motional property.

We are conscious of matter only as a collection of properties and one of these properties certainly is that it is capable of marking position. Therefore, the unknown reality, which exhibits this positional attribute in space as one of its properties, can be said to resemble matter to this limited extent at least, and upon this sure foundation can fitly be based the physicist's conception of an ether.

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NOTES ON ORGANIC CHEMISTRY

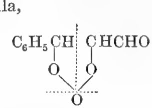
GLYOXAL

THE properties of the substance known as glyoxal, which is the simplest dialdehyde, certainly do not accord with its formula, OCHCHO. Some text-books try to evade this difficulty by describing it as 'a hydrate,' but this, to say the least, is hardly satisfactory. The matter has been cleared up very completely by a recent paper of Harries and Temme.[1] Glyoxal is found to exist in four forms: (1) Monomolecular glyoxal, OCHCHO, which is unstable and is described more fully below. (2) A trimolecular modification $(C_2H_2O_2)_3$, which is a colorless, pulverulent solid, readily soluble in water. It forms no characteristic derivatives, *i. e.*, reagents give with it only derivatives of (1). (3) Paraglyoxal $(C_2H_2O_2)_x$, a white powder, insoluble in water. (4) The ordinary form of glyoxal, now termed polyglyoxal $(C_2H_2O_2)_n$, which has been known during the past fifty years.

[1] *Ber. d. Chem. Ges.*, **40**, 165 (1907).

Monomolecular glyoxal is prepared by distilling a mixture of commercial glyoxal (4) with phosphorus pentoxide, and cooling the distillate by means of solid carbon dioxide and ether. Care must be taken to avoid the presence of moisture in the apparatus. The compound crystallizes in yellow prisms or spangles, becomes opaque at 10°, melts at 15° and boils at 50°. The vapor is intensely emerald green and condenses to a liquid which is at first green; as this is cooled it becomes yellow and, at very low temperatures, colorless. The vapor has an odor like that of formic aldehyde, but as inhalation continues the smell becomes sweet and not disagreeable. It burns with a violet flame and forms with air a mixture which explodes violently on the application of a flame. Glyoxal changes spontaneously into paraglyoxal (3) in a few hours, but on the addition of a *little* water the transformation is instantaneous. If, however, glyoxal be *added to a large* volume of water it dissolves with a hissing noise and the resulting liquid, which has a decidedly acid reaction, consists of an aqueous solution of monomolecular glyoxal (1). The glyoxal volatilizes with the steam when the solution is boiled. The solution readily reduces ammoniacal silver nitrate solution, but not Fehling's solution, in which latter respect it resembles the poly- and para-modifications.

Trimolecular glyoxal (2) is readily prepared by treating cinnamic aldehyde, $C_6H_5CH:CHCHO$, with ozone. The resulting ozonide, when mixed with water, gives hydrogen peroxide, benzoic aldehyde (or benzoic acid) and the glyoxal. The reaction may be represented by the formula,



the dotted lines showing the positions at which cleavage occurs.

The intense color of glyoxal is very interesting, the simplest diketone, diacetyl, $CH_3COCOCH_3$, is also strongly colored, whereas oxalic acid, HOCOCOOH, is absolutely colorless, yet all three substances possess in common the grouping OCCO, in one case united with hydrogen or methyl (colored), in the other combined with hydroxyl (colorless). It is tolerably certain that, of the compounds consisting only of the elements carbon, hydrogen and oxygen, glyoxal is the simplest one to exhibit color.

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CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

HAIL-SHOOTING—THE QUESTION SETTLED

It may be remembered that the 'hail-shooting' campaign in Europe was made the subject of a conference held at Graz in 1902. This body concluded that the results up to that time had been negative, and the hope was expressed that the experiments might be continued and carefully watched. The Italian Minister of Agriculture appointed a commission to undertake new experiments, under the presidency of Senator Blaserna. After a study of the results during the period 1902–1906 the conclusion reached by the Commission (*Att. dei Lincei,* 1906, II.; *Ciel et Terre,* January 16, 1907, 591–592) is that the five-year period has yielded an absolutely negative result. The commission reports that there is no hope of preventing damage by hail by means of the so-called 'hail-shooting,' and that protection must be sought along wholly different lines. The question may now be regarded as definitely settled.

TROPICAL CYCLONE TRACKS

We had occasion recently to call attention in these columns to a report by A. Schück, entitled 'Zur Kenntniss der Wirbelstürme' (Hamburg, 1905). A second report on the same subject has since been issued (dated 1906), presenting, in elaborate detail, the facts now at hand regarding the tracks of tropical cyclones in the West Indies, the Indian Ocean, and in the Pacific. Dr. Schück has made search through all available literature, and has plotted the tracks so far as known, on numerous charts. In future all students of cyclonology will need to refer to Schück's work.

THE BRÜCKNER PERIOD OF RAINFALL AT RIO DE JANEIRO

A RECENT study of rainfall at Rio de Janeiro, summarized in the *Meteorologische Zeitschrift* for January, 1907, shows that a thirty-five-year periodicity seems to prevail there. It is interesting to observe the increasing number of cases of periodicity in climatic averages which fall in line with the Brückner period. An important difference between the original work by Brückner and these later investigations is, however, this: that Brückner started on his quest without prejudice in favor of any particular period, while the more recent students of the same subject have naturally been prejudiced by the conclusions reached by the author of the now famous 'Klimaschwankungen seit 1700.'

METEOROLOGICAL PHENOMENA OF VOLCANIC ERUPTIONS

THE inflowing air currents towards volcanoes which are in active eruption, and the local whirlwinds which are sometimes generated in these currents, have been described by several writers. During the last eruption of Vesuvius (April, 1906), as pointed out by W. H. Hobbs (*Journ. Geol.*, 1906, 636–655) the windows of some houses were broken on the side away from the mountain. This is explained as having been due to the strength of the air currents which were moving towards the volcano.

TREE-PLANTING FOR SNOW-BREAKS

TREE-PLANTING has been begun along the western lines of the Canadian Pacific Railway. Over one hundred miles of trees are to be planted between Calgary and Winnipeg for snow-breaks. Experiments are to be made with tamarack for use as ties, and plantings of jack pine and tamarack are to be started at Medicine Hat in this connection.

NOTE

THE general title under which these 'Notes' have been printed since 1896 is changed with the present number of SCIENCE from *Current Notes on Meteorology to Current Notes on Meteorology and Climatology.* The latter title expresses more clearly the scope of the subjects which are here included, and gives deserved prominence to the geographical aspects of meteorology which are properly embraced in the term climatology.

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PHOTOGRAPHS OF FAINT STARS[1]

THE number of facts now being accumulated by means of photographs of the stars is enormous. Unfortunately, only a small portion of these facts is now available, and therefore of any use to science. This applies particularly to the faint stars. Many photographs are taken by professional and amateur astronomers, which are followed carefully during long exposures with telescopes having large apertures. In some cases, several hundred thousand stars appear upon a single plate. Unfortunately, no record has been published of many of these photographs, and therefore no use can be made of them. It is the object of the plan described below to remedy this difficulty.

The Harvard collection of photographs in part supplies this need, for stars of the thirteenth magnitude and brighter. On the average, this collection contains images of all the stars of the fifth magnitude and brighter on over a thousand nights. The number of these stars is about two thousand, the photographs are distributed throughout the last twenty years, and cover all parts of the sky. For stars of the twelfth magnitude, which can be taken with a lens of one-inch aperture and an exposure of one hour, the number is reduced to five hundred. This includes the stars in the Harvard Map of the sky, about two million in number. There are about five million stars of the thirteenth magnitude and brighter. They appear on plates taken with eight-inch doublets, and having exposures of ten minutes. About two hundred images of each of these are contained in the Harvard collection. For fainter stars, the number of images falls off very rapidly. Stars of the

[1] Harvard College Observatory, Circular 123.

fourteenth magnitude would appear on plates, having exposures of ten minutes, taken with the twenty-four-inch Bruce telescope, and on plates, having exposures of an hour, taken with the eight-inch telescopes. The average number of times the whole sky is thus covered does not exceed three or four. A large part of the southern sky is covered by plates having an exposure of an hour or more, taken with the twenty-four-inch Bruce telescope. One hundred and sixty regions, covering a tenth of the sky, have exposures of four hours. While, therefore, material exists for studying the variability of stars of the thirteenth magnitude or brighter, but few images can be found of stars much fainter than this limit, except in special regions like the Nebula of Orion. This limit could probably be extended nearly two magnitudes, if the following plan could be carried out. For the present it seems best to confine this work to photographs having an exposure of half an hour or more, and taken with doublets whose aperture is six inches or more. Photographs taken with large reflectors or single lenses may take fainter stars, but in general they relate to special regions and except in the charts of the Astrographic Congress do not cover large parts of the sky. The latter great work will of course, eventually, furnish one or two images of all stars of the fourteenth magnitude, and brighter.

Information is frequently desired regarding very faint stars. For instance, evidence of the previous existence of a new star; of a star now missing; the position of an asteroid; the magnitude at minimum of a faint variable; early position of a faint star suspected of proper motion. Three questions suggest themselves. Where are the principal collections of photographs showing faint stars, and what is their character and extent? Does a photograph exist which is likely to contain the required information? Is this photograph accessible, so that the information desired can be secured from it? Satisfactory answers to these questions can generally be obtained for stars brighter than the thirteenth magnitude, but not for fainter stars. Astronomers having collections of photographs showing such faint stars are urged to publish the material required to answer these questions, or to send the information needed to this observatory. The principal facts desired are given below:

1. A description of the instrument, stating its location; form of lens; maker; aperture; focal length, or scale of photograph, that is, number of seconds of arc to the millimeter; size of plate, or region covered; diameter of circle over which stars as faint as the fourteenth magnitude are shown; kind of plate used; name of observer.

2. A list of photographs, which should include the number designating each plate; the time of the center of the exposure (found by adding half the duration of the exposure to the time of beginning), expressed either in Julian Days and thousandths following Greenwich Mean Noon, or in calendar days, hours and minutes; the right ascension and declination for 1900 of the center of the plate; the time of exposure in minutes; the quality on a scale of 5, 5 denoting that the images are circular and that no serious defect appears upon the plate, 4 that the images are nearly circular so that their positions can be accurately measured, 3 that faint stars are shown so that the relative brightness can be accurately estimated, even if the images are not circular.

3. A statement whether the owner is willing to examine and if necessary measure his photographs to furnish any desired information; to lend them under suitable restrictions, so that copies may be made; to furnish contact prints at cost.

It is the policy of the Harvard Observatory to make the material it has collected as widely useful as possible. Accordingly, preparations are being made to publish the facts asked for in Nos. 1 and 2. For many years the observatory has offered to furnish copies of its photographs at cost to whoever might desire them. If this policy is adopted at other observatories, a purchase of large numbers of photographs of faint stars is contemplated here. The observatory will take charge, temporarily or permanently, of good photographs for which the owners have no further use, and will render them accessible to astronomers by the methods described above.

EDWARD C. PICKERING

January 19, 1907

THE SEISMOLOGICAL SOCIETY OF AMERICA

THE Seismological Society of America was incorporated under the laws of the state of California on February 13, with a board of directors as follows: George Davidson, president; Andrew C. Lawson, 1st vice-president; T. J. J. See, 2nd vice-president; Alex. G. McAdie, 3rd vice-president; J. N. Le Conte, treasurer; Geo. D. Louderback, secretary; Chas. Burckhalter, W. W. Compbell, C. Derleth, Jr., G. K. Gilbert, A. O. Leuschner, J. S. Ricard.

A letter has been issued, which says: In October 1891 there occurred in Japan what is now historically known as the Mino-Owari Earthquake. Over 7,000 people were killed, 17,000 injured, and 20,000 buildings destroyed. This disaster so impressed the people of Japan that a national movement resulted in the formation of a large general society for the study of earthquake phenomena. An earthquake investigation committee was appointed by the emperor. This committee has already published fifty volumes in Japanese and over twenty in foreign languages. Seventy-five stations have been provided with seismographs and over 1,500 stations report. As a result, Japan is now the foremost nation in activity of earthquake investigation and in devising and applying scientific methods of protection.

The earthquake of April 18, 1906, has brought home to us also the desirability of organization for similar purposes, with the hope that our labors may ultimately be of value to the people in protecting life and property. It is in this spirit that the Seismological Society has been organized. It aims to collect accurate information concerning the mode of action and effects of earthquakes, to establish and train a corps of reliable observers throughout the country, to disseminate the truth among the people, to inform them of the methods devised in various countries to protect life and property, to supplant any element of terror or helplessness which results from imperfect knowledge by an interest in natural phenomena and a sense of security resulting from familiarity with the facts and the taking of reasonable precautions.

To be of general benefit the society must have a large membership, and the dues ($2.00 per year, life membership $25.00) have been placed low for this purpose. The society will inform its members from time to time as to the progress of its work and hopes in the near future to establish a regular series of publications.

Those who are in sympathy with the objects of the society are requested to communicate their desire for membership by writing to the secretary, Professor George D. Louderback, University of California, Berkeley, Cal.

SCIENTIFIC NOTES AND NEWS

A SPECIAL act of congress, passed on February 27, authorized the president to appoint Lieutenant and Assistant Surgeon James Carroll, U. S. Army, a surgeon with the rank of major, in recognition of his important experimental work on yellow fever. The president at once approved this bill and sent the nomination to the senate, and it was confirmed promptly, so that Dr. Carroll is now a major in the Medical Department of the Army.

DR. FREDERICK REMSEN HUTTON, for thirty years adjunct professor and professor of mechanical engineering at Columbia University and for six years dean of the faculty of applied science, will become professor emeritus on July 1 next. A suitable tablet commemorating Professor Hutton's services will be placed in the mechanical engineering laboratory.

PROFESSOR LE ROY C. COOLEY will retire from the active duties of the chair of physics of Vassar College at the end of this year.

PRINCE ROLAND BONAPARTE has been elected a member of the Paris Academy of Sciences in the room of the late M. Bischoffsheim.

MR. JOHN HAYS HAMMOND has been nominated president of the council of the American Institute of Mining Engineers, which will hold its next meeting in New York City during April.

The sum of about $16,000 has been subscribed toward purchasing the homestead of Mr. Alexander Graham Bell at Bradford, Ontario.

Captain Amundsen, the Norwegian explorer, who recently sailed through the Northwest Passage in the *Gjoa*, has lectured before several Paris scientific societies and has been made commander of the Legion of Honor. Captain Amundsen expects to visit the United States in the autumn.

Mr. Frank M. Chapman, curator of ornithology of the American Museum of Natural History, is engaged in making a collection for the museum of southern birds, especially of white herons in various stages of development.

A grant of $500 has been made from the Hodgkins fund of the Smithsonian Institution, to Professor R. von Lendenfeld, of the Royal Zoological Institute at Prague, for an investigation of the organs of flight of Lepidoptera, Hymenoptera and Diptera. In this investigation the wings of the insects will be considered in their relation to mechanical flight, as well as from a purely morphological point of view.

The Carnegie Institution has renewed its grant of $1,000 to Professor Haskins, of Harvard University, for the exploration of documentary materials for Anglo-Norman history.

Mr. J. A. Fleming, of the department of terrestrial magnetism of the Carnegie Institution of Washington, is at present engaged in making magnetic observations in Central America.

Dr. Bradley M. Davis has resigned from the botanical department of the Marine Biological Laboratory and Dr. George T. Moore will be in charge next summer. Dr. Moore may be addressed at West Chester, Pa., until June 1, and after this date at Woods Hole, Mass.

President Eliot spoke before the Canadian Club of Montreal, on February 22, on 'Advantages of Variety in Experiments on Free Government.' On the same day he spoke at the annual dinner of the American University Men's Association in Montreal. On February 23 he spoke before the Canadian Club of Ottawa on 'The Way of Escape from Competitive Armaments.'

At Brown University lectures have been given by Professor Josiah Royce, of Harvard University, on 'Provincialism,' and by Professor E. C. Sanford, of Clark University, on 'The Rôle of the Different Senses in Mental Life.'

Professor E. F. Nichols, of Columbia University, lectured on March 12, before the Middletown Scientific Association, his subject being 'The Pressure due to Radiation.'

The Society for Ethical Culture has arranged for a course of free lectures on 'The Mental Life of Animals' to be delivered at the Ethical Culture building, Sixty-third Street and Central Park West, on the following dates: March 6—'The Behavior of the Lower Animals,' Professor H. S. Jennings, Johns Hopkins University; March 13—'Some Remarkable Instincts of Ants,' Professor W. M. Wheeler, American Museum of Natural History; March 20—'The Behavior of the Higher Animals,' Professor E. L. Thorndike, Teachers College, Columbia University. The lecture begins at 8:15 P.M.

In view of the death of Professor Charles Edward Garman, of Amherst College, Professor Frederick J. E. Woodbridge, of Columbia University, will give a series of lectures before the department of philosophy during the spring term.

Professor John Krom Rees, since 1881 professor of geodesy and astronomy and director of the Observatory of Columbia University, died on March 9, in his fifty-sixth year. Professor Rees had been ill for several years and had recently been made professor emeritus.

Professor Henry Davis Todd, U. S. N. (retired), died at Annapolis, on March 8, at the age of sixty-nine years. Professor Todd served through the civil war with distinction and became head of the Deparment of Physics and Chemistry at Annapolis in 1878. From 1886 to 1899, he was assistant on the Nautical Almanac, and was director from 1899 to 1900, when he was retired.

M. HENRI MOISSAN, professor of general chemistry at the Sorbonne and director of the Institute of Applied Chemistry, known especially for his work on fluorine and with the electric furnace, died at Paris, on February 20, at the age of fifty-five years.

DR. SCHREIBER, of the Russian Army, died at Kronstadt on March 7, from the bubonic plague, which he contracted while carrying on experiments with bacilli at the Alexander Laboratory. Dr. Pedlevsky, who was working in the same laboratory, has also contracted the disease.

Dr. Alfred Kirchhoff, emeritus professor of geography, University of Halle, died on February 8, aged sixty-eight years.

The death is announced of Mr. Henry Chamberlain Russell, F.R.S., government astronomer of New South Wales since 1870, at the age of seventy-one years.

THE New York assembly on March 5 passed the Young bill, which provides for the establishment of a nautical museum and observatory in Bronx Park, New York.

PRESIDENT ROOSEVELT on March 2 signed proclamations creating or increasing thirty-two forest reserves in various western states. The agricultural appropriation bill, then pending, contained a provision that such reserves should not be established except by act of congress. The president believed that if such a law were made, important timber lands would be largely dissipated before congress had an opportunity to consider the matter, while under the action taken they will be preserved. In a memorandum he says that these reserves were determined on and the preparation of the necessary papers ordered some months ago—in two thirds of the cases some years ago.

AN expedition from the Desert Laboratory of the Carnegie Institution of Washington conducted by Dr. D. T. MacDougal has recently circumnavigated the Salton Sea in a sail boat. This lake was found to have a length of over fifty miles and an area of nearly seven hundred square miles. Although the break in the banks of the Colorado River from which the main channel leads to the lake was closed on February 10, yet minor channels and seepage poured sufficient water in the lake to maintain its level during the month following at the maximum depth. It is therefore to be expected that the present level may be retained for some time, and that the evaporation may not exceed the inflow by as much as fifty inches during the present year. Five stations were located and surveyed for the study of the reoccupation of the basin by vegetation as the lake recedes. A second sunken basin south of the Salton and lying between the Cucopa Mountains and the main range of Baja California in Mexico was traversed by wagon, skirting the shores of a lake which partially fills it. This basin seems to be subject to more frequent overflow from the delta, and during 1905 a lake thirty miles long and fifteen wide was formed, which has now shrunken to a third of the dimensions given. Some important material and data bearing upon the plants and animals living in saline waters, hot springs and clay deserts were obtained.

THE Seventh International Zoological Congress, to be held in America, has established a section of heredity. Dr. Chas. B. Davenport, Cold Spring Harbor, L. I., N. Y., as secretary of this section, solicits from investigators in the subject, titles of papers which they would like to present to the section.

THE next meeting of the American Electrochemical Society will be held at the University of Pennsylvania on May 2, 3 and 4, under the presidency of Mr. Carl Hering.

THE second congress of the International Surgical Society will be held in Brussels in September, 1908, under the presidency of Professor Czerny of Heidelberg.

ACCORDING to press dispatches the Argentine Antarctic ship *Uruguay,* which left Buenos Ayres on January 29, 1906, has arrived at Scotia Bay, South Orkney Islands, after a perilous voyage, during which she encountered hundreds of icebergs and heavy pack-ice and sustained considerable damage. The Scotia Bay station was established by the Scottish Antarctic expedition of five years ago, and was taken over in 1905 by the Argentine Government, which is continuing the meteorological and magnetic observations begun by the Scottish expedition.

The Journal of the American Medical Association states that it is almost settled that the Ontario Medical Library Association, the Toronto Clinical Society, the Toronto Medical Society and the Toronto Pathological Society will merge into the Academy of Medicine, Toronto. The academy will be managed by a council of twelve, and the first council will be composed of the officials of the Ontario Library Association and the presidents and secretaries of the other three societies. These will choose their own officers, while succeeding councils will be composed of nineteen members. The academy has a house in Queen's Park, and a library and auditorium to cost $50,000 will be added. Dr. J. F. W. Ross is president of the Ontario Medical Library Association; Dr. H. B. Anderson, of the Toronto Clinical Society; Dr. R. D. Rudolf, of the Toronto Medical Society, and Dr. J. A. Amyot, of the Toronto Pathological Society.

UNIVERSITY AND EDUCATIONAL NEWS

Columbia University has received a gift of $30,000 from an anonymous source to be used for pressing needs of the university.

Mr. Andrew Carnegie has offered $50,000 toward the building fund of the McGill University College of British Columbia, conditional on a similar sum being collected.

Announcement is made that a graduate school of agriculture will be conducted at Cornell during the summer of 1908, under the management of the Association of American Agricultural Colleges and Experiment Stations. Sessions were held at the Ohio State University in 1904, and at the University of Illinois in 1906.

Various bills have been introduced in the Massachusetts legislature, which would tax the residences of college and university professors owned by the institutions. One of these bills provides for reimbursing the town or city by the state. President Eliot and other representatives of universities and colleges were expected to appear before the legislative committee on taxation on March 13.

A course in anthropology was offered at Minnesota for the first time last autumn by Professor A. E. Jenks, then beginning work in the department of sociology, and it was elected by six students. The course was offered again the second semester, beginning on February 4, and has been elected by eighty-three students. An advanced course was also offered the second semester which has been elected by eighteen students.

The trustees of Columbia University at their last meeting promoted a number of officers of instruction, including the following: From adjunct professor to professor—Earl B. Lovell, civil engineering; Henry C. Sherman, chemistry. From lecturer to professor—Dickinson S. Miller, philosophy. From instructor to adjunct professor—William Campbell, metallography; William P. Montague, philosophy; James H. McGregor, zoology. From tutor to instructor—Tracy E. Hazen, botany; Floyd J. Metzger, chemistry; Charles P. Berkey, geology; Bergen Davis, physics; Cavalier H. Jouet, chemistry; Victor J. Chambers, chemistry. From lecturer to tutor—Samuel R. Williams, physics; Pauline H. Dederer, zoology. From assistant to tutor—William N. Berg, physiological chemistry; Marion E. Latham, botany.

Mr. William Spens, B.A., has been elected director of natural science studies and fellow of Corpus Christi College, Cambridge.

Professor Josef Pompeckj, recently called to Königsberg, has now accepted the chair of geology and mineralogy at Göttingen, as successor of Professor von Koenen.

Errata: In Science xxv., p. 293, second column, the fourth word of line 12 should be *spikelets*, the fifth word *ligule*. In line 18, the first word should be *Commutata*.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

FRIDAY, MARCH 22, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

CLOCKS—ANCIENT AND MODERN[1]

Mr. Chairman, Ladies and Gentlemen:

To those who have never had an opportunity to acquaint themselves with the history of the development of the modern clock, I wish to say that no attempt will be made to trace out that development. Time will not permit, as those familiar with the subject well know. I only wish to call to your attention a few points that I hope may be of general interest.

In Nepal, a small independent state situated on the northeastern frontier of Hindustan, there is still practised what is one of the crudest and probably earliest methods of measuring time. A copper vessel with a small hole in the bottom floats on water, sinks and fills sixty times a day. Each time it fills a gong, or ghari, is struck, in progressive numbers from dawn to noon; after noon the first gong struck indicates the number of gharis which remain of the day until sunset. Day is considered to begin when the tiles of a house can be counted or when the hairs on the back of a man's hand can be discerned against the sky. The day is divided into 60 gharis of 24 minutes each, each ghari into 60 palas and each pala into 60 bipalas.

Leaving India for Sumatra and stepping aboard a Malay proa we should there find, floating in a bucket of water, a cocoanut shell having a small perforation, through

[1] Address of the retiring vice-president of Section A, American Association for the Advancement of Science.

which the water by slow degrees finds its way into the interior. When the shell fills and sinks, the man on watch calls the time and sets it afloat again.

The gradual development through untold ages of the floating copper vessel has given us our finest astronomical clocks of to-day, and similarly the floating cocoanut shell may be considered the simplest form of a marine chronometer.

If now we visit the Chinese Empire we find in use there also the water clock, but instead of the water flowing into a vessel through a small hole in the bottom, it flows out of the hole. In attempting to calibrate such a vessel so that any given portion of the whole time required to empty it might be determined, it is at once noticed that when the vessel is nearly full the water flows out more rapidly than when it is nearly empty. The difficulty here presented is most easily overcome by keeping the vessel filled with water when the flow will be uniform, and catching the discharged water in a cylindrical vessel in which the surface of the water will rise equal distances in equal times. Chinese writers ascribe the invention of this instrument, called a clepsydra, to Hwang-ti, who lived more than twenty-five centuries before our era. About fourteen centuries later Duke Chau introduced a float upon the surface of the water in the final cylinder by means of which an indicator was made to travel over an adjacent scale as the water rose in the cylinder, thus allowing the indications of the instrument to be perceived at a greater distance.

These instruments that have just been described may be called artificial timekeepers, and are used primarily to subdivide the day, while the sundial of equal antiquity with the others may be called a natural timekeeper, as it gives a means of determining day after day a particular time of day, such as apparent noon.

The earliest mention of a sundial is found in Isaiah 38: 8, in connection with the promise of the Lord to add fifteen years to the life of Hezekiah who lived about 2,600 years ago. "And this shall be a sign unto thee from the Lord, that the Lord will do this thing that he hath spoken; behold, I will bring again the shadow of the degrees, which is gone down in the sundial of Ahaz, ten degrees backward. So the sun returned ten degrees, by which degrees it was gone down."

Of the nature of this sundial nothing is said, nor is there found any description of such an instrument until 350 years later, when we find the sundial of the Chaldean priest Berosus, who lived in the time of Alexander the Great and his immediate successors.

This consisted of a hollow hemisphere placed with its rim perfectly horizontal, and having a bead fixed at its center. So long as the sun remained above the horizon the shadow of the bead would fall on the inside of the hemisphere, and the path of the shadow during the day would be approximately a circular arc. This arc, divided into twelve equal parts, determined twelve equal intervals of time for that day. Now supposing this were done at the time of the solstices and equinoxes, and on as many intermediate days as might be considered sufficient, and then curve lines drawn through the corresponding points of division of the different arcs, the shadow of the bead falling on one of these curve lines would mark a division of time for that day, and thus we should have a sundial which would divide each period of daylight into twelve equal parts. These equal parts were called *temporary hours;* and since the duration of daylight varies from day to day, the temporary hours of one day would differ in length from those of another. Dials of this form were still used by the Arabians a thousand years ago, and about 1750 four such were found in Italy.

The introduction of the sundial into Greece is generally fixed by historians in the latter part of the sixth century B.C. At that time the instrument seems to have been a very crude one, consisting merely of a pillar without any graduated dial by means of which the day could be divided into a number of equal parts. The length of the shadow determined the time for certain regular daily duties, as a shadow six feet long might indicate the time for bathing, and one twelve feet long that for supper.

As civilization advanced and the needs of the people required more accurate measures of time, the sundial was developed. We have already mentioned the sundial of Berosus, about 350 B.C., by means of which each day from sunrise to sunset was divided into twelve equal parts, that is provided the sun was visible all day long.

As the sundial could not be used indoors or on cloudy days, as soon as the Grecian life became complex enough to need a timepiece under such circumstances, this demand was met by the clepsydra. The time of its introduction is not definitely known, but the familiar references of Aristophanes show its use to have been common about 430 B.C., while no mention is made of it by Herodotus, whose history ends fifty years earlier.

A passage in Aristotle gives some idea of the early form of the clepsydra: it was a spherical bottle with a minute opening at the bottom and a short neck at the top into which the water was poured, and by closing which the flow of the water could be stopped. This form of clepsydra became a necessary adjunct of all courts of justice, and the number of gallons of water that a lawyer was allowed for his speech gave some indication of the importance of the trial. In fact, the word ὕδωρ became a synonym for time. We find Demosthenes charging his opponent with talking 'in my water'; and on another occasion he shows the value he attached to the time allotted him by turning to the officer, when interrupted, with a peremptory 'You there! stop the water!' reminding one of the 'take out time' of our modern football contests.

The first timepiece of the Romans seems to have been the watchman, who, from the Senate House, called forth noon as soon as he caught sight of the sun between the Rostra and the Græco-Statis. From the same point he watched the declining sun and proclaimed its disappearance. This custom was probably instituted towards the close of the fifth century B.C. According to Pliny the first sundial was set up in Rome about 290 B.C. About thirty years later Consul Valerius Messala erected at Rome a sundial which was captured at Catania in Sicily. This instrument was not a mere gnomen such as was introduced three centuries earlier into Greece, but was the result of Grecian science and genius, constructed for a particular latitude, that of Catania 5° south of Rome. For a hundred years this sundial supplied the needs of the Romans for a timepiece, although it was constantly in error. Finally, in 164 B.C., Marcius Phillippus set up near the dial of Catania, one constructed for the latitude of Rome, and Rome then possessed her first accurate timepiece; and during the time of Plautus the use of sundials became common, as is evidenced by the following quotation:

When I was young, no time-piece Rome supplied,
But every fellow had his own—inside;
A trusty horologe, that—rain or shine—
Ne'er failed to warn him of the hour—to dine,
Then sturdy Romans sauntered through the Forum,
Fat, hale, content; for trouble ne'er came o'er them,

But *now* these cursed dials show their faces,
 All over Rome in streets and public places;
And men, to know the hour, the cold stone question,
 That has no heart, no stomach, no digestion,
They watch the creeping shadows—daily thinner—
 Shadows themselves, impatient for their dinner.
Give me the good old time-piece, if you please,
 Confound the villain that invented these!

As in Greece so in Rome, the clepsydra followed in the wake of the sundial, and as in the case of the sundial, Grecian science and genius had by this time produced a much more perfect instrument than that first used by the Greeks.

In describing the sundial of Berosus, I stated that by it the day was divided from sunrise to sunset into twelve parts, varying in lengths from day to day, called temporary hours. If now the clepsydra and the sundial were to read alike, it was necessary that the hours recorded by the clepsydra should also change from day to day. Various devices were adopted to accomplish this. Further, as the clepsydra could be used throughout the entire day and night, it was necessary to have it record hours of a different length at night from what it did in the day, as each night, *i. e.*, from sunset to sunrise, was divided into twelve hours as well as each day.

Fig. 1 represents an early form of the clepsydra. It consists of an inverted cone *A*, with a small aperture at its vertex. The water is supplied through the pipe *H* and is prevented from rising above a fixed level in *A* by the waste pipe *I*, which carries off the superfluous water. Thus there will be a uniform flow of water from the vertex of the cone into the cylindrical vessel and the cork *F* will rise uniformly, communicating its motion through the rod *E* to the hand *G*, which indicates the hours on the dial. In order to produce a change in the rate of flow of the water from the conical vessel, as is necessary in having the hand indicate hours of different length from day to day, a solid cone *B*, similar to *A*, was plunged into the hollow one, and its position for any given day or night was indicated by the coincidence of a particular one of the adjusting marks on the stem *D* with the top cross-piece of the frame *C*.

A later form of clepsydra, attributed to Ctesibius, who lived during the latter part of the third century B.C., is shown in Fig. 2. *A* is the end of a tube over which an image stands, which is connected with a full reservoir, and from the eyes of which, considered as invariable apertures, the water continually flows or drops in a regulated manner into it; this tube conveys the water into

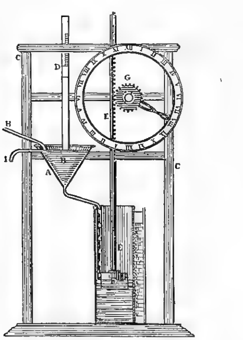


Fig. 1. Early Form of Clepsydra.

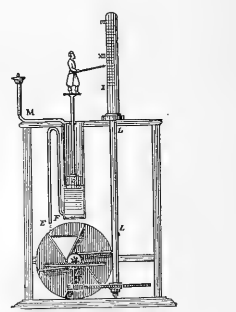


Fig. 2. Clepsydra of Ctesibius.

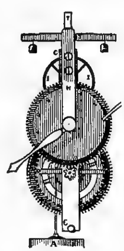
the top of the cylindrical vessel *BCDF*, which gradually fills, the cork *D*, with its attached pillar *C*, rising at the same time. On the top of this pillar is an image holding an index which points to the divisions on the large column above. Now when the water rises in the cylindrical vessel it also rises in the small tube *FB*, which constitutes one leg of a syphon; consequently, when the small tube fills and the index has mounted to the top of the large column, the water flows over the bent part *B* of the syphon and the vessel is emptied into one of the six troughs of the waterwheel *K*, which is thus turned one sixth of a revolution, and the column, by means of the gearing shown in the figure, 1/365 of a revolution, during which time the image falls with its index to the bottom of the column, to be ready for the next day. There are ruled around the large column twenty-five lines properly sloped with respect to each other, so that the index may indicate the hour on any day of the year.

For the next fifteen centuries or more after the introduction of this clepsydra of Ctesibius, sundials and clepsydras were the timekeepers of mankind. These were then gradually supplanted by weight clocks.

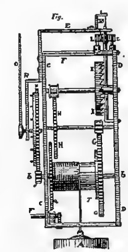
I am not able to give the date of the manufacture of the first clock composed of an assemblage of wheels actuated by a suspended gravitating body and controlled by a regulator of some determined shape and dimensions, *i. e.*, a regulated machine for measuring time without the assistance of water.

There may be seen at the South Kensington Museum an old clock movement, now controlled by a pendulum, which was removed about seventy years ago from a clock in Wells Cathedral and which is said to have been built by Peter Lightfoot, about 1335, for the church of Glastonbury Abbey, from which it was removed to Wells Cathedral in the reign of Henry VIII., where, I believe, the dial and its accessories may now be seen. In addition to indicating the time, this clock was intended to indicate the age of the moon and to show its phase. Also it caused a man to strike the quarter hours with his feet on two little bells, and the hours on another bell before him with a battle-axe that is in his hand. It also set in motion four equestrian knights equipped for a tournament. If the date given for the clock is correct the figures operated in connection with it are hardly the original ones. One of the knights is painted in the civil costume of the seventeenth century.

About 1360 there was erected in the tower of the palace of Charles V. of France a clock constructed by Henry de Vick, of Würtemberg. This clock was made for the simple purpose of telling the time of day and was not expected to perform the numerous feats that were so frequently required of clocks in earlier days, and may be taken as a type of the earliest clock movements.



Front view.



Side view.

FIG. 3. Ancient Clock by Henry de Vick.

From Fig. 3, giving both a front and a side view, the operation of the clock may be clearly understood. The motion of the falling weight *A* is transmitted by means of the wheels *G*, *e*, *H* and *g* to the wheel *I*; this last wheel, by giving the pallets or short levers, *h*, *i*, each a push alternately by two teeth, at opposite sides of its circumference, and moving in opposite directions, one forward and the other backward, gives a vibratory motion to the vertical arbor K, and as the regulator or balance was fixed on this arbor, it was thus made to vibrate backwards and forwards at every push of the escapement wheel upon the pallets, the period of the vibration being regulated by the position of the small weights m, m on its arms. Thus the whole duration of a vibration was the measure of time, and the wheels and pinions were employed first to transmit the maintaining power to the balance, and secondly to number the vibrations and indicate them in visible form by a hand O on a dial plate.

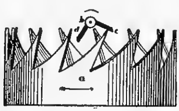


Fig. 4. View of de Vick's Escapement Wheel from above.

Fig. 4 gives a view of the escapement wheel looking from above. The pallets were placed at about 90° from each other on the arbor or verge of the balance, so that when one of them was parting with its tooth of the escapement wheel, the other was in a situation to receive the opposite one immediately, but the motion of the verge will not be at once reversed. The escape wheel will recoil until the impetus of the balance is exhausted.

The substitution of the main spring for a large heavy body as a first mover constituted a second era in modern horology, from which we may date the origin of the fusee, or mechanism for equalizing the variable power of a coiled spring.

While the date at which the first portable clock was made may not be definitely stated, it was certainly as early as 1525, as the Society of Antiquaries in England has in its possession one made in that year by Jacob Zech at Prague, the inventor of the fusee. Its construction differs materially from that of De Vick's clock only in that it has a spiral spring with a fusee instead of the driving weight.

Such was the state of clockwork when Galileo, the celebrated philosopher and mathematician, while watching the vibrations of the great bronze lamp swinging from the roof of the cathedral of Pisa in 1583 observed that, whatever the range of its oscillations, they were invariably executed in equal times. Because of this isochronal property of a vibrating suspended body the pendulum was introduced as the regulator of clockwork, thus inaugurating the third era in the development of the modern clock.

The honor of being the first to apply a pendulum as a regulator of clockwork is claimed for several clock-makers.

The earliest of these is, I believe, Richard Harris, who is said to have made a pendulum clock for St. Paul's Church, Covent Garden, in 1641. Vincent Galileo claims to have applied his father's discovery to the construction of a pendulum clock in 1649, but does not seem to have made the fact public until after Huyghens in 1657 presented to the States of Holland a clock controlled by a pendulum, claiming for himself the invention of this form of control. Certain it is that Huyghens gave much study to the mathematical theory of the pendulum, and proved that in order that pendulum vibrations of different lengths should be strictly isochronal, the pendulum should vibrate between cycloidal checks. Such an arrangement he introduced into his clock of 1657, which also contained the famous Huyghens loop in connection with the winding apparatus.

With the verge escapement, Fig. 4, the one in use when the pendulum was applied to clockwork, and which required a long arc of vibration for the escapement of the escapement wheel, the deviation of the circular arc described by an ordinary pendulum from the theoretical cycloidal arc was, of necessity, taken into account. With the introduction of the anchor or recoil escapement, Fig. 5, invented by Dr. Hooke in about 1675, the long swing of the pendulum was obviated and the cycloidal cheeks were found to be more detrimental than advantageous.

The expansion of metals by heat has been known ever since the middle of the seventeenth century, and early in the eighteenth George Graham set himself the task of making a clock pendulum such that the distance between the center of oscillation and the center of suspension would be independent of the temperature. I quote from his paper 'A Contrivance to avoid Irregularities in a Clock's Motion by the Action of Heat and Cold upon the Pendulum,' communicated to the Royal Society in 1726:

> Whereas several who have been curious in measuring time have taken notice that the vibrations of a pendulum are slower in summer than in winter, and have very justly supposed this alteration has proceeded from a change of length in the pendulum itself, by the influence of heat and cold upon it, in the different seasons of the year; with a view, therefore, of correcting, in some degree, this defect of the pendulum, I made several trials, about the year 1715, to discover whether there was any considerable difference of expansion between brass, steel, iron, copper, silver, etc., when exposed to the same degree of heat as nearly as I could determine, conceiving it would not be very difficult, by making use of two sorts of metals, differing considerably in their degrees of expansion and contraction, to remedy, in great measure, the irregularities to which common pendulums are subject. But although it is easily discoverable that all these metals suffer a sensible alteration of their dimensions by heat and cold, yet I found their differences in quantity from one another were so small, as gave me no hopes of succeeding this way, and made me leave off prosecuting this affair any further at that time. In the beginning of December, 1721, having occasion for an exact level, besides other materials I made trial of, quicksilver was one; which, although I found it was by no means proper for a level, yet the extraordinary degree of expansion that I observed in it when placed near the fire, beyond what I had conceived to be in so dense a fluid, immediately suggested to me the use that might be made of it by applying it to a pendulum. In a few days after I made the experiment, but with much too long a column of quicksilver, the clock going slower with an increase of cold, contrary to the common pendulum; however, it was a great confirmation of the advantage to be expected from it, since it was easy to shorten the column in any degree required.

As his first jar was too long, so his second was too short, but by June 9, 1722, he was ready to test the running of his mercury pendulum clock with one regulated by an uncompensated pendulum. He says:

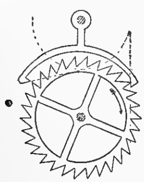
> For the first year I wrote down every day the difference between the two clocks, with the height of the thermometer, not omitting the transits of the stars as often as it was clear. The result of all the observations was this, that the irregularity of the clock with the quicksilver pendulum, compared with the transits of the stars, exceeded not, when greatest, a sixth part of that of the other clock with the common pendulum; but for the greatest part of the year, not above an eighth or ninth part, and even this quantity would have been lessened had the pillar of mercury been a little shorter, for it differed a little the contrary way from the other clock, going faster with heat, and slower with cold; but I made no alteration in length to avoid an interruption of the observations.

A few years afterward John Harrison brought out his gridiron pendulum composed of four brass and five steel rods, so constructed that the expansion of the steel rods tended to lower the pendulum bob while the expansion of the brass ones tended to raise it.

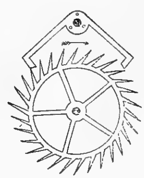
Another form of compensated pendulum that has found favor is the zinc-tube pendulum, in which the zinc tube surrounds a rod of steel and is itself surrounded by a tube of steel. Here the zinc tube fulfils the purpose of the brass rods in the gridiron pendulum. At present at least one firm of clock manufacturers is using invar or nickel steel for its pendulum rods.

Attempts have also been made to compensate a pendulum to avoid changes in the rate due to changes of the barometer. At Greenwich this is accomplished by attaching a magnet to the end of the pendulum and causing a second magnet supported below the pendulum to approach or recede from the pendulum with changes of the barometer. This second magnet increases the acceleration due to gravity to a greater or less degree as it approaches or recedes from the pendulum.

The most satisfactory way, however, of freeing a clock rate from variations due to changes of temperature and pressure is to enclose the clock in an air-tight glass case kept in a room of constant temperature. A number of observatories now keep their standard clocks under such conditions.



Anchor or Recoil Escapement.



Dead-beat Escapement.

FIG. 5.

Another improvement in the manufacture of clocks was accomplished by Graham in the introduction of the dead-beat escapement.

In Fig. 5 are shown both the anchor or recoil escapement and the dead-beat escapement. In the first, shown on the left, the pendulum moving to the left has just escaped a tooth at the left-hand pallet and allowed a tooth to fall on the right-hand one. The pendulum, however, still continues its swing to the left, and in consequence the pallet pushes the wheel back, thus causing the recoil that gives the name to the escapement. It is only after the pendulum comes to rest and begins its excursion the other way that it gets any assistance from the wheel, and the difference between the forward motion of the wheel and its recoil forms the impulse. In the right-hand figure, the pendulum moving to the right has just escaped a tooth from the right-hand pallet while another has fallen upon the left-hand one. As the pendulum continues its motion towards the right, the left hand-pallet slides over the point of the tooth, but there is no recoil, as the 'dead' face, as it is called, is the arc of a circle whose center is the point about which the anchor turns. As the pendulum returns towards the left, the tooth traverses the 'dead' face in the opposite direction, and immediately upon leaving this face it passes to the 'impulse' face, and while passing along this face gives the impulse to the pendulum.

The great advantage of the dead-beat escapement over the anchor or recoil type is that, although a slight increase of force on the escapement wheel increases the arc of the pendulum, it does not sensibly increase the time, while the time does sensibly increase with the recoil escapement.

At about this time also, Mudge introduced the gravity escapement. With all the previous escapements the impulse was given to the pendulum by the driving weight acting through the train so that any irregularities in the train would cause a variation in the impulse. With the gravity escapement a weight is raised by the train and the falling of this weight gives the impulse to the pendulum. We thus have a uniform impulse at each oscillation due to the falling of a weight through a fixed distance. Simple and elegant as is this theory, the application of it gave a great deal of trouble and all gravity escapements were regarded with suspicion, as having a tendency to trip, until Mr. Denison designed the double three-leg one for the great clock at the Houses of Parliament about fifty years ago. Incidentally, it might be mentioned that this Westminster clock has turned out to be the finest timekeeper of any public clock in the world. The original specifications required that the clock should be guaranteed to perform within a margin of a minute a week, which caused the leading clock-makers of England to decline to bid for the work. However, under Mr. Denison's supervision the clock was built by Mr. Dent, and from reports of the Astronomer Royal, who receives at Greenwich two signals a day from this clock, sent automatically, its error is rarely over a second a week.

The Denison gravity escapement is shown in Fig. 6. This escapement con-

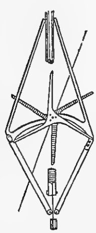


FIG. 6. Denison Gravity Escapement.

sists of two gravity-impulse pallets pivoted as nearly as possible in a line with the bending point of the pendulum spring and touching the pendulum near the bottom of the figure. The locking wheel is made up of two thin plates having three long teeth or 'legs' each. These two plates are squared on the arbor a little distance apart, one on each side of the pallets. Between them are three pins which lift the pallets. In the figure, one of the front legs is resting on a block screwed to the front of the right-hand pallet. There is a similar block screwed to the back of the left-hand pallet for the legs of the back-plate, which is shaded in the figure, to lock upon. Projecting inward from each of the pallets is an arm. The tip of the one on the right-hand pallet is just in contact with one of the pins which has lifted the pallet to the position shown. The pendulum is traveling in the direction indicated by the arrow, and the left-hand pallet has just given impulse. The pendulum rod in its swing will push the right-hand pallet far enough for the leg of the front locking plate, which is now resting on the block, to escape, Directly it escapes, the left-hand pallet is lifted free of the pendulum rod by the lowest of the three pins. After the locking wheel has passed through 60°, a leg of the back locking plate is caught by the locking block on the left-hand pallet. As the three-leaved pinion always lifts the pallets the same distance, the pallets in returning give a constant impulse to the pendulum.

About fifteen or twenty years ago the Rieflers, clockmakers of Munich, introduced into their clocks an escapement in which the impulse is communicated to the pendulum through the suspension spring. The pendulum is supported by a rocking frame to which is attached the anchor carrying the pallets which are acted upon by the escapement wheel. Just after the pendulum has passed through its vertical position, the escapement wheel, when released, gives to the supporting frame of the pendulum suspension spring a slight tilt in the opposite direction from that in which the pendulum is moving, thus increasing the tension of the spring due to the swing of the pendulum to one side.

In the next few minutes I wish to consider the accuracy with which our astronomical clocks perform their function.

The earliest star catalogue of precision is that of Bradley and the observations upon which it is based were made about a third of a century after the introduction of the compensated pendulum. In discussing the performance of his clock, I have used the adopted rates as given by Auwers in his rereduction of Bradley. The monthly means of the rates from July, 1758, to July, 1759, were taken and the difference of each rate from its monthly mean. Then the mean of these differences without regard to sign was taken for each month.

VARIATION OF DAILY RATE OF THREE GREENWICH SIDEREAL CLOCKS

Bradley's Clock			Sidereal Stand. "Hardy"			Sidereal Stand. "Dent"		
Date	Daily Rate	Mean Residual	Date	Daily Rate	Mean Residual	Date	Daily Rate	Mean Residual
1758	s	s	1850	s	s	1900	s	s
July	−0.086	0.060	January	+0.666	0.269	January	+0.280	0.035
August	+0.012	0.079	February	+0.857	0.162	February	+0.196	0.041
September	−0.012	0.077	March	+0.930	0.117	March	+0.170	0.038
October	−0.130	0.166	April	+1.154	0.257	April	+0.134	0.027
November	−0.094	0.127	May	+1.607	0.105	May	+0.078	0.028
December	−0.190	0.096	June	+1.372	0.117	June	+0.022	0.045
1759								
January	−0.264	0.106	July	+1.058	0.202	July	−0.096	0.039
February	−0.343	0.072	August	+0.821	0.104	August	−0.245	0.053
March	−0.449	0.108	September	+1.003	0.087	September	−0.381	0.056
April	−0.310	0.125	October	+1.049	0.120	October	−0.549	0.048
May	−0.322	0.127	November	+1.232	0.063	November	−0.522	0.161
June	−0.439	0.086	December	+1.389	0.187	December	−0.259	0.036
Mean		0.102	Mean		0.149	Mean		0.051

The rates of two other clocks of the Greenwich Observatory were likewise discussed, the standard clock for the year 1850 and that for 1900, the adopted daily rates as published in the annual volumes being used. The first of these was kept in the observing room and thereby subjected to large variations of temperature, while the second, made in 1871 by E. Dent and Company, was fixed to the north wall of the magnetic basement, as in this apartment the temperature is kept nearly uniform. The pendulum of this latter clock is provided with barometric as well as thermometric compensation.

I sought to make a similar comparison for the clocks of the other large observatories, but soon found that the information concerning the performance of the various clocks was given in such a form that it was in almost every case either impossible or extremely laborious to secure the desired result. However, there are two well-known clocks which should be mentioned in this connection, and in conclusion I will give some hitherto unpublished data concerning the clock with which I have been working during the past three years.

Probably no clock has had its rate more thoroughly discussed than Hohwü No. 17, the standard clock of the observatory at Leiden.

It was set up in the transit room in 1861 and in December, 1898, was removed to the large hall of the observatory, where, enclosed in two wooden cases, it was placed in a niche cut in the pier of the ten-inch refractor. To further guard against sudden changes of temperature the niche is closed by a glass door. At the meeting of the Royal Academy of Sciences at Amsterdam, held September 27, 1902, Dr. E. F. van de Sande Bakhuyzen submitted the following formula as the best representation of the daily rate of the clock:

$$\text{Daily rate} = -0^s.173 + 0^s.0157\,(h-760^{mm}) - 0^s.0253\,(t-10^\circ) + 0^s.00074\,(t-10^\circ)^2 + 0^s.0465 \cos 2\pi(T-\text{May } 3)/365$$

and gave the result of a comparison of the observed daily rates 1899–1902, the average interval of time for each rate being six days, with those computed by means of the above formula. I find during the year 1900 the mean of these difference is $0^s.028$ and the largest difference is $0^s.071$.

About 1867 F. Tiede installed at the Berlin Observatory a weight-driven clock enclosed in an air-tight case. The original escapement was replaced in 1876 by a gravity escapement and the clock has continued to give satisfaction, certainly up until 1902, when it was dismounted for cleaning. The only published rates that I have been able to secure are those during twelve weeks in 1877–8. During this period the average deviation of the observed daily rates, the average interval for each rate being six days, from the mean daily rate for the entire period is $0^s.030$.

In connection with the publication of these rates[2] the statement is made that for weeks at a time this mean deviation will not exceed $0^s.02$.

DAILY RATE OF RIEFLER SIDEREAL CLOCK NO. 70.

Date	Daily Rate	Mean Barometer	Mean Temp.	Computed Rate	O—C.
1904	s.	mm.	°	s.	s.
Feb. 8–11	+0.019	631.0	28.3 C.	+0.009	+0.010
11–15	—0.014	631.5	28.5	—0.006	—0.008
15–20	+0.005	631.5	28.3	—0.002	+0.007
Mar. 1– 4	—0.026	631.0	28.2	—0.012	—0.014
4– 9	—0.010	631.0	28.2	—0.016	+0.006
9–16	—0.022	631.5	28.1	—0.018	—0.004
16–18	—0.043	631.0	28.1	—0.022	—0.021
18–22	—0.022	631.0	28.0	—0.021	—0.001
22–25	—0.029	631.0	28.0	—0.024	—0.005
25–28	+0.002	631.0	27.7	—0.014	+0.016
28–34	—0.007	631.0	27.7	—0.018	+0.011
Apr. 3– 5	+0.017	631.0	27.4	—0.009	+0.026
5–13	+0.002	631.0	26.9	+0.014	—0.012
13–16	+0.026	631.0	26.5	+0.021	+0.005
16–19	+0.034	631.0	26.3	+0.027	+0.007
19–22	+0.002	631.0	26.4	+0.020	—0.018
22–31	+0.029	631.0	25.0	+0.077	—0.048
May 1– 4	+0.113	631.5	24.3	+0.103	+0.010
4– 7	+0.082	631.0	24.1	+0.109	—0.027
7–12	+0.161	631.0	24.0	+0.109	+0.052
					Mean ±0.015

In 1903 there was installed at the Naval Observatory one of Riefler's clocks, No. 70, with a nickel-steel pendulum, the impulse being communicated to the pendulum through the suspension spring. This clock was enclosed in an air-tight glass case and was mounted in a vault where the temperature was artificially controlled. The definitive rates have been determined from September, 1903, to May, 1904, but, unfortunately, during this entire period we were unable to prevent the glass case leaking and there was a variation of temperature in the vault of about 5° C. However, from the first of February to the middle of May the pressure was kept nearly constant by reducing it each day by means of an air pump by from 1 to 2 millimeters. During this period the following results were obtained:

The formula from which the computed rate is obtained,

$$\text{Daily rate} = +0^s.0161 - 0^s.00103\,(T\text{–Mar. }29.0) - 0^s.0456\,(t - 27^\circ.0)$$

is the result of a least square solution of the twenty observed rates.

Collecting together the results given above, we have:

MEAN DEVIATION OF DAILY CLOCK RATE.

Clock	Date	Mean Deviation
Bradley	1759	0.102
Greenwich Observatory	1850	0.149
Greenwich Observatory	1900	0.051
Berlin Observatory	1877	0.02–0.03
Leiden Observatory	1900	0.028
U. S. Naval Observatory	1904	0.015

In considering this table I hope the particular method by which each of the numbers given was obtained will be remembered. The reason for using different methods has already been given. If the rates of the first three clocks had been treated in a manner similar to that used in the last three cases, the numbers given might have been slightly smaller, and if the period under consideration with the Berlin and Washington clocks had been

[2] A. N. Nr. 2184.

longer the two corresponding residuals might have been slightly larger.

W. S. Eichelberger

U. S. Naval Observatory

THE NEW YORK MEETING OF SECTION C OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE AND THE THIRTY-FIFTH GENERAL MEETING OF THE AMERICAN CHEMICAL SOCIETY—II.

BIOLOGICAL CHEMISTRY

Wm. J. Gies, Chairman

The Rational Conversion of Energy: J. E. Siebel, Jr.

The Thermodynamics of Nutrition: J. E. Siebel, Jr.

The Effects of Magnesium Sulphate upon Seedlings: Gertrude Burlingham.

From extended experiments upon the growth of seedlings in dilute solutions of magnesium sulphate, it was found that while it is usually toxic in strengths greater than $M/8192$ (0.003 per cent.), it produces decided stimulation in $M/16384$, reaches a maximum stimulation at dilutions from $M/32768$ to $M/131072$ (0.00075 per cent. to 0.00018 per cent.), then beyond this point gradually diminishes in action. The point of toxicity and of greatest stimulation varies with the type of seedling. Both the plumule and the roots attain greater growth in these favorable dilutions of magnesium sulphate and in the control of distilled water, and often the lateral roots develop two or three days sooner. While growth in the control practically stops at the end of one week, it continues from four to five weeks in the magnesium sulphate cultures. Two parallel series, one magnesium sulphate and control, the other calcium nitrate and control, in dilutions from $M/8192$, $M/16384$, etc., to $M/262144$, showed that calcium ceases to be stimulating in the dilution in which magnesium loses its toxicity and produces maximum stimulation; in $M/32768$ magnesium sulphate the root growth is eight times that in calcium nitrate. In every instance after the renewal of the solutions, growth was accelerated in the magnesium cultures, while there was little change in the control. Seedlings allowed to grow for several weeks in a dilution of magnesium sulphate which was at first slightly toxic, finally developed strong lateral roots and attained a root growth far beyond the control. These results show conclusively that magnesium sulphate in proper dilution is beneficial to the growth of seedlings, and that any inhibitory effects are due to the presence of excessive amounts, thus controverting Loew's theory that magnesium salts when alone in solution are always injurious to plant growth.

Chronic Arsenical Poisoning and the Distribution of Arsenic in the Animal Organism: Wm. D. Harkins.

In copper smelting regions arsenic is likely to be a constituent of grass, hay, and all the organs of animals. Grass has been found to contain as high as 1,500 parts of arsenic trioxide, and the ulcers in the noses of horses as high as 1,000 parts per million. 0.362 Gram of arsenic trioxide killed a sheep in eight days, 0.123 gram of sodium arsenite killed a sheep in thirty-three days with thirty-one pounds loss of weight, and 0.055 gram in sixty-nine days with ten pounds loss in weight.

On Proteose Fever: R. B. Gibson.

Fever has been considered to result from the injection of prepared proteoses and of bacterial endo- and extra-cellular proteose-like substances, especially as albumosuria is often observed in septic and aseptic fevers of experimental or natural origin. As yet no observations have been made with proteoses prepared from highly purified or crystalline proteids. Pepsin-HCl proteoses were prepared as usual, but without subjecting the products to temperatures over 50° C., from repeatedly precipitated caseinogen, recrystallized egg albumin and recrystallized edestin. Proteoses soluble and insoluble in 95 per cent. alcohol were obtained from each. Bromelin and papain caseoses were also employed. The proteoses were injected in doses of 0.3 to 0.7 gm. into guinea-pigs and 0.5 to 1.2 gm. into rabbits; the temperature variations were observed on several days previous to the injection and were controlled by saline injections. The bromelin and papain caseoses and egg albumin proteoses have at times caused slight rise of temperature, while the purest material—the edestinose—has been practically without effects whenever tried. Extraction of the bromelin and papain caseoses with boiling alcohol has lessened the pyrexia, and the products soluble in 95 per cent. alcohol were found non-febrile. The primary cleavage products of pepsin-HCl digestion, when prepared as above, never have more than a slight pyrogenic effect when injected into rabbits and guinea pigs. This effect is so slight that such substances can not be the direct inciting cause of the severe naturally occurring fevers.

The Fractional Precipitation of Antitoxic Serum: E. J. BANZHAF and R. B. GIBSON.

A new practical method for the partial purification and concentration of antitoxin, based on the solubility of antitoxic proteids in saturated NaCl solution. The diluted plasma is precipitated with an equal volume of saturated $(NH_4)_2SO_4$ solution and the antitoxic proteids extracted from the precipitate with saturated NaCl. The method has been used to study further the concentration of antitoxin. Twenty liters of plasma (475 units per c.c.) were diluted with 20 liters of water; by fractioning with $(NH_4)_2SO_4$, the three proteid precipitates were obtained containing 1,150, 1,350 and 1,750 units, respectively, per c.c. The NaCl-soluble (total antitoxic) globulins of these fractions of the plasma were prepared as usual.

A second experiment resulted similarly. All the preparations, when precipitated from the NaCl with acetic acid and dialyzed, probably contained a partially denaturalized antitoxic globulin, which had a diminished solubility and antitoxic potency (per gm.) and was precipitated on slight acidification by great dilution. Further partition (after complete neutralization) of the fractions when freed from the water-acid precipitable proteid yielded filtrates that coagulated at 73° C. The more soluble proteids of the fractions were found to have a relatively higher antitoxic content per gm., the last to be precipitated being at least three to four times stronger than the total NaCl-soluble globulin.

Some Factors Modifying the Output of Endogenous Uric Acid: E. W. ROCKWOOD and C. VAN EPPS.

An attempt was made to determine the effect of some substances commonly used to increase the elimination of uric acid by man; at the same time the nitrogen, phosphoric acid and, in some cases, the creatinine, were determined. The diet was constant and either purine-free or contained a fixed amount of purine derivatives. After it was proved that chocolate did not cause an increase of the uric acid this was included, in one case, in the food. Alkalies and some of their salts whose decomposition products make the urine alkaline were tried. Of these sodium bicarbonate caused no increase; lithium carbonate, a decrease; potassium acetate and sodium citrate, no change. Colchicum slightly decreased the uric acid. With both sodium salicylate and aspirin (the acetic acid ester of salicylic acid) the uric acid of the urine was increased. One subject with chronic albuminuria, but otherwise no marked renal lesions, eliminated no more than the normal amount when aspirin was taken. Creatinine with all these drugs showed a remarkable constancy.

The Properties of Culture Media as affected by Certain Products of Plant Metabolism: Oswald Schreiner and J. F. Breazeale.

It was shown that certain products of plant metabolism, degradation products of proteids and lecithins, were harmful to seedling plants. Some of these products found in green plants lose their toxic properties on oxidation and become beneficial. It was pointed out that this explained the advantages of green manuring over mineral fertilizers and the chemistry of green manuring was discussed in this connection.

On the Increase in Weight in the Hydrolysis of Casein: J. H. Long.

By the digestion of casein and other proteins by action of pepsin and hydrochloric acid an increase in weight follows from the absorption of water and the addition of molecules of the acid to form salt-like bodies with the products of hydrolysis. In this paper the author describes a series of experiments in which casein was treated through a period of about five weeks with pepsin and dilute hydrochloric acid at a temperature of 40°. The digestion mixture was distributed through a series of flasks and from time to time one of these was removed from the thermostat and used for tests. The residue on evaporation was determined and in this the combined hydrochloric acid. The titration values by aid of dimethylaminoazobenzene, phenolphthalein and *p*-nitrophenol were also found. By combination of all these data from the whole series of mixtures an attempt was made to show in tabular form the rate of addition of water and acid throughout the digestive period.

The author called attention also to the value of *p*-nitrophenol as an indicator in the titration of digestion mixtures, an application which seems to have been quite overlooked.

Further Experiments on the Composition of Feces Fat: J. H. Long.

In previous papers by the author attention has been called to the large phosphorus content of the purified fat from the feces of individuals in normal health. This paper presents the results of a lengthy series of experiments in which the characteristics of feces fat are more fully worked out, and especially of the portion which may be described as a body, or mixture of bodies, of the lecithin type. It was found that a large fraction of the fat could be precipitated from alcohol solution by cadmium chloride and from ether solution by acetone. Nitrogen and phosphorus were determined in the fractions precipitated by acetone, and in the fraction not so precipitated. It was found that practically all the phosphorus was contained in the acetone precipitate, while the nitrogen was present in both. A part or all of the nitrogen may have been split off from the original complex, however. The ratio of phosphorus to nitrogen was determined in the original fat, and in the several fractions; also the iodine members, the saponification equivalents and other constants.

The Influence of Alcohol on the Excretion of Inorganic Constituents of the Urine: F. C. Hinkel and Wm. Salant.

Fifty cubic centimeters of 50 per cent. or 70 per cent. alcohol given by mouth to a healthy dog caused a diminution of chlorides—about 22 per cent. The phosphates were diminished about 40 per cent.

Total sulphates were decreased during the alcohol period about 36 per cent. After alcohol was discontinued the urinary chlorides returned to the normal. The phosphates likewise increased in amount, but at the end of ten days the daily total was about 15 per cent. below the daily average for the control period.

A Study of the Effects of Cerium Salts and of Salts of Some Related Metals: George Baehr and Harry Wessler.

Cerium oxalate was administered to dogs in relatively large doses for days, occasionally as much as 50 grams at a dose, without eliciting any symptoms beyond a slight increase in the amount of feces evacuated daily, a symptom following the administration of any inert powder in large quantities. The oxalates of neodymium, praseodymium, lanthanum and thorium, all of which are associated as impurities in commercial cerium oxalate, were also absolutely non-toxic. As to the efficiency of cerium oxalate in alleviating vomiting, when induced artificially by drugs acting either locally on the gastric mucosa or centrally on the medulla, our results up to date are not conclusive enough to permit us to decide, but apparently, except in large doses, it has little or no influence on vomiting. The action of cerium oxalate seems to be simply mechanical and local on the gastric membrane. Cerium nitrate, as a soluble salt, combines readily with protein and when injected hypodermically in dogs, a large ulcer was always formed around the site of injection. Besides this, a few days after the injection intense gastro-intestinal disturbances occurred, such as vomiting and diarrhea which was sometimes very bloody. The autopsies showed an unusual violence of the gastric and intestinal peristalsis and an empty gut, the mucous membrane of which was markedly congested and in places ecchymotic. The assumption is justifiable that at least a large part of the cerium in such cases must be eliminated from the body through the alimentary tract.

The Solubility of Acetanilide, Phenacetine, Caffeine and Salol in Several Organic Solvents: Atherton Seidell.

The author has determined the solubility at room temperature of these common constituents of headache powders in acetone, benzene, benzaldehyde, amyl alcohol, amyl acetate, aniline, acetic acid, xylene and toluene. Salol may be removed almost completely from a mixture of all four of the substances by either toluene, xylene or benzene, and acetanilide and phenacetine can be removed fairly satisfactorily from their mixtures with caffeine by means of amyl alcohol or acetate.

The Effect of Caffeine on the Contractions of the Different Parts of the Ureter: D. R. Lucas.

Medium-sized dogs were narcotized by subcutaneous injections of 0.006 gm. of morphine and 0.001 gm. of atropine per kilo of body weight. Venous perfusion of one per cent. caffeine sulphate in Ringer's solution caused the pressure in the renal pelvis to rise steadily; the tracing was surmounted by small curves, the rise in pressure continued for thirty to ninety seconds, the pressure in the straight portion remained unaltered, until at the end of the time indicated a sudden reversal of pressure took place, several smaller variations from normal pressure followed in both parts of the ureter, and the relative pressure in the different parts was finally reestablished. However, the frequency of contractions in each portion continued at an increased rate. The stimulating effect of caffeine on the ureter was studied in a series of experiments which were planned entirely to eliminate the factor of renal secretion. The kidney, ureter and adjacent portion of the bladder were excised, *in situ,* and placed in a bath of warm Ringer's solution. The caffeine was used by suffusion or perfusion. The stimulating effect was observed by the graphic method. When solutions containing 2–5 per cent. of caffeine were used, tonic contractions took place that lasted one to three minutes, after which the peristaltic waves were much smaller and more frequent. In both series of experiments the stimulating action of caffeine caused the pelvis to contract in a tonic manner, the isthmus apparently exerting a sphincteric action, which prevented the urine from passing until the constriction was overcome by the increasing pelvic pressure, at which time equilibrium in the tone of the different parts was reestablished.

The Occurrence of Manganese in the Fresh-water Clam: Harold C. Bradley.

Manganese has been found in considerable amounts in the tissue ash of specimens of the common fresh-water clams of the Mississippi Basin—*Unio* and *Anadonta*—obtained from the lakes at Madison, Wisconsin. It was suspected in the first instances that the manganese might be present in the intestinal tract, from manganese bacteria ingested with the food; but after six weeks starvation the apparent amount of the metal in the ash was unchanged. It seems probable, therefore, that manganese is normally present in these molluscs. Further examination shows it to be regularly present in the blood and eggs, while iron is present in small amounts and copper only in minute traces. The presumption seems warranted that the respiratory exchanges of *Unio* and *Anadonta* are carried on by a manganese-proteid combination analogous to the iron and copper compounds, hemoglobin and hemocyanin, and the less well-known copper and zinc combination, hemosycotypin.

The Hexone Bases in Camembert Cheese: Arthur W. Dox.

Studies on the Metabolism and Physiological Function of Phosphorus Compounds: W. H. Jordan.

On Protein Analysis: P. A. Levene, W. A. Beatty, R. D. MacLaurin and C. H. Ruiller.

On the Influence of Internal Hemorrhage on Chemical Changes in the Organism, with Particular Reference to Proteid Catabolism: F. S. Weingarten.

Two dogs were subjected to the conditions of internal hemorrhage by transferring measured volumes, about 3 per cent. of the body weight, of blood from the femoral artery to the peritoneal cavity. This was done respectively six and seven times, the dogs having first been put in nitrogenous equilibrium and fed each day with the same amount of food of known composition. Due surgical care was observed, the dogs kept under excellent conditions, and urine and feces collected each day. From the first there was a steady decline in weight, although all food given was eaten. The second dog did not regain his weight till six months after the seventh operation. At autopsy neither dog showed blood in the peritoneal cavity, there was no matting of the omentum; the only marked change was the thickening of the abdominal wall after the numerous laparotomies and one band of adhesions between the stomach and the left parietes. Collateral circulation had been prompt and effective in the legs. Daily urine volume showed marked changes, there being a decided fall on the day of hemorrhage and a very marked increase from two to four days later. The average of daily nitrogen in the urine was about the same at the end as at the beginning of each series, having increased for the first few periods, then decreased. The average for the first three days of each period was less than that for the whole period, and more than for the last three days of the period. Similarly, the average for the last three days of a period was less than the daily average of the period. Comparison between total nitrogen ingested and excreted showed that for the first few periods in each series the amount excreted was greater than that ingested, while toward the end the reverse was true. Apparently, in the first case, the nitrogen of the hemorrhagic blood was disposed of, or, if taken into the system, allowed greater elimination from other sources. When the animals felt the effects of the hemorrhages they no longer rid themselves of this extra nitrogen, but used it in the general economy. In the one exception to this a second hemorrhage followed the first so closely that the dog was possibly unable to work up its material.

The research was carried out jointly by B. B. Crohn and the writer along lines similar to those of a previous study of the effects of external hemorrhage.

On Iodomucoid: Gustave M. Meyer.

Fairly concentrated solutions of tendomucoid in 0.5 per cent. sodium carbonate were set aside at 40° with several grams of pulverized iodine. Iodine was added from time to time, so that an excess was always present during the first twelve hours. Warming was then continued until no particles of iodine were noticeable. The liquid had a deep red color. After filtration, the iodomucoid was precipitated with 0.2 per cent. HCl. The precipitate was purified by repeated washings with water, alcohol and finally with ether. A yellow powder was thus obtained, which contained 14 per cent. of iodine. The other halogens were absent. The substance otherwise possessed all of its previous protein properties. It could be redissolved in dilute alkali and precipitated without loss of iodine. Details of further study will be reported later.

A Preliminary Report on the Toxicity of Some Artificial Dyestuffs: Gustave M. Meyer.

A number of commercial dyestuffs commonly used as food colorants were studied in a preliminary way as to their general behavior when administered in different amounts to dogs, with a view of selecting one or more pigments best suited for a further study of effects on metabolism. The coal-tar colors thus far studied are: curcumin *S*, tartrazine, naphthol red *S*, carmoisin *B*, naphthol yellow *S*, gold orange and ponceau 2*R*. The effective amount, or the quantity causing sufficient disturbance to warrant a discontinuance of that dose, varied from 0.83 gram per kilo body-weight for curcumin, to 7.69 grams per kilo for ponceau 2*R*. By way of comparison, it was found that the effective amount of potassium sulphate was 1.75 grams per kilo of body-weight. Ponceau 2*R* is the only one which appeared to be poisonous in the doses given. One animal succumbed after the administration of comparatively small amounts of this dyestuff (2.9 gm. per kilo of body-weight) whereas another animal was unaffected by much larger amounts (7.69 gm. per kilo). In general equally large amounts of various saline purgatives or even many substances commonly regarded as innocuous, gave effects similar to those following the use of the colorants mentioned. These dyestuffs were absorbed only to a slight extent and, therefore, their elimination through the urine was not very marked. The larger part of the ingested dyestuff passed off unaltered in the feces. This was particularly true when an excessive dose of material had been administered.

On the Excretion of Barium: Gustave M. Meyer.

On Alkaverdin, the Coloring Matter of the Purple Pitcher Plant, Sarracenia purpurea: Gustave M. Meyer and Wm. J. Gies.

The carefully selected leaves of the plant were cut and dried, and then extracted with 95 per cent. alcohol. The alcoholic extract was concentrated by evaporation, with repeated additions of water to precipitate the chlorophyl, then filtered and finally evaporated under reduced pressure at a low temperature. Heating the alcoholic solution seemed to have little effect on the pigment, but continuous heating in the presence of considerable water caused chemical alteration of the coloring matter. The residue thus obtained is a perfectly clear, red, resinous mass, soluble in water, but insoluble in absolute alcohol and in ether. It could not be induced to crystallize. The substance is free from nitrogen. The aqueous solution reduces Fehling's solution and yields glucosazone with phenylhydrazine. With benzoyl chloride a bulky benzoyl derivative was obtained. Dilute aqueous solutions are changed in color by acids and alkalies. Alkali produces a deep green, acids discharge the color. This indicator can be used for acidimetry and alkalimetry under conditions similar to those that apply to litmus. The decided change from the green to colorless condition on the addition of acid is, however, far more delicate than the change of litmus from blue to red under similar circumstances. The delicacy of this indicator is not affected by the presence of neutral salts.

On the Formation of Sugar from Amino Acids: Wm. Salant.

Experiments carried out on rabbits in which phlorhizin diabetes was induced, have shown that glycocoll administered subcutaneously or by mouth is not followed by an increased elimination of sugar.

A Biological Method for the Detection of Fluorides in Food Products: S. Amberg and A. S. Loevenhart.

Loevenhart and Peirce have recently shown that sodium fluoride even in very minute quantities greatly inhibits the hydrolysis of esters of the lower fatty acids by extracts of different animal tissues. The effect of a large number of other substances representing widely differing classes of compounds on the hydrolysis of the esters has been studied, with the result that no other substances have been found which have an inhibiting effect comparable in extent with that shown by the fluorides. Ammonium fluoride and hydrofluoric acid act quite similarly to sodium fluoride. It occurred to us that a simple and delicate test for fluorides might be founded on these observations. Clear liver extracts were prepared by the method of Loevenhart and Peirce and the activity of 1 c.c. of this was tested by diluting with 4 c.c. of water and adding 0.26 c.c. of ethyl butyrate and toluene. After acting for 16 to 24 hours at 35° to 40° the increase in acidity was determined by titration with N/20 NaOH. Simultaneously with the above test, experiments were performed in which the filtrates, neutralized if not already neutral, from various food products were used in place of the water in diluting the 1 c.c. of extract to 5 c.c. Other experiments were also simultaneously performed, using the filtrates of food products to which sodium fluoride had been added in various quantities. Working in this way, we have found that sodium fluoride when present in milk in a concentration of 1:5,000 caused an inhibition of 89.5 per cent.; 1:100,000 caused an inhibition of 87.8 per cent.; 1:1,000,000 caused an inhibition of 66.5 per cent. in the production of acid. Slight variations were noted with different extracts. Similarly we have found that the presence of 6 mg. of sodium fluoride in a kilo of meat can readily be detected by using the boiled aqueous extract of the meat in diluting the extract. In some cases, as with grain and beer, it is necessary to ash the product in order to concentrate the fluoride and destroy certain organic substances which when present in large quantities inhibit the action of the enzyme. A more detailed account of these experiments will soon appear.

The Elimination of Radium in Normal and Nephrectomized Animals: WM. SALANT and GUSTAVE M. MEYER.

Radium bromide was injected subcutaneously into dogs, and normal and nephrectomized rabbits. The bile, feces and intestinal contents of a dog provided with a permanent and complete gall bladder fistula were found radioactive two hours after the injection of radium. Radium bromide was also injected into a dog with a temporary biliary fistula under ether narcosis. The urine, bile, and contents of the small intestine were radioactive. Radium was not found in the contents of the large intestine. The gastric contents of these two dogs failed to show the presence of radium. The elimination of radium in rabbits takes place all along the intestinal tract, but not in the stomach. In one of the nephrectomized rabbits radium was absent in the contents of the large intestine. The presence of radium was determined by the electrometer. The materials tested were heated to destroy induced radioactivity.

A Study of the Elimination of Casein in the Bile: WM. SALANT.

Hallauer and Gürber claim to have found large quantities of casein in the bile of rabbits after injection. The elimination of native or foreign proteids in the bile has recently been made the subject of several investigations. Brauer claims to have found albumin in the bile of the dog after poisoning with amyl alcohol. Pilzecker states that large quantities of albumin are found in the bile of dogs poisoned with phosphorus or arsenic. Hallauer and Gürber injected intravenously a solution of casein into rabbits; the bile of these animals when tested with rennin showed the presence of casein soon after its administration. A critical analysis of these results convinced the writer that the subject needs further study before the conclusion may be drawn that proteins, native or foreign, are eliminated in the bile. Accordingly a series of experiments was carried out on dogs and rabbits into which milk or casein, or both, were injected intravenously and the bile was then tested for casein, in the way indicated above, with the result that none has been found. In some experiments 20 gm. of casein dissolved in sodium carbonate were injected intravenously into dogs. The bile was examined at short intervals for the next ten to twelve hours and failed to show the presence of casein.

A Study of the Conditions affecting Zymolysis: WM. N. BERG and WM. J. GIES.

Peptolysis of either fibrin, edestin or elastin is quantitatively unequal in a series of aqueous solutions of different acids under any uniform digestive conditions. Striking disparities in the velocity, quality and extent of digestion of these proteins occur in solutions of common acids, whether the acids are present in the solutions in *equal masses* (equipercentage), or in *equal numbers* of acid *molecules* (equimolecular), hydrogen *atoms* (equinormal), or hydrogen *ions* (equidissociated). Tryptolysis of fibrin or elastin is markedly unequal in equivalent solutions of bases. Among the conditioning influences that were obviously influential in all our zymolytic experiments were the character, state and strength of the acid or base, the quality and concentration of the enzyme, the duration of the period of digestion, the temperature of the digestive mixture, and the nature of the protein. No doubt the different digestive products themselves exerted unequal influences as the proteolytic transformation proceeded. In experiments with fibrin and elastin to determine the effects of the zymolytic products of one of these proteins on the peptic digestion of the other, there was marked interference with the peptolysis of one or the other (or both) when samples of the two proteins were together in 0.2 per cent. HCl in the presence of small proportions of pepsin. The hydrogen ion is the favorable acid factor in peptolysis. The associated anions or molecules (or both) appear to interfere (as a rule) with the peptic process, and their divergent influences seem to account, in part at least, for the quantitative disparities noted in each digestive series of our experiments. It has been shown in our work, however, that *acetic acid molecules* are practically without influence, under ordinary conditions, on the peptolysis of fibrin or elastin in solutions of hydrochloric acid $M/20$—a suggestion that the purely *chemical* phases of the normal gastric digestive process are practically unaffected by vinegar in the proportions commonly ingested. The effects of acetic acid and of vinegar on the *secretory* process in the stomach will be investigated. In tryptolysis the hydroxyl anion is the favorable basic factor and the associated cations or molecules (or both) seem to exercise the deterrent influences. In general harmony with the observed digestive disparities, there were marked inequalities in the swelling effects on fibrin in every equivalent series of acid or basic solutions. Bloating influences on fibrin were due primarily to the acid or base, but were more pronounced in the presence of enzyme. Elastin did not swell perceptibly in either the acid or basic solutions employed, but did so in the latter when trypsin was present. In a given series of equivalent or basic solutions under uniform digestive conditions the degree and sequence of zymolysis of fibrin were strikingly different from those of the digestion of elastin. This fact necessitates the thorough study of the zymolysis of many proteins in samples of the same equivalent acid and basic solutions.

Putrefaction of Tendon, Collagen and Mucoid: D. E. Roelkey and Wm. J. Gies.

Unwashed tendon pieces undergo spontaneous putrefaction in distilled water, tap water and 1 per cent. sodium chloride solutions, under both aerobic and anaerobic conditions. The collaginous masses gradually undergo complete disintegration, although even after putrefaction has continued for about two months in a liquid teeming with motile bacteria, collaginous fragments containing unchanged fibers may be present. The putrefactive odors are very marked. Putrefaction at the end of the period noted, and in the liquids used, has been most advanced in the salt solution and least advanced in the tap water. Anaerobic conditions favored special velocity and extent of the changes. A study of the products of, and the conditions best suited for, the putrefaction of tendon is now yielding numerous results. Similar experiments have been begun with tendomucoid and tendocollagen.

A Further Study of the Chemistry and Pharmacology of Ibervillea Sonoræ: Julia T. Emerson and Wm. H. Welker.

Some of the largest tubers of *Ibervillea Sonoræ* were collected and an effort was made to ascertain their general chemical composition, and to determine the nature of the substance or substances which account for the poisonous qualities attributed to them by the inhabitants of the regions in which the plant is found. Qualitative tests showed the presence of proteins, carbohydrates, fats, cholesterins, lecithins, alkaloid-like substances, inorganic salts and salts of organic acids. Quantitative determinations gave the following results: water 87.10 per cent., solids 12.90 per cent., organic matter 11.48 per cent., inorganic matter 1.42 per cent. Alcohol and ether extracts were made and the Stas-Otto method applied for the extraction of alkaloidal material. The alcohol and ether extracts showed toxic action on frogs. The material obtained by the Stas-Otto method failed to show any toxic action when administered per os or subcutaneously to a small dog. This material seemed alkaloidal in nature but could not be identified as any one of the known alkaloids. Tuber meat from normal tubers, from brown spotted tuber and the rind were fed separately to dogs in amounts up to 2 gm. per kilo of body weight without producing any apparent effects excepting marked diarrhœa. Some work now in progress is intended to determine whether any of the inorganic salts are present in sufficient quantity to produce the cathartic effect noted, or whether this action may be due to an organic acid.

The Effects of Salts of Some Rare Elements on Seedlings: ALICE A. KNOX and WM. H. WELKER.

The general method followed was that described by True and Gies.[2] Salts of the following elements were studied in this work: didymium, beryllium, erbium, lanthanum, cerium, neodymium, yttrium, cesium, and praseodymium. The comparative effects of the anions TeO_4, SeO_4 and SO_4 were also studied. The greatest molecular concentration points at which growth occurred during the first twenty hours and the least molecular concentration points where no growth occurred after the first twenty hours were carefully noted, and it was found that the increase in toxicity followed Mendeléeff's table almost mathematically, the toxicity increasing from group to group and also down each individual group with increase of molecular weight. Points of concentration where the growth reached that of the water control, where initial stimulation occurred, where maximum stimulation occurred and where the growth again returned to that of the control were noted. The results on these points were less sharply defined, as could be expected, when the high dilutions and the normal variation in the seedlings are considered. In general, however, the trend was along the lines of the periodic system.

[2] True and Gies, *Bull Torrey Bot. Club*, **30**: 390, 1903.

Studies of the Effects of Ions on Lipase: RAYMOND H. POND.

The study of lipolytic digestion has been very little in comparison with the attention that has been paid to amylolytic and proteolytic reactions. In the case of the two latter it has been found that dissociable salts inhibit digestion inversely as their decomposition tension. As the correlation mentioned has never been established completely an extension of the study to lipolytic digestion would seem profitable. A commercial product having been found to be very active in the saponification of ethyl butyrate some initial steps have been taken in the intended study. The activity of the enzyme can be satisfactorily expressed in the number of c.c. of $M/20$ KOH required to neutralize the fatty acid arising from the saponification of the neutral butyrate. A sample preparation would be 2 c.c. of a solution of one gram of the enzyme powder in 100 c.c. of water + 2 c.c. of the toxic salt, for instance $M/64$, silver nitrate + 0.1 c.c. of the ethyl butyrate. The control would be an identical preparation except that boiling would occur before the addition of butyrate. The difference in acidity of two such preparations after a given period of digestion at a given temperature would express the amount of lipolytic activity possible in $M/128$, silver nitrate under the conditions of the test. Thus barium nitrate at a concentration of $M/4$ allows lipolytic activity, while a dilution of $M/1024$ is without any inhibitory effect. In the case of silver nitrate $M/256$ inhibits, $M/512$ allows and $M/16384$ is indifferent. The corresponding points for lead nitrate are, $M/64$, $M/128$ and $M/8192$. These figures show that the enzyme is very resistant to toxic salts. This may be due to proteid impurities. No certain stimulation has thus far been established though some figures have seemed to indicate it.

Further Observations of the Effects of the Rays of Radium on Plants: C. Stuart Gager.

Results previously reported led to the conclusion that the rays of radium and of other radioactive substances act as a stimulus to germination and growth, accelerating or retarding these processes according to the duration and distance of exposure, and the strength of the radium salt employed. Further experiments warrant a similar conclusion with respect to other plant activities, such as respiration, starch-making, geotropic response, etc. The growth of plants watered with radioactive water may be accelerated or retarded. The result varies, not only with the degree of radioactivity, but also with the species of plant employed. Growth may be accelerated in an atmosphere containing the emanation of radium. When pollen or ovules are exposed before pollination, or when exposure is made after fertilization of the egg, plants grown from the resulting seeds vary profoundly from the parent plants. The heritability of these variations has not yet been tested.

Some Chemical Notes on Specimens of American Amber: Wm. J. Gies.

The amber was discovered by Dr. Arthur Hollick several years ago, associated with fossil leaves in the cretaceous clays at Kreischerville, Staten Island. The various samples were quite different in color and transparency, but in each case closely resembled a well known variety of amber in hardness, color, specific gravity, etc. Pulverized portions of the most typical samples lost only 0.45 per cent. in weight on drying several days at 110° C. Ash content was only 0.1 per cent. Analysis indicated the following elementary composition: C, 78.5; H, 9.5; S, 0.25; O, 11.75. On destructive distillation succinic acid appeared to be formed and a considerable quantity of volatile sulphide was evolved. Long continued extraction in absolute alcohol and in anhydrous ether led to the solution of 42.6 per cent. of the powder in the former liquid, and 45.5 per cent. in the latter. The specimens appeared to consist of typical amber. Further observations on larger quantities are expected to clear up any doubt in this regard.

Effects of Acids on Tendon, with some Notes on the Preparation of Elastin, Collagen and Mucoid: Wm. J. Gies.

When sections of Achilles tendon are immersed in dilute acid, *e. g.*, 0.1 per cent. HCl, they become greatly bloated in a few hours. If the pieces are thin transverse sections to begin with, they swell to large semitransparent mulberry-masses. Such sections swell unequally in any series of different acids of equivalent concentrations. The bloated masses are rigid and rebound when dropped on a hard surface. Although tendon is ordinarily very resistant to any process of maceration, the swollen masses produced by treatment with acid may be readily minced in a hashing machine. The hash thus produced forms with water a clear viscid mass, the whole of which may easily be pressed through cheese cloth, and in which the gelatinous particles are in great part indistinguishable. This viscid product bears a striking superficial resemblance to egg white, so far as consistency is concerned, but it lacks the yellowish tinge of fresh egg white. When such a viscid acid-tendon-water mixture is made alkaline, or a swollen piece of acid-tendon is dropped into dilute alkali, *e. g.,* half saturated lime water, transparency is quickly lost and there is a rapid return to the original white, opaque, *fibrous* condition of the material. This striking transformation may be brought about with great ease, even with pieces of tendon that have been immersed in acid for months. The alkaline liquid contains practically all the mucoid in the tissue. The collagen does not appear to be gelatinized or particularly dissolved. These facts furnish starting points for improved methods of preparing elastin, collagen and mucoid from ligament and tendon. This particular study was undertaken in an effort to discover the chemical state of mucoid in connective tissues. At present the results suggest a union in the tissues between collagen and glucoprotein. Further study of the phenomena alluded to and numerous physico-chemical influences on them, is in progress.

On a Glucoprotein from Tendon that is Non-precipitable by Acid: WM. J. GIES.

Tendon yields a water-soluble, acid-reacting glucoprotein product that may be extracted from that tissue with lime water, and which is non-precipitable by acid from such extracts. It is possible that this product is derived from the main mass of glucoprotein in the extract when the latter is acidified for precipitation of the traditional mucoid. Extended comparative observations of elementary composition, reactions, etc., will be made before an answer to this question will be offered.

New Studies of Mucoids and Nucleoproteins: (1) *Inorganic Salts;* (2) *Organic Salts;* (3) *Color Compounds;* (4) *Quantitative Determination:* WM. J. GIES.

Numerous *water-soluble* salts of mucoids and nucleoproteins have been made, such as salts of calcium, tetraethylammonium hydroxide, conine and azolitmine. As a rule the acid protein combines readily with the base, and the water-soluble compound may be obtained from the filtrate by precipitation with alcohol. An elaborate study of the chemistry of these compounds is now under way, including experiments on the electrical conductivity of the solutions, with the cooperation of Messrs. W. H. Welker, A. D. Emmett and J. Rosenbloom. The electrolysis of the salts and their pharmacological effects and various biological relationships are also being studied. It is impossible *completely* to precipitate either mucoid or nucleoprotein from alkaline tissue extracts by the usual acidification process, even when the greatest care is taken to prevent over-acidification. In the case of mucoid, for example, a loss of 20 per cent. or more of the material usually results with each precipitation by this method. The portion of the mucoid that is non-precipitable by acid may be precipitated from the filtrate by treatment with alcohol in moderate excess. All data heretofore obtained by various observers for the quantitative contents of mucoid in normal and abnormal tissues are incorrect —probably invariably too low. Mr. C. E. May is cooperating in a study of the quantitative determination of compound proteins in tissues.

Determination of Acetanilide in Headache Powders: ATHERTON SEIDELL.

The method consists in boiling the weighed sample of powder with 20 per cent. or stronger hydrochloric acid for about five minutes, by which treatment the acetanilide is converted into aniline hydrochloride. On titrating the hot or cooled solution containing a large excess of acid with standard potassium bromate solution a flocculent precipitate of aniline tribromide separates and as soon as an excess of the bromate solution is added the yellow color of the liberated bromine indicates the end of the reaction. Experiments showed that such substances as caffeine, salol, inorganic salts, etc., do not interfere with the accuracy of the titration, but that the presence of phenacetine or of antipyrine renders the method inapplicable.

Most of the papers presented at the meetings will be published later in the different chemical journals.

The meetings were well attended, and owed much of their success to the trustees and faculty of Columbia University. A unanimous vote of thanks was tendered to them and the Chemists' Club, to the College of the City of New York and to the different industrial establishments which the visiting chemists had been invited to inspect.

This report has been transmitted through Professor Charles L. Parsons, Secretary of Section C.

C. E. WATERS,
Press Secretary.

SCIENTIFIC BOOKS

Inheritance in Poultry. By C. B. DAVENPORT, Director of Station for Experimental Evolution. Publications of the Carnegie Institution of Washington. No. 52, 1906.

The important and extensive series of experiments with poultry, carried out by Bateson and Saunders and Hurst in England have shown the general application of Mendelian principles to inheritance in this group. These authors have demonstrated the relation of dominance and recession of many of the characters of poultry, showing, for example, the dominance of rose comb, white plumage, extra toe, feathered shanks, white and blue shanks, crested head, brown egg color, and broodiness; and the recession of leaf comb, single comb, black plumage, buff plumage, normal foot, clear shanks, uncrested head, white egg color, and non-broodiness. The same investigators have likewise shown that Mendelian splitting occurs in the second generation, and have referred the results to Mendel's hypothesis of segregation in the gametes of the first generation of hybrids. They have also drawn attention to the fact that the dominance of characters in poultry is not always complete in the first generation. Hurst estimated, in fact, that incomplete dominance is twice as numerous as complete dominance. It was also observed that in the second generation there is often a mixing of the characters, so that it is difficult or impossible to distinguish the pure forms from the 'dominant-recessives.' In other words, there may be almost a continuous series in this generation. Such results are difficult to account for on the basis of 'pure' gametes, although a tendency towards segregation may be distinctly recognized. The case most difficult to explain in this connection is the inheritance of extra toes. Castle's recent experiments with polydactylism in guinea pigs have shown in fact that prepotency, rather than Mendelism, is a more important factor in this kind of inheritance. It looks as though certain individuals may transmit a given peculiarity differently from other individuals; and while the 'lump-sum' may often give an approximation to Mendelian expectation, the really important fact is not the chance result, but the prepotency of certain individuals in regard to the transmission of characters.

Professor Davenport's work covers in part the same ground as that of Bateson and his co-workers; in part, however, he has studied different characters and races, and has been able to add many new and important facts to those already known. The present work is, however, to be looked upon rather as preliminary—a first instalment of the extensive experiments under way at Cold Spring Harbor.

It would seem that studies dealing with the hybridization of unit-characters relate to the field of heredity rather than to that of evolution. In fact, many evolutionists of the old school deny the applicability of such results to the theory of evolution. There seems to be here a difference of opinion, whether well founded or not the future will decide. The opening paragraph of Professor Davenport's paper leaves, however, no room for doubt as to his attitude in the matter. "Evolution proceeds by steps. These steps are measured by the characters of organisms. * * * Since the character is the unit of evolution it deserves careful study."

The paper before us consists of thirteen studies in hybridization with thirteen different races of fowls. The facts are presented with admirable clearness and conciseness, and despite the large number of details that the subject demands the matter is handled in a very attractive way. The seventeen plates of photographs add materially to the interest and importance of the work.

For the detailed results of the author's experiments, the original paper should be consulted. It must suffice here to give a few of the more salient points.

When the single-comb, black Minorca is crossed with the single comb black Polish fowl "no characteristic [character?] is inherited in strictly Mendelian fashion, for, in no case is dominance complete." In the second generation twenty-four individuals were wholly black, and twenty-five wholly black and white or mosaics. "I interpret this irregular result to be due to the imperfect dominance of black."

When the single-comb, white Leghorn was crossed with the Houdan, the offspring were white with traces of black on one or more feathers. When inbred, these hybrids produced 18 per cent. pigmented individuals like the Houdan,—the Mendelian expectation being 25 per cent.—and the rest of the offspring was in part pure white and in part impure white. The former proved to be all males and the latter females! Other characters also, nostrils, muff, beard, crest, polydactylism, etc., show the influence of both parents in the first generation, and incomplete dominance and recession in the second, although a tendency to separate into Mendelian groups may be detected.

In the next cross, Houdan by Minorca, the author states: 'Striking is the nearly universal imperfection of dominance.' Imperfect dominance is also recorded for many characters of the five crosses that are next described, but since the experiments were carried only to the first generation, the results are still too incomplete for satisfactory interpretation.

An interesting cross was made between the Japanese Tosa fowl and the white Cochin bantam; the former race is characterized by abnormally long tail coverts. The first hybrids were like the Tosa fowl, although the tail was intermediate; every feather of the male was barred with white and in the female the shafting was broadened. In the second generation 28.1 per cent. were white to 71.9 per cent. pigmented individuals, but of the sixteen white individuals only five were without reddish pigment. "The forty-one pigmented individuals show a curiously mixed lot of coloration." One showed a Partridge Cochin plumage which must have 'lain latent in the white Cochin.' It is important to notice that in this cross white is not dominant in the first generation as with most other races of fowls.

The following four crosses were carried through the first generation only. They show, nevertheless, the complete dominance of frizzling; the incomplete dominance of the white of the white Leghorn over red of the game; the complete dominance of long tail over rumplessness (although one chick had no tail!); incomplete dominance of the black of the Cochin bantam over the red of the game.

A clear and able summary and discussion follows the detailed account of the experiments. Unit characters are not regarded as rigid or immutable from the evidences of hybridization in poultry. DeVries's distinction between varietal and specific crosses, based on their behavior in inheritance, does not hold, as is shown by contrasting the behavior of those characters that are new to the race with those that have been lost. Furthermore a similar comparison shows that deVries's view, that a patent character dominates a latent one, as color does albinism, is not always found to hold in poultry. Standfuss's idea, that older characters or species dominate, has been shown by Bateson and Saunders not to hold good, and Davenport is able to confirm their conclusion. Of nineteen characters examined nine old ones dominated and ten new ones. The result depends clearly on some physiological peculiarity of the character that has no relation to its antiquity. These and other conclusions of the author are of importance for a fuller understanding of the laws of inheritance.

This admirable study of Professor Davenport's gives promise of still more important work to follow when the history of the material is further worked out. The Carnegie Institution is to be congratulated on the high order of work accomplished under its auspices.

T. H. Morgan

Columbia University

SOCIETIES AND ACADEMIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 629th meeting was held on February 16, 1907, President Hayford in the chair. The evening was devoted to a paper by Dr. C. G. Abbot, on 'Solar Radiation and Terrestrial Temperature.'

The speaker gave a general account of the work of the Smithsonian Astrophysical Observatory in recent years, including its two expeditions of 1905–1906 to Mt. Wilson. The means of observing solar radiation both in total, and for all parts of its spectrum, were briefly described, and bolographic spectrum energy curves extending from wave-length $0.37\,\mu$ to $2.8\,\mu$ were exhibited. Such curves are obtained in twelve minutes each, and a series of eight or ten, covering the hours from low to high sun, is secured on each favorable day. By means of the expression $\log d = m \log a + \log d_0$ (in which d and d_0 are ordinates of such curves for a given wavelength at the earth's surface and outside the atmosphere, respectively, m, is secant of the sun's zenith distance, and a the transmission of the atmosphere for zenith sun) the form of the spectrum energy curve outside the atmosphere is obtained. By means of diagrams it was shown how closely this straight line equation fits the observations, and further evidence of the trustworthiness of the formula as a means of estimating the transmission of solar radiation in the atmosphere is furnished by the close agreement of simultaneous Washington and Mt. Wilson determinations of the solar constant, though a mile difference of altitude and 3,000 miles difference of longitude is between the stations.

About sixty values of the 'Solar Constant of Radiation,' as determined by Mt. Wilson observations of 1905, were shown in a diagram, and the evident change of nearly eight per cent. in August, 1905, pointed out. It was stated that single determinations were thought to be accurate, relatively, to one per cent. for usually good Mt. Wilson days.

The average numerical value of the solar constant is believed to be 2.12 calories per square centimeter per minute; and high values obtained by Langley, Ångström, and others, as quoted in text-books, can be shown to be ill founded. Ångström, indeed, has himself withdrawn his value four calories, but it still continues to be quoted.

Observations of the reflecting power of clouds were described and preliminary results showing wide departure of clouds from ideal matt surfaces were presented. Attention was drawn to the very large amount of solar radiation lost by reflection of clouds to space, and the probability that variations of cloudiness may in many instances mask the effect of variations of the solar radiation. Inland stations, only, seem likely to yield trustworthy evidence of direct connection between variations of the solar constant and the terrestrial temperatures, but evidence was cited of a direct connection of the kind as shown in Mr. Langley's paper (*Astrophysical Journal*, June, 1904) where nearly one hundred stations over the north temperate zone united, not only as a whole, but by groups separately, to show a marked fall of temperature immediately succeeding an observed fall of solar radiation. A large number of inland meteorological stations have been selected to furnish data for an examination of the probability of variation of solar radiation heretofore, and this material is being worked over at the Astrophysical Observatory, and will form a part of Vol. II. of its *Annals* now being prepared.

R. L. Faris,
Secretary

THE CHEMICAL SOCIETY OF WASHINGTON

The 172d regular meeting was held in the Cosmos Club, February 14, at 8 p.m. After the regular business, President Fireman brought before the society the subject of the advisability of appointing a local sanitary committee—"The chemist, like every other citizen, is vitally interested in the sanitary conditions of the locality in which he lives. But, more than any other citizen, he is qualified, owing to his particular training, to examine into, and form a true judgment of the sanitary conditions surrounding him. Self interest, and whatever altruism there is in each civilized person should impel him, especially the chemist, to take an active interest in observing how the local sanitary requirements are provided for." After discussing the subject pro and con, the president was authorized to appoint a committee of seven to advise the society on local sanitary matters, and the committee was instructed to report at the next meeting.

Mr. Sherman Leavitt then read a paper on 'The Manner in which the Alkaline Earth Metals are held in Solution by Carbonic Acid.' The writer gave an account of the investigation of the boiler-water supplies of several western railroads, in collaboration with Professor Keiser, of Washington University, St. Louis. This work, carried on during 1901–1904, showed that the methods for testing waters were not capable of giving the necessary information for properly softening waters for steam purposes. The results indicated that the calcium carbonate held in solution by carbonic acid, as an acid carbonate, required two molecules of carbonic acid instead of one. Later experiments were conducted in a temporary laboratory established in an ice plant, where the temperature was kept at 28° F. The investigator finally succeeded in isolating an acid carbonate of calcium which was found to decompose at about $+2°$ C. On analysis, this compound gave a percentage of water and carbon dioxid gas corresponding very closely with a formula for the acid carbonate containing two molecules of carbonic acid in combination with one of calcium carbonate. Barium was found to give a similar compound with even more favorable results.

On February 16, Professor E. C. Franklin, of the Department of Physical Chemistry at Stanford University, delivered an address before the Chemical Society at the George Washington University on some of his researches regarding the reactions which take place in liquid ammonia. The speaker called attention to the fact that liquid ammonia was neutral and had properties in all respects analogous to those of water. The lecture was illustrated by charts and equations.

J. A. LeClerc,
Secretary

Bureau of Chemistry

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA

The 170th meeting was held in the main lecture room of Chemistry Hall on Tuesday evening, February 12, 7:30 p.m., with the following program:

Professor Collier Cobb: 'Some Human Habitations.'

Professor Joseph Hyde Pratt: 'The Fishing Industries of North Carolina.'

Alvin S. Wheeler,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

THE 'FIRST SPECIES' AND THE 'FIRST REVISER'

The vast extension of our knowledge of animals and plants has forced upon all investigators the necessity of reducing systematic nomenclature to law and order, and to eliminate from it all elements of personal choice and personal favoritism. It has, moreover, shown that no rules or laws can be made binding by mere agreement. They must in the very nature of things represent the best possible adjustment, else later generations will cast them aside. Hence laws must be simple, clear, and as far as may be, free from exceptions.

As part of the nature of things, the law of priority has forced its way to acceptance. In like fashion the law of unchanging spelling and the rule that nouns spelled differently are different words, regardless of etymology, must become universal.

The chief point of variance now left is this: A genus is known by its type. In case an author of a genus names several species, but fails to designate one as type, how shall the actual type be determined?

In this case we have two possible methods, both resting on a logical basis. I do not count among these the rule of elimination, which in my judgment has never been defined in workable fashion, and, however defined, will never meet with general adoption.

These two methods are, in brief, that of the 'first reviser,' and that of the 'first species.'

In the first method, when a genus is left without type indicated, the genus rests with the first of the original species which any subsequent author may select as type. In the second method, the type, unless otherwise indicated, is the first species named under the head of the genus in question, by its original author.

The logical basis in the first case is this: No generic diagnosis is complete until a type species has been indicated. If the original author neglects this, the first of his successors who does it, completes the generic diagnosis, and the generic name in question must stand or fall with the species selected. The rule of elimination may be a handy device for the use of the first reviser, and as such it has been justly commended; but he does as he pleases, and from his decision, the type once chosen from among the original species, there is no appeal. That is, we have no appeal, unless we find an earlier 'first reviser,' in which case the act of the later one is null and void. This rule of the first reviser is probably more generally recognized than any other in systematic zoology and botany. It has been lately strongly supported by Dr. Charles Wardell Stiles. Objections to it are these: Often the first reviser overlooks the fact that a type was virtually or even actually indicated by the original author. This makes it necessary to reverse many time-honored decisions, in which the work of the reviser is better known than that of the original author. More often the first reviser fails to make his own position clear. Whether he has actually chosen a type or merely used a species of illustration is often a matter of doubt. Still worse, after we have followed the first reviser, we find that we had overlooked a still earlier one. As a result of this, no name of this sort is safe until all the returns are in and all the work of obscure authors has been examined. This is bad enough when the priority of names is in question. It is a trial to do it in the interest of the meaning of names as well.

The first species method rests on this principle; the application of a generic name should depend solely on data furnished by the author himself. If he fails to indicate a type species, either directly or in some one of the recognized methods, then the first species he mentions must be considered as type. If he mentions no species, directly or by implication, the genus has no existence. This rule has been lately adopted by the American Ornithologists' Union and a strong argument for it has been made in this journal by Mr. Witmer Stone.

There is no injustice in this rule. Its application rarely if ever admits of serious question. It does not involve any bootless investigation of the meaning or intention of subsequent writers. It would involve probably less change of accepted nomenclature than any other rule that can be framed. The first reviser, in fact, has usually chosen as type the first species named by the original author. The French school of zoologists, Lacépède, Cuvier and their successors as a rule have placed first as *chef de file* the typical species of each genus. Those memoirs in which the type species is placed in the middle of a genus are, in general, systematic catalogues, not descriptions of new species. When a genus is first named, nine writers out of every ten place first a species they know and which they deem typical of the group in question.

For all these reasons, the writer thinks that the first species rule should prevail, though he would not strenuously object to the rule of the first reviser. In so far as the method of elimination sets aside both first species and first reviser, doing the work over again with every change in our views of generic limits, he feels sure that future systematists will have none of it.

DAVID STARR JORDAN

SPENCER F. BAIRD

THE following, which I have just come across in the 'Autobiography' of Moncure D. Conway (Vol. 1, p. 49) seems worth reprinting in SCIENCE. Conway was a student at Dickinson College, Carlisle, Pa., when Baird was professor of zoology there.

T. D. A. C.

Baird, the youngest of the faculty, was the beloved professor and the ideal student. He was beautiful and also manly; all that was finest in the forms he explained to us seemed to be represented in the man. He possessed the art of getting knowledge into the dullest pupil. So fine was his spirit that his explanations of all the organs and functions of the various species were an instruction also in refinement of mind. Nothing unclean could approach him. One main charm of spring's approach was that then would begin our weekly rambles in field, meadow, wood, where Baird introduced us to his intimates. About some of these —especially snakes—most of us had indiscriminate superstitions. Occasionally he would capture some pretty and harmless snakes, and show us with pencillings their difference from the poisonous ones. He even persuaded the bolder among us to handle them. * * * After Professor Baird went to reside in Washington I had opportunities of seeing him and his family often. Mrs. Baird was a lady of fine culture and much wit. Baird was very lovable in his home, and to the end of his life he remained a man in whom I never discovered a fault of mind or heart. He awakened in me a love of science, to which I had previously given little thought.

MONUMENT TO MENDEL

TO THE EDITOR OF SCIENCE: It may not be generally known that an effort is being made to erect a suitable monument to Gregor Mendel. The movement has the support of leading biologists both in Europe and in America. It is hoped that American biologists will use their best efforts to see that this country is not behind Europe in its appreciation of the remarkable work of this pioneer in a field which at the present time occupies so important a place in biology.

According to circulars just received from Professor Tschermak, Professor C. B. Davenport, Cold Spring Harbor, Long Island, New York, has consented to receive subscriptions to the fund.

While American scientific men are, generally speaking, not wealthy, I think we all appreciate fully the service rendered by Mendel, and should not only contribute according to our means, but should endeavor to induce those who have the means and are interested in the progress of science to do so.

W. J. SPILLMAN

U. S. DEPARTMENT OF AGRICULTURE

ASSOCIATED PRESS FAKES

TO THE EDITOR OF SCIENCE: The Associated Press cables us that Matteucci says that within a few weeks the earth is to strike the tail of Marchette's comet, with dangerous consequences to the world. Now does the Associated Press believe this, and has it made any provision against such catastrophe; or has it merely preempted the right to say 'I told you so'?

Every one recognizes the attempted scare as one of the vapid jokes of an Associated Press agent. No one is soft enough to be hoaxed, and no one has suffered from the stupid fake, except a few frightened invalids and that particular Press Association itself which allows its name to be used as guarantor for such folly. Luckily there are other press associations, whose reputations are above sending such stupid cablegrams, or making their patrons pay for such nonsense.

C. A.

SPECIAL ARTICLES

RECENT DISCUSSIONS OF THE ORIGIN OF GYMNOSPERMS [1]

The discovery a few years ago by Professors Scott and Oliver[2] of the relation of the Paleozoic seeds known as *Lagenostoma* to the stems of *Lyginodendron*, in itself perhaps the most important contribution of paleobotany to botany that has ever been made, appears to have inaugurated an era of speculation in England during which the ferns seem to be in danger of almost total elimination, if one may judge from some of these recent contributions to the literature, Professor Seward even bringing forward the Lycopodiales as the ancestors of the Araucarieæ and necessarily the balance of the Coniferales as well, as if to relieve the dwindling Paleozoic Filicales from the burden of standing godfather to too many modern lines of descent.

While yielding full appreciation of the remarkable discoveries so admirably worked out in connection with the Pteridosperms, it seems to the writer that the present is an opportune time for recalling that threadbare maxim 'to make haste slowly,' for while we observe a laudable conservatism when it comes to a mere 'impression,' a 'structure' seems to be a peg on which it becomes immediately necessary to hang a theory.

Aphlebiæ may indicate pteridospermous affinity as Professor Oliver has intimated, and exannulate sporangia may also suggest, to some, that the Eusporangiate ferns are absent in the Paleozoic, although this latter view is a rather sweeping generalization from Mr. Kidston's *Crossotheca Hœninghausi* and Miss Benson's *Telangium Scotti*. One is tempted to inquire whence came these structures? Were they evolved among the Pteridospermæ? or rather do they not furnish another illustration of what Mr. Worsdell styles the grain of truth which underlies Professor Seward's discussion, that "All groups of plants shewed the same organs because they had inherited them from common ancestors." While the presence of *Aphlebia*-like organs and exannulate sporangia may eventually be found to characterize the Pteridospermæ, they may with safety be considered to also characterize some of those members of the Filicales from which the Pteridospermæ took their rise. While I would not press the existing terminology of the Filicales too closely upon the generalized forms of the Paleozoic any more than I would consider the mammal *Phenacodus* a horse or a cow, still in our endeavor to get away from a too rigid terminology we are in danger of going too far in the opposite direction, and while it may perhaps be well to set the early Filicales apart under an ordinal name, that of Primofilices (personally I would prefer Eofilicales), it savors somewhat of Saporta's Pro-Gymnospermæ. Mr. Arber, however, seems to limit the proposed term to the Leptosporangiate Paleozoic ferns or their immediate ancestors so that the group might equally well be termed the Primo-Leptosporangiatæ, in fact his diagram (*Ann. of Botany*, fig. 1) shows that he 'does not consider the evidence for the existence of the Eusporangiatæ entirely satisfactory' until we come down to so comparatively recent a period as the Tertiary. The writer feels very strongly that the future will show this view to be a reactionary one. Personally I place more reliance on the resemblance of the 'frond genera' *Tæniopteris*, *Danæopsis*, etc., to modern forms, especially as the fructifications are known in several instances, than I do upon the suggestion that these latter may be the sporangiate organs of the Bennettiteæ the descendants of the Paleo-

[1] Oliver, F. W., 'The Seed, a Chapter in Evolution,' Pres. address to Botanical Section, Brit. Assn. Adv. Sci., York, 1906. Arber, E. A. Newell, 'On the Past History of the Ferns,' *Ann. of Bot.*, Vol. XX., pp. 215–232, July, 1906. 'The Origin of Gymnosperms at the Linnean Society' (a discussion by Oliver, Scott, Arber, Seward, Weiss, Worsdell and others), *New Phytologist*, Vol. V., pp. 68–76, 141–148, 1906. Seward, A. C., and Ford, Sibille, O., 'The Araucarieæ, Recent and Extinct,' *Phil. Trans. Royal Soc. London*, B, Vol. 198, pp. 305–411, 1906.

[2] Scott, D. H., and Oliver, F. W., 'On the Structure of the Paleozoic Seed *Lagenostoma Lomaxi*, with a Statement of the Evidence upon which it is Referred to *Lyginodendron*,' *Phil. Trans. Royal Soc. London*, B, Vol. 197, pp. 193–247, 1904.

zoic Pteridospermæ. This is surely replacing a probability by a possibility, and we are led to wonder if the filicinean standing of the modern Marattiaceæ will be the next point assailed. In this connection it may not be amiss to quote from Wieland's summary in his splendid study of American Fossil Cycads, just published,[1] he says: "Plainly the preceding résumé of the principal characters of the two great cycad groups as combined and showing their descent from Marattiaceous ferns of the Paleozoic, is not merely conclusive, but one of the great cornerstones upon which the conception of evolution can rest secure."

At the risk of being classed as an 'impressionist' I would maintain that impressions lacking the talismanic 'structure' are not without value, and that resemblances to modern forms, while they may sometimes be instances of homoplasy, are far from being 'of absolutely no value.' It should be borne in mind that structures concerned with the vital process of reproducing the plant species, particularly at a time when seed-bearing was being inaugurated, would be far more liable to show homoplastic variations than would the purely vegetative structures. And far from echoing Professor Seward's statement that at the Linnean Society discussion too great stress had been laid on vegetative and too little on reproductive organs, it would seem to me that the reverse has been the true case.

Dr. Scott's discussion, as usual, is admirable and only too brief in the printed report. The novelties which he can always be depended upon to bring before his audience are facts of observation and not subjective. It may not be amiss to repeat his statement that he repudiated entirely the origin of the Araucarieæ or of any of the known Gymnosperms from Lycopods, and this leads us to a brief consideration of the more pretentious paper by Seward and Ford in the *Philosophical Transactions*. As a summary of existing knowledge of the fossil forms which may be or have been referred to the Araucarieæ, and as a contribution to our knowledge of the relatively little known living forms, this contribution contains much that is valuable, especially under the second head just mentioned.

As a possible illustration of the somewhat biased view-point assumed throughout, the fossil cone scales which have been referred to *Dammara* may be cited. With reference to all of these the authors say—"we fail to recognize any sufficient reason for this comparison." They quote Berry (1903) as seemingly concurring in Newberry's doubt regarding their relation to *Dammara,* which doubt they characterize as 'well founded.' This in spite of the fact that Newberry was quoted by me and his view discredited in the same paragraph, while in the next paragraph my suggestion of a further reason for doubting Newberry's view is quoted by the authors in another part of their paper (p. 380) as casting doubt upon Hollick's determination of Araucarian foliage from Cliffwood, N. J. While the facts are matters of no very vital importance in this connection, I may state that I have recently collected an undoubted Araucarian cone of large size from the New Jersey Cretaceous and foliage similar to *Araucarites ovatus* Hollick from the Cretaceous of North Carolina, and that I do not entertain the slightest doubt of their Araucarian affinities. Furthermore, in reference to *Dammara* in a paper published in 1904,[3] a copy of which was mailed to Professor Seward, I state that additional material had led me to remove the interrogation mark which Hollick had placed after the generic name in *Dammara Cliffwoodensis*. These details are only mentioned in this place to show the misconceptions, presumably present in other cases, arising from a misquotation of various authors. Happily Hollick and Jeffrey have recently shown[4] that the relationship of the *Dammara* scales, founded as it was upon external resemblance, is amply proven by the anatomical structure, although it must be confessed that this contribution does not seem to have greatly impressed the English authors if we may judge from their summation of its contents.

[1] Carnegie Institution of Washington, Publ. No. 34, August, 1906, p. 240.

[3] *Bull. Torrey Club*, Vol. XXXI., p. 69.

[4] *Amer. Nat.*, Vol. XL., No. 471, March, 1906.

When we come to consider the theoretical conclusions which Seward and Ford feel called upon to bring forward relative to the Lycopodian ancestry of the Araucarieæ, which conclusions are evidently those of the senior author, we cannot assent to any of them, and while it is expressly stated that they do not include the other Coniferales—the Araucarieæ standing far removed from them, it is impossible to understand, as has been already pointed out by Dr. Scott, how the Araucarieæ can be disassociated from their present position in the order Coniferales, which is an eminently natural group as it stands.

In conclusion to refer briefly to Professor Oliver's address on 'The Seed, a Chapter in Evolution,' it may be said that it is a delightful sketch of the possible origin of the seed-habit, couched in a popular style and full of pertinent and suggestive points.

Edward W. Berry
Maryland Geological Survey,
Baltimore, Md.

BOTANICAL NOTES

THE COLLECTION AND STUDY OF VEGETABLE GALLS

Botanists should not neglect the collection of vegetable galls of all kinds, whether caused by plant or animal parasites, since in either case the galls themselves are plant growths. These galls have been studied for some time by Dr. Mel. T. Cook, now of the New York Botanical Garden, Bronx Park, New York City, and he now asks all collectors to aid him in securing as many specimens as possible for his use. He asks that collectors bear the following suggestions in mind.

1. Many species of hard, woody galls should be dried and kept in boxes.

2. Most species of leaf galls should be dried in the same manner as herbarium specimens, except that the weights used should usually be much less—only sufficient to keep them straight.

3. Succulent species which lose their characteristic form in drying should be preserved in alcohol or formalin.

4. The host plant should be determined, or sufficient material sent to permit satisfactory determination.

5. The species should be wrapped separately in paper, or preferably in 'cheese cloth' so that any insects which mature in transit may be kept with their respective galls.

6. While galls produced by both insects and fungi are desired, it should be remembered that Dr. Cook is making a special study of the *galls* rather than the insects or the fungi. The work is strictly botanical, and he, therefore, appeals to botanists to aid him.

7. When the specimens are ready send them to Dr. Cook, at the address given above, accompanying them with an explanatory letter.

MORE PHILIPPINE BOTANY

The closing number (December) of the *Philippine Journal of Science* contains two articles of botanical interest, viz.:—'The Physiologically Active Constituents of Certain Philippine Medicinal Plants,' by R. F. Bacon, and 'Philippine Fibers and Fibrous Substances,' by G. F. Richmond. The latter is illustrated by several plates. During the year there have been printed in this publication eight botanical papers, and if we add those printed in the five supplements, the number is brought up to nineteen. These supplements, which have been wholly botanical, make a good-sized volume of themselves, covering about 400 pages. Added to the 1,100 pages of the *Journal* proper, the total result is about 1,500 pages of scientific matter for the year. As previously announced, the *Journal* will be divided hereafter, so that the botanical papers will constitute a series by themselves.

In the closing number of the 'Supplement' series E. D. Merrill contributes an interesting paper entitled 'An Enumeration of Philippine Gramineae, with keys to Genera and Species,' covering eighty-six pages, and including notices of seventy-two genera and 226 species and varieties. The paper is almost wholly based on material collected since the American occupation of the islands, and all species not verified by actual specimens are referred to the lists of 'doubtful or excluded' species, which are appended to the genera or tribes. Hackel's well-known monograph is followed rather closely in arrangement and nomenclature.

In looking over the tribes one finds many familiar genera, more than half being more or less common in this country. Thus one finds *Andropogon* (18 species), *Paspalum* (4 sp.), *Panicum* (34 sp.), *Setaria* (4 sp.), *Leersia* (1 sp.), *Sporobolus* (3 sp.), *Agrostis* (1 sp.), *Aristida* (3 sp.), *Calamagrostis* (2 sp.), *Diplachne* (1 sp.), *Eragrostis* (10 sp.), *Poa* (2 sp.), *Bromus* (1 sp.), etc. Four genera and thirteen species of bamboo (*Bambuseæ*) are recorded, but as to these the author states that his treatment is still necessarily incomplete, owing to the rare flowering of most of the species, and the unsatisfactory condition of the available herbarium specimens. The author concludes that 'on the whole, the Philippine Gramineae are strongly Malayan or Indo-Malayan, with a decided northern element in the highlands of northern Luzon, and a rather characteristic [north] Australian one.' In this connection the reader may profitably consult C. B. Robinson's paper on 'Some Affinities of the Philippine Flora' in the January number of *Torreya.*

Here may well be mentioned four pamphlets from the Philippine Bureau of Forestry, consisting of the annual report of the director, Major G. P. Ahern (including sixteen fine plates, most of which are of botanical interest); Bulletin 4 (including a paper on mechanical tests, properties and uses of thirty woods, and another on Philippine sawmills, lumber market and prices); Bulletin 5 (including a working plan for a forest tract on Negros Island, with twelve plates and a map); Bulletin 6 (including a working plan for a forest tract on Mindoro Island, with fourteen plates and a map). These bulletins contain much matter of interest to the general botanist, as well as to the forester.

EFFECTS OF SHADING ON SOIL CONDITIONS

THE Bureau of Soils of the United States Department of Agriculture has published a bulletin (No. 39) prepared by Mr. J. B. Stewart, on the effects of shading on soil conditions which is of much interest to plant physiologists. He shows that under the protection of a tent (1) the soil retains more moisture, (2) the temperature of the air is slightly warmer, (3) the relative humidity of the atmosphere is greatly increased, diminishing the transpiration of the plants and increasing their turgidity, (4) the velocity of the wind is reduced, still more reducing evaporation, (5) the plants make a larger, more rapid and earlier growth. The exact data for 1, 2 and 3 are as follows, in averages for the season:

	Inside tent	Outside tent	Difference
Soil moisture	14.7%	11.6%	3.1%
Temperature	72.8°	71.4°	1.4°
Relative humidity ..	79.0%	71.7%	7.3%

The growth of plants inside and outside of the tent is plotted on cross-section paper, showing very plainly that the plants in the tent grew faster and larger, and matured at an earlier date than the plants outside. The author points out, however, that "while the plants grew faster, and the leaves larger, the yield per acre was less by 100 to 300 pounds inside the tent than outside. This was probably due to the influence of the shade, which tends to make the surface of the leaves larger, but at the same time much thinner."

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

HENRI MOISSAN[1]

MEMBRE DE L'INSTITUT

BUT what I can not put into these successive chapters, is the joy I experienced in pursuing these discoveries. To walk in a newly-turned furrow, to feel one's self untrammelled, to see new subjects for study spring up on all sides, is so delightful that it can not be fully comprehended except by those who have experienced the ardent pleasure of research.

With this closing to a preface, Moissan throws open the door and invites us to participate with him in the discoveries made with the electric furnace. This most skillful of experimenters, having won his spurs in the conflict of isolating fluorine, thus distancing all others who had attempted the task, he attacks new problems with zeal, and the ability of an accomplished craftsman. He was truly

[1] Read before the New York Section of the American Chemical Society.

the embodiment of Davy's adage, 'It is the duty of the chemist to be bold in pursuit.'

Almost his last words as he left this city in 1904 was the terse sentence, 'Chemistry is still an experimental science.'

Moissan was born in Paris, September 28, 1852. He completed his education at an early age, his professional work being guided by Berthelot and Déherain. He was chief of laboratory at the École de Pharmacie in 1879. He attained his doctorate in science in 1885, presenting a remarkable thesis on the 'Série du Cyanogene.' This secured him the appointment of professor of toxicology at the school of pharmacy. He was elected a member of the Academy of Medicine, Section of Pharmacy in 1888, and made a member of the Academy of Sciences, replacing Cahours, in 1891. He was decorated with the Legion of Honor in 1886.

His earlier researches on the 'Oxides of Iron,' 1877, on 'Chromium and its Compounds,' in 1882, and on the 'Fluoride of Arsenic,' 1884, gradually led to his notable discovery of a method of isolating fluorine, in 1886.

His indefatigable pursuit of a problem is typically exemplified in a remark in his monograph on 'Fluorine and its Compounds': "And so after three years of research, I got to the first important experiment in the isolation of fluorine."

Another instance of his indomitable persistence will be remembered by some who were fortunate enough to hear his brilliant lecture, delivered at the College of Physicians and Surgeons, in 1896, when at the invitation of the New York Academy of Sciences, the American Chemical Society, the American Institute of Electrical Engineers, and the College of Pharmacy, he demonstrated the use of his electric furnace and showed his method of making diamonds. As he removed the glowing crucible from within the furnace and plunged it into a mass of water, some of his audience, all scientists, involuntarily moved as if expecting an explosion "Have no fear," said he, "I have performed this experiment without accident over three hundred times."

In friendly appreciation of the bonors done him on that visit to the United States, he sent to the National Museum a collection of sixteen specimens of the products he had obtained by the use of this new instrument of research. The bottles were neatly closed with parchment and on each he had placed his signature. This collection will now be a unique record of his genius. During the same visit Moissan lectured at the Chicago University, and attended the sesqui-centennial celebration at Princeton. He made a study of our educational institutions and on his return wrote a report in which he dilated on the advantages our country was deriving from the liberality of her citizens towards the institutions of learning, and he exerted all his influence, which constantly increased, to bring about the same state of affairs in France, where because of the university belonging to the state, endowments were rarely forthcoming.

In 1889 he published some new researches on the 'Isolation of Fluorine,' and in 1890 he wrote on the 'Fluoride of Carbon.' He received the 'Prix Lacaze' in 1887.

His work on the fluoride of carbon led to his undertaking a study of all three forms of carbon, amorphous, graphite and diamond, and it was with the object of subjecting carbon to excessively high temperatures that he devised his electric furnace, a simple yet all-powerful instrument. With it he undertook a long series of researches on the elements and some of their compounds, the story of which would not only take us through many a chapter of science but also unfold the beginnings of a new art in chemistry. Later, together with Dewar, he liquified fluorine and showed how it would combine spontaneously with hydrogen, thus establishing, at least so far, the lowest temperature at which chemical union takes place. With his electric furnace he has demonstrated not only the indestructibility of many of the elements by the highest heat yet attained, for in a brilliant discourse given at Rome in 1906, at the International Congress of Applied Chemistry, he detailed his distillation of gold, the platinum metals, chromium, iron, nickel, manganese, tungsten, molybdenum and uranium, and before he had also demonstrated in the preparation of titanium carbide and other compounds that chemical combination took place at the highest temperatures at our command. What a technique! It is as if some master of the piano began at the base notes and swept up majestically to the highest treble!

Moissan studied particularly the chemistry of the elements. His isolation of calcium is a further example of new methods of attacking his favorite problem. At the International Congress at Berlin, he gave a lecture which charmed his audience and elicited the highest admiration. He presented his results on the hydrides of the alkalies and the alkali-earth metals. He added with these a further demonstration that hydrogen is not a metal, by showing that potassium and sodium hydrides, KH and NaH, do not conduct electricity, and are therefore not alloys.

We find him passing readily from the realm of inorganic to that of organic chemistry, producing potassium formiate, HCOOK by the direct union of KH and CO_2.

One can never read many pages of his memoirs without being struck with the fact that he made no sharp line of demarcation between the inorganic and the organic; to him they were but one chemistry.

With a host of collaborators Moissan was engaged to the last on a great work, 'Chimie Minerale.' A discourse given at the convocation of scientists at the St. Louis Exhibition in 1904, delivered at the invitation of the government, was a résumé of his introduction of this important treatise.

During the last few years Moissan had forged ahead. He was the president of the International Congress of Applied Chemistry in Paris in 1900. He had previously been appointed to Friedel's professorship at the Sorbonne, and with highest inspiration he was devoting his irrepressible energies to many problems. These were indeed multitudinous. His strictly scientific work was always paralleled with important applications for the well being of the human race. His originality showed him as a leader of thought and also a worker for his fellow beings, inaugurating new industries, planning new manufactures. Few chemists have had wider influence. A medal struck in honor of the twentieth anniversary of the isolation of fluorine was only recently given him by his students and friends.

The recognition of all this was freely given to him not only at home, for France is loyal to her sons, but also abroad; the last highest honor conferred on him was the Nobel Prize, for chemistry, in 1906.

But to those who came within the subtle influence of his personality the fact that an operation for appendicitis, which resulted fatally on February 20, 1907, has severed a friendship with one whose charm of manner endeared him to students, associates and friends alike, has come as a blow so sudden and unexpected that it is difficult to fully realize it. Our sympathy goes out to his noble wife, that faithful amanuensis, who has indefatigably taken down the thoughts of the adored husband and preserved them for us, and to the young man, his only child, who is devoting himself to the same profession.

Resolved, That the New York Section of the American Chemical Society respectfully request the council of the society to fully recognize the esteem in which our honorary member, Henri Moissan, was held by the members and associates of the society, and that appropriate resolutions be sent to his widow and son.

CHARLES G. DOREMUS

JOHN KROM REES

IT is the custom to mark the passing of a well-known man with a short notice of biography; and it is not difficult to recite a list of services, enumerate honors and distinctions conferred by public bodies, or recapitulate scientific researches and publications. But to the writer these things are cold and hard when said of Rees; to him Rees was known best as a friend—that rarest friendship whose beginning is outside the grasp of memory; whose end is a green sod.

Surely, if there exists a relation adapted better than any other to make one acquainted with the good or bad in any man, it is the relation of a subordinate to his chief. During eighteen long years the writer sat at his work under Rees: in all that time there never came down to him an unkind word; never once did a serious difference of opinion arise. In eighteen years one becomes accustomed to any man; the few like Rees one comes to love. These words will fail signally in their purpose, if they do not convey to his sorrowing family such poor consolation as may come from those who feel and suffer with them.

Rees was but fifty-five on his last birthday; in his short life he had served Washington University as a professor five years, and Columbia University twenty-one. He had been president of the New York Academy of Sciences two years, and secretary of the American Metrological Society fourteen years. For six years he was secretary of the Columbia University Council. He was a fellow of the Royal Astronomical Society of London, a member of the Astronomische Gesellschaft of Leipzig, and in 1901 was created a chevalier of the Legion d'Honneur in recognition of his services as one of the judges at the Paris exposition in that year. His principal observational research was a study of the 'Variation of Terrestrial Latitudes and the Aberration of Light,' made at Columbia University, in cooperation with the Royal Observatory, Naples. This was the first application of the method of simultaneous observations at two stations situated on the same parallel of latitude, but separated widely in longitude. The work was continued from 1893 to 1900: the method has since come into general use; and the International Geodetic Association, which includes all civilized governments, has now established four permanent stations to carry it on.

It was also during Rees's administration that the astronomical department of Columbia University undertook the publication of Rutherfurd's valuable series of star photographs. Through his efforts this enterprise was made possible; he took a keen interest in it, and spared no pains to further the work during a long series of years.

In educational matters Rees was at all times most active. His public lectures were frequent. Characterized especially by lucidity, they always attracted large audiences; people came to hear him again and again. But his most lasting contribution to educational development was his establishment of the Columbia summer school of geodesy. It is probable that he was the first to recognize practical field work in this subject as an indispensable adjunct in the training of civil engineers. Here he was at his best: his point of view always that of the genuine man of science, seeking ever the truth for its own sake; never exalting mere technique at the expense of theoretic perfection; never limiting his exposition of a subject to the side having most value from the financial point of view. Students frequently came back to him in later years; they always spoke of his summer school as the most agreeable memory of their college years. Whenever this occurred, Rees was a happy man for days.

At times Rees was persuaded to go beyond his quiet field of activity in the university. His most lasting public service to the nation was rendered as secretary of the Metrological Society, in furthering the introduction of standard or railroad time. The late Dr. F. A. P. Barnard, president of Columbia College, made the following reference to Professor Rees in his testament, dated 1886:

> I give to my friend, Professor John Krom Rees, the watch known as my Cosmic Time Watch, as a mark of my regard and of my appreciation of his zealous efforts for the promotion of metrological reform and for the introduction of the now established system of public standard time.

Rees's attitude towards the scientific work of others was one of extraordinary modesty. To him the past masters of astronomy were not men; they were demi-gods, to be mentioned in respectful accents and with lowered tones. Even living visible masters of the craft commanded from him a degree of respect such as he could not have offered even to crowned royalties. His own work might be as good as theirs, but he could never see it so.

Another marked characteristic was his extreme delight when visited by any one to whom it was possible to do a favor. This was his pleasure. No trouble was too great or time-consuming; no return, not even thanks, was expected. No man ever had fewer enemies: his friends equalled in number those who knew him. When at last the heavy weight of disease was laid upon him he met it as a man should. No querulous repining: regret only that his work must stop; his solace that others would carry the good work on. Mother earth, that he loved well to measure and compute, will give him sleep; to him the peaceful end is surely a release.

Harold Jacoby

COMMISSION ON AGRICULTURAL RESEARCH

At the meeting of the Association of American Agricultural Colleges and Experiment Stations at Baton Rouge last November a resolution was adopted instructing the incoming president of the association to appoint a commission of five persons to inquire into and report to the association on the organization and policy that should prevail in the expenditure of public money provided for experimentation and research in agriculture. It was provided that two of these persons should represent the research efforts of the association; one the United States Department of Agriculture, and two the scientific men not connected with agricultural investigations. The president of the association, Dean L. H. Bailey, of Cornell University, has appointed the following commission, the first two representing persons outside agricultural investigations, the second two representing the association, and the last representing the Department of Agriculture: David Starr Jordan, president of Leland Stanford University, *chairman;* Carroll D. Wright, president of Clark College; H. P. Armsby, director Pennsylvania State College Agricultural Experiment Station; W. H. Jordan, director of the New York State Experiment Station; Gifford Pinchot, forester, U. S. Department of Agriculture.

SEVENTH INTERNATIONAL CONGRESS OF PHYSIOLOGISTS

The seventh International Physiological Congress will be held under the presidency of Professor A. Kossel in the Physiological Institute of the University of Heidelberg August 13, 14, 15 and 16.

Titles of papers and an exact list of apparatus or other articles required for demonstrations should be sent to the Physiological Institute at Heidelberg not later than June 15.

From August 12 to August 17 there will be held an exhibition of apparatus, by members of the congress, directors of physiological institutes and mechanics recommended by members or directors.

The official languages are French, German, Italian and English. The general secretaries are Professors Dastre (Paris), Fano (Florence), Grützner (Tübingen), Porter (Boston) and Sherrington (Liverpool).

THE CENTRAL BRANCH OF THE AMERICAN SOCIETY OF NATURALISTS

There will be held at the University of Wisconsin, Madison, Wis., on March 28, 29 and 30, 1907, meetings of the Central Branch of the American Society of Naturalists in affiliation with the Central Branch of the American Society of Zoologists, the Association of American Anatomists and the Central Branch of the American Society of Botanists. Each of these societies have an interesting program of papers and demonstrations. Papers will be read during the morning sessions on March 28 and 29 and demonstrations will be given in the afternoons. On Thursday evening, March 28, there will be a smoker for the affiliated societies and on Friday evening a dinner of the Naturalists and affiliated societies, at which time the address of the president of the Central Branch of Naturalists will be given by Professor J. Playfair McMurrich, of the University of Michigan.

SCIENTIFIC NOTES AND NEWS

Lord Lister will celebrate his eightieth birthday on April 4, and it is proposed that to mark this event his scientific papers be collected and published. Dr. C. J. Martin, director of the Lister Institute, Chelsea Gardens, London, S. E., will be glad to receive the names of those who desire to take part in this tribute.

The council of the Royal Society has nominated for membership the following fifteen candidates: Frank Dawson Adams, Hugh Kerr Anderson, William Blaxland Benham, Lord Blythswood, William Henry Bragg, Frederick Daniel Chattaway, Arthur William Crossley, Arthur Robertson Cushny, William Duddell, Frederick William Gamble, John Ernest Petavel, Henry Cabourn Pocklington, Henry Nicholas Ridley, Grafton Elliot Smith, William Henry Young.

Professor W. A. Tilden, F.R.S., professor of chemistry and dean of the Royal College of Science, London, has been elected a member of the Athenæum Club for 'distinguished services in science.'

Dr. George E. Hale, director of the Solar Observatory of Mount Wilson, has been elected one of the alumni members of the corporation of the Massachusetts Institute of Technology.

Dr. Frederic T. Lewis, of the Harvard Medical School, Boston, Mass., has been appointed editor of *The American Naturalist*. The editor desires contributions from 'all naturalists who have anything interesting to say.' Candidates for the higher scientific degrees are invited to contribute concise summaries of the special literature pertaining to their chosen topics. All manuscripts should be sent to the editor.

It is announced that the Carnegie Institution of Washington will erect the laboratory for the study of human nutrition which is planned adjacent to the Harvard Medical School. The work is under the direction of Professor F. G. Benedict, of Wesleyan University.

The Rumford Committee of the American Academy of Arts and Sciences has recently made the following appropriations in aid of researches on light and heat: To Professor F. E. Kester, of the Ohio State University, $315, in aid of his research on the thermal properties of gases flowing through porous plugs; and $400 to Dr. Harry W. Morse of Harvard University, in aid of his researches on fluorescence.

At Oxford University grants have been made from the Craven Fund of £60 to Mr. Günther, M.A., fellow of Magdalen, to assist his archeological investigations in Southern Italy; of £70 to Mr. Burrows, M.A., of Christ Church, to assist his investigations in Bœotia with a view to determining the site of the Delium; and of £40 to Mr. Dodd, B.A., University College, to assist his study in Germany of the coinage and history of the Antonine period.

The *British Medical Journal* states that the colleagues of Professor Ramón y Cajal and Professor Camillo Golgi, to whom was awarded the Nobel Prize for medicine, intend to mark their appreciation of the honor done their distinguished countrymen. On February 22 a meeting was held in the Colegio de Medicos, Madrid, at which it was agreed that a public subscription should be opened for a gold medal to be presented to Professor Ramón y Cajal; that a volume containing reports of original researches should be prepared under the direction of a committee consisting of Professors Calleja, Gomez Ocaña, Oloriz, Jimeno and San Martin, and that the government should be asked to confer a life senatorship on Professor Ramón y Cajal. The committee appointed to make arrangements for doing honor to Professor Golgi has decided that a scholarship bearing his name should be founded in connection with the University of Pavia.

Dr. John Collins Warren, who has been instructor and professor of surgery in the Harvard Medical School since 1871, will become professor emeritus at the close of the present year.

Dr. Raymond Pearl, instructor in zoology in the University of Pennsylvania, has accepted an appointment as biologist in the Maine Agricultural Experiment Station, Orono, Maine, beginning with the next academic year. The appointment is made under the provisions of the Adams act. The work will be entirely research along the lines of plant and animal breeding. Especial attention will be devoted to an investigation of the principles of inheritance in poultry, a line of work which on its practical side has already been extensively developed by the station.

DR. FRIEDRICH MÜLLER, professor of medicine in the University of Munich, was tendered a complimentary dinner by prominent American physicians on March 8 at the University Club, New York. Dr. Francis P. Kinnicutt was toastmaster, and addresses were made by Drs. E. G. Janeway, A. Jacobi, W. H. Welch, L. F. Barker and by the distinguished guest. On March 9 Professor Müller spoke before the Harvey Society, and beginning on March 11 gave a series of six lectures on chemical pathology under the Herter foundation before the students of the University and Bellevue Hospital Medical College. He will visit Baltimore, New Haven, Boston, Albany, Montreal, Toronto and Cleveland.

PROFESSOR CHARLES S. MINOT, of the Harvard Medical School, has visited the Universities of Missouri, Kansas, Nebraska, Iowa and Louisville, and at each of these has delivered an address before the medical faculty and students on the new ideals of medical education for which the new laboratories of the Harvard Medical School stand. He also delivered before the Sigma Xi societies of the four state universities an address on 'The Biological Interpretation of Life,' speaking at Columbia, Mo., on the ninth of February; at Lawrence, Kansas, on the fourteenth; at Lincoln, Nebraska, on the sixteenth, and at Iowa City on the eighteenth. During his visit at Kansas University Professor Minot also delivered a course of three lectures on 'The Problems of Age, Growth and Death,' and a morning chapel address on the 'Functions of Consciousness.' At Louisville he participated as a representative of the university at the combined dinner of the Harvard and Yale Clubs, held on the evening of February 21.

PROFESSOR ROLAND THAXTER, of Harvard University, addressed the Botanical Club, on February 13, on 'A Botanical Trip to South America.'

PROFESSOR BECHTEREW, of St. Petersburg, has examined the brain of the late Professor Mendeléeff. It is said to weigh more than 1,200 grams, and to be remarkable for the number of its convolutions.

WE learn from the *Journal of the American Medical Association* that a research professorship in the Liverpool School of Tropical Medicine has been proposed as a memorial to Dr. Joseph Everett Dutton, who did valuable work in four successive expeditions sent out by this school to Nigeria, the Gambia, Senegambia and the Congo Free State. He described *Trypanosoma gambiense,* the parasite since recognized as the cause of sleeping sickness, and gained high distinction for himself and a world-wide renown for his school. While investigating tick fever, previously little known, he contracted that disease and died. The committee in charge of the fund says: "All who knew Dr. Dutton agree that the most fitting form which a memorial can take is one which will help to continue those researches in tropical medicine for which he gave his life." It is proposed to raise $50,000, and over $20,000 has already been pledged.

THE sum of $30,500, given by more than 760 alumni as a Shaler memorial fund, has been accepted by the president and fellows of Harvard University. The fund will commemorate the long and great services of Professor Nathaniel Southgate Shaler. A sum has been set aside to procure a memorial tablet to be put in the geological section of the University Museum, or some other suitable place; and the income of the balance will be used for the benefit of the division of geology in support of original research and in the publication of the results of research.

DR. ALLAN MACFADYEN, head of the Biological Department of the Lister Institute of Preventive Medicine and one of the leading English bacteriologists, died on March 1, at the age of forty-six years, from infection contracted in his laboratory.

The foreign papers state that the reporter of the inter-ministerial committee recently appointed by M. Barthou, minister of public works, to examine the various proposals for making a shorter route to Italy and to Central Europe has concluded in favor of the piercing of Mont Blanc. The report of the committee will now be discussed by the government, which will eventually propose a bill to parliament.

The Goldsmith's Company has contributed the sum of 1,000*l.* to the research fund of the Chemical Society, London.

A grant of £1,000 has been made by the Mercers' Company to the Imperial Institute for scientific research in regard to the economic products of British Colonies and Protectorates.

UNIVERSITY AND EDUCATIONAL NEWS

At the recent session of the legislature $25,000 a year was added to the appropriation for maintenance of the University of North Carolina and $25,000 a year for building and repairs. The annual appropriation for support is now $70,000.

The Alabama state legislature has recently passed a bill, since signed by Governor Comer, appropriating $400,000 for buildings at the University of Alabama (at the rate of $100,000 annually during this and the next three years), and $25,000 a year towards maintenance. The latter is a continuing appropriation. This is the first time in the university's seventy-five years' history that the state has made a distinct appropriation for the regular work of the institution.

In the state of West Virginia the new tax laws have yielded less revenue than the state has had formerly. The legislative appropriations for educational institutions have consequently been kept down to the level of previous years and have been further greatly reduced by the governor's veto. The state university, in spite of its recent rapid growth, receives considerably less than two years ago. To meet expenses the board of regents has been compelled to institute a system of tuition in the professional schools. The fund for salaries in the medical school was cut out by the governor and no way has yet been found for making good the deficit.

At the semi-annual meeting of the board of trustees of Princeton University on March 15, gifts amounting to more than $75,000 were announced. Among the gifts was one of $10,000 from Mr. Morris K. Jesup to increase the endowment of the Morris K. Jesup fund. Through the generosity of alumni the Carpenter building, situated on Nassau Street, has been purchased for the university.

At a recent meeting of the board of trustees of Carroll College it was announced that Mr. Andrew Carnegie had offered to give the last $25,000 of $100,000 towards increasing the endowment. It was reported that $75,000 had been pledged conditional, however, on raising a total of $175,000. It is expected that the full amount of $175,000 will be raised by June, 1908.

The Massachusetts Institute of Technology has received a further anonymous gift of $5,000 for the support of its Sanitary Research and Sewage Experiment Station.

Dr. Edward Charles Jeffrey has been promoted to a professorship of plant pathology at Harvard University.

Reginald Aldworth Daly, head geologist of the Canadian internal boundary commission, and formerly instructor in Harvard University, has been appointed professor of physical geology at the Massachusetts Institute of Technology.

Dr. A. D. Cole, professor of physics at the Ohio State University, has accepted a call to Vassar College, to succeed Professor Cooley who retires at the close of the academic year.

William D. Ennis has been appointed to the chair of mechanical engineering at the Polytechnic Institute of Brooklyn.

Dr. James Martin Beattie, senior assistant to the professor of pathology, University of Edinburgh, has been elected by the council to the chair of pathology, in the place of Dr. Cobbett. Dr. Cobbett resigned the chair on his appointment as lecturer on bacteriology at Cambridge.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MARCH 29, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE FACTORS OF SAFETY IN ANIMAL STRUCTURE AND ANIMAL ECONOMY[1]

THE living animal body is like a machine in action. Like a machine its structures are subject to a variety of stresses, and like a machine the work is accomplished by an expenditure of energy derived from a supply of fuel. I intend to discuss in this lecture, whether, as in the human-made machines, the structures and functions of the animal mechanism are provided with factors of safety. The term 'factor of safety' is employed in engineering to designate the margin of safety required in the building of engines, bridges, houses, etc. For instance, in designing a boiler, if the tensile strength of the steel of which the plates and stay-bolts are made, is 60,000 pounds per square inch, the actual stress which is allowed for the work of the boiler should not be more than 10,000 pounds per square inch for the plate and not more than 6,000 pounds per square inch for the stay-bolts; that means the stress to which the plates or the bolts may be exposed in the boiler should only be one sixth or one tenth of the actual strength of the steel. The factors of safety are said to be here six for the plate and ten for the bolts. In some instances the required factors of safety may be as low as three, in other cases again they may be as high as twenty and even forty. The character of the stress to which the structures might be subjected is an important point in deciding upon the

[1] Harvey Society lecture, delivered at the New York Academy of Medicine, December 15, 1906.

size of the margins of safety. Structures, for instance, which are to be employed for alternating loads require high factors of safety; the highest margin of safety is required when the structures are subjected to rhythmic shocks. In constructing a bridge or a machine it is then calculated that the structures should be capable of withstanding not only the stresses of reasonably expected maximum loads, but also the stresses of six or ten times the size of such loads. The factor of safety has its foundation in our ignorance of what might happen and in the reasonable desire to meet unexpected contingencies. Some writers are therefore inclined to designate the factors of safety as factors of ignorance.

It is obvious that the factors of safety are applicable not only to the structures, but also to the supply and expenditure of energy of the machine. The supply of fuel is calculated to have the engine in readiness not only for expected maximum work, but also to be capable of meeting unexpected contingencies. On the other hand, when there is no exceptional need for it, no engine is allowed to perform maximum work; this economy here is again a factor of safety.

Are the structures and the functions of the living animal body provided with such factors of safety? As far as I know, that question has never yet been clearly raised, and certainly was never made the subject of a direct investigation. There is, however, no lack of casual remarks bearing on that problem and these are manifestly unfavorable to an assumption of the existence or requirement of factors of safety in animal organisms. On the contrary, there are many to whom it is apparently self-evident that nature is economical and wastes neither material nor energy. Theories and practical suggestions are based on such a view as a premise which seems to their authors to require no special proof. Verworn, for instance, asserts that the assumption of special inhibitory nerves for skeletal muscles can be rejected *a priori,* because the presence of such nerves would be a waste of matter and energy and in contradiction with the prevailing principle of economy in the animal body. Another instance is the extreme position held by some recent writers with respect to the supply of energy to the animal machine. Factors of safety, maximum or optimum supply of fuel, do not come in for a consideration in the discussion of these writers. The argument is directed against the use of a dietary standard which represents the average mean supply of energy, the minimum supply of food being considered as the ideal standard of diet. As is known to all of you, Professor Chittenden and his co-laborers have carried out nutrition experiments of long duration upon a number of men. The essential feature of these experiments was the use of a low proteid diet; in some instances the diet was also combined with a considerable reduction in the caloric values of the food. All the subjects of the experiments retained their usual health. Professor Chittenden admits that the diet used in these experiments, especially with regard to the proteid intake, represents the minimum requirement of the human body; he, nevertheless, earnestly advocates its acceptance as a general standard of diet, assuming *a priori* that the minimum food with which a number of men can manage to live for some time without harm is the desirable standard of supply of energy for all animal machines. Whereas in the economy of the human-made mechanisms and, in fact, in the economies of all human organizations, decrease in supplies and increase in expenditure lead invariably to disaster, it would seem that in the physiological economy of the living mechanism such a procedure may even lead to a greater efficiency of the mechanism. Professor Irving Fisher tells us recently that nine Yale students under the influence of prolonged mastication of a diet greatly reduced in proteid and in caloric values, gained very much in endurance in performing certain physical tests.

Is there, indeed, a difference between the economies of human-made organizations and those of the living organism? I have stated above that the factors of safety in mechanical constructions are after all only factors of ignorance. Possibly wise nature constructs her organisms on such an efficient principle which permits the accomplishment of the greatest amount of work on a minimum supply of material and energy. It would be a fascinating distinction between a dead mechanism and a living organism—if true.

The subject of this lecture will be an investigation of this question, an investigation whether the structures and functions of the animal organism are constructed with a special consideration for the greatest economy or for the greatest safety. Or to leave the purposefulness of the organization out of discussion, I may perhaps put it more correctly by saying that it will be essentially an investigation into the ratios of the supply of material in many organs of the body to the amount of work they are expected to perform. I believe that the investigation may lead to some instructive general conclusions of a theoretical and practical character. As already stated, the problem seems to me to be new and, as far as I know, no original investigations were carried out with the special purpose of solving it. There are, however, a great many well-established facts brought out in theoretical and practical work undertaken for other purposes, which are, nevertheless, capable of throwing a good deal of light upon our problem. Such facts have the advantage of being unbiased witnesses, since no preconceived theory was at the bottom of their discovery. My task will consist in reviewing these facts as far as they are available, or more correctly, as far as they are known to me, and bringing them impartially in proper relation to our problem.

I shall commence with the plain tissues of the body. In the multitude of studies on these tissues there are not many investigations which could be utilized for our purpose. However, a series of careful investigations, recently published by Triepel, have an intimate bearing on our problem. Triepel investigated the elasticity and resistance of several tissues, like muscle, tendon, elastic tissue, bone, cartilage, etc. For us the following statements are of special interest. For muscle, tendon and elastic tissue Triepel found that the maximum stretching which may occur in the animal body is not far below that degree which can cause tearing of these tissues. The resistance of bones and cartilages to a crushing stress is, however, far above any stress which might occur in normal life. With regard to muscle, tendon and elastic tissue it appears, therefore, that the structures in themselves have practically no factors of safety above the maximum stress to which they might be subjected. Any unexpected tension above the maximum occurring in ordinary life might lead to a rupture of these tissues. Triepel, however, calls attention to the fact that the degree of stretching of these tissues is greatly limited by their connections with the structures surrounding them, especially by the skeletal parts. These limitations will for the most part prevent these tissues from reaching their breaking point. We may then say that muscle, tendon and elastic tissues have no factors of safety in the structures themselves; but they are provided, nevertheless, with some such factors by their connections with other tissues. The bones and cartilages, on the other hand, which are but little influenced by other tissues, are provided with a very large margin of safety over the stresses to which they might be exposed normally. Triepel here makes a remark which has a direct bearing upon the problem with which we are dealing. He says that the large surplus of material in bone and cartilage shows that nature does not follow the law of obtaining a result by the smallest possible means.

It is worth noticing that the large margin provided here can not have the object of offering protection against unexpected contingencies coming from within the body, as these, according to Triepel, will never reach even the yield-point of these tissues. The protection is here provided against contingencies coming from without, against injuries of external origin. It is a protection not against an internal, a physiological, calamity, but against an external, so to say pathological, contingency.

A sufficient number of readily available data for the study of our problem we find in researches upon complex tissues or organs. We shall begin with the bilateral mechanisms. Here are, in the first place, the kidneys. Every medical man knows now that one kidney can be removed with entire impunity, if the other kidney is normal. The amount and the composition of the urinary secretion remains practically unaltered and this even soon after the removal of the kidney. That can only mean that normally the kidney has an abundance of tissue which can do at a moment's notice at least twice the normal amount of work. From the experimental work of Tuffier, Bradford and others we know that at least two thirds of both kidneys may be removed without serious detriment to the animal's life and to the secretory function of the kidneys. At the same time we must remember that the normal secretion represents by no means the minimum amount of work of the kidney. We know that the average quantity of the urine as well as the normal quantities of its various constituents may be greatly reduced without any visible detriment. In fact there may be anuria for many days without any serious symptoms and perhaps also without serious consequences, if the anuria be not due to a disease of the kidney, but to such causes as hysteria, calculus, reflex, or compression. The margin of safety in the tissue of this eliminating organ amounts at least to twice its normal need.

This would seem to be an unreasonable luxury, a waste. But what a blessing. For a score of years, or more, in many of us the kidney is gradually losing some of its valuable material from one cause or another without any symptom, without a reminder sufficient to spoil our pleasure of life or to hamper our activities. Not until that luxurious surplus is approaching its exhaustion, do we get a warning. But then our work is mostly done and our time limit nearly reached.

Next we shall consider the lungs, an organ of supply and elimination of first order. We all know that life may continue though a great part of the lungs be destroyed, if only the disease which caused the destruction come to a standstill. We know that in some cases of pneumonia one lung can be entirely consolidated without seriously impairing the process of ventilation. Furthermore, a patient whose thorax was freely opened to evacuate a one-sided pleural abscess has after the opening less dyspnœa than before. In empyema as in pneumonia, it is essentially the infection and intoxication with their reactions which cause the apparent disturbance in the respiratory mechanism and not so much the mechanical interference with the ventilation of the corresponding lung. Since the classical experiments of Regnault and Reiset many investigators have stated that compression of one lung or a unilateral pneumothorax exerts very little influence upon the respiratory exchange of gases. Hellin reported recently a series of experiments on rabbits in which the right lung was completely removed. The right lung of the rabbit has four lobes and is much larger in volume than the left; that means that more than one half of the lung tissue was removed. Most of the animals survived the operation and some lived a year and longer. Except for a temporary moderate dyspnœa lasting only an hour or two the animals were in a normal condition, and the respiratory quotient continued to be after the removal of the lung exactly as it was just before the operation. We see then that the normal process of respiration can be carried out with at least one half of the lung tissue and probably with a good deal less. We have here with regard to the quantity of tissue a factor of safety equal at least to two, which does not appear to be an excessive margin considering the importance of the function which that tissue has to carry out.

Of the bilateral organs of reproduction we know from numerous surgical operations that the removal of one ovary or of one testicle does not interfere in the slightest degree with the corresponding functions of the individual. For the female organs it has been frequently established that even a small part of one ovary is sufficient to carry on the function of menstruation and conception. In fact, there are a number of reliable cases on record in which pregnancy occurred after the removal of both ovaries, which cases were explained by the assumption that some particle of normal ovarian substance was caught in the ligature and retained in the body, and this fragment was then sufficient to carry out the function of ovulation and conception.

For the testicles we may safely assume also that a small fragment of one testicle left in the body would be capable of carrying on the function of reproduction. But I did not come across experimental or surgical data which directly bear out this assumption. There are definite data with regard to the secondary sexual characteristics in fowls. If in the process of castration some fragment of one testicle is left, the cock, according to Foges and others, does not lose the comb and other secondary sexual characteristics. However, these secondary characteristics are probably connected with the internal secretion of these organs, and their persistence might not be a sufficient proof for the persistence of the function of reproduction. At any rate, it is sufficiently evident, especially as will be seen later, that the tissues of the organs of reproduction are greatly in excess of the maximum need of the chief function of these organs.

Among the bilateral organs there are two whose functions are carried on exclusively by internal secretion, I mean the thyroid and the adrenal glands. We do not notice their activity while they are present, but we recognize their importance by the serious effects which follow their removal. The complete removal of both thyroid glands is followed either by acute symptoms of a tetanic type or by chronic states which are known under the names of myxœdema and cretinism. It is, however, a well-established fact that the removal of four fifths or even five sixths of both thyroids is not followed by perceptible consequences, which means that one fifth or one sixth of the entire gland is amply sufficient to provide the body with the indispensable substance contained in the secretion of the gland. It was just on that account that at first the experimental results showing the importance of this gland were disputed by some observers: small accessory glands were hidden in some cases which made the apparently complete removal of both thyroids ineffective. The thyroid gland possesses, accordingly, four or five times more tissue than necessary for the complete maintenance of health and life of the animal.

In recent years some of the symptoms following the removal of the thyroid gland, especially the acute manifestations, are ascribed to the simultaneous removal of the epithelial bodies known as parathyroids. They are four in number. I do not know of a statement dealing directly with the question how much of the parathyroids has to be removed in order to bring out the pathological effects. However, in the dog the parathyroids are imbedded in the thyroids, two in each lobe, and some of the acute symptoms following the removal of the thyroids in dogs are ascribed as stated above, to the simultaneous removal of the parathyroids. By the removal of four fifths of the thyroids surely two and probably three of the parathyroids are also removed. But since the removal of four fifths of the dog's thyroids is not attended with any evil consequences, we may also conclude that a good deal of the substance of the parathyroids can be dispensed with without any ill effects.

For the suprarenal glands it is now well established that their removal is absolutely fatal to the animal. Death follows within nine to thirty-six hours after the extirpation of the glands, under conditions of low blood pressure, extreme muscular weakness and exhaustion. But the removal has to be complete; if one tenth of the glands or even less is left in the body, the animal shows no pathological symptoms. Here again, as in the thyroid, this fact caused the divergence of opinion which sprang up soon after Brown-Sequard made the discovery of the importance of this ductless organ. In many of the experiments bits of the tissue of that organ were left behind; besides, many an animal hides somewhere accessory organs of the same type. For the adrenals then it is evident that the body possesses indispensable tissue at least ten times as much as is necessary for the maintenance of normal life.

The brain is built on a bilateral plan. In former years, when following the lead of Flourens and as a reaction to the teachings of Goll, the brain was considered as a uniform organ, attending only to one function, some facts seemed to demonstrate indeed that there is a great excess of tissue in that organ, since the older experiments of Flourens and newer experiments of Goltz indicated that large parts of the brain could be removed without serious injury to life. To-day we know that the brain presents a collection of many organs, of many centers, the injury of each of which is followed by sensory or motor disturbances in definite areas of the body. As a whole, the bilateralness of the hemispheres does not mean the same as bilateralness in other organs, namely a duplication of tissue for one and the same function. One hemisphere attends to the needs of one side; for instance, the motor areas of the right arm or right leg are located in the cortex of the left hemisphere, and those of the left arm and left leg are located in the right hemisphere. The same is true of the subcortical centers and apparently also of the medulla oblongata.

To this rule there is, however, an exception for the motor organs having in charge such muscles or group of muscles which normally contract on both sides simultaneously. The motor area of one side can take charge of the muscles of both sides. Such is the case with the motor areas of the respiratory muscles, the muscles of the larynx, of deglutition, etc. An injury to the motor areas of these muscles in one hemisphere only does not cause paralysis of these muscles. An instance well known to practitioners is the one-sided injury to the motor area of the orbicularis palpebrarum. The muscle, as a rule, is not paralyzed by such an injury, at least not when the muscles on both sides contract simultaneously. As is well known, the absence or presence of paralysis of that muscle in cases of facial paralysis serves as a means to diagnose whether the paralysis is of central or peripheral origin.

An example of an uneconomical principle, to use the expression of Verworn, we find in the bilateral innervation of certain viscera by the pneumogastric nerves. For instance, the normal rhythm of respiration is completely changed when both vagi are cut, whereas when only one vagus is cut, the respiration remains normal. Apparently one vagus nerve is amply sufficient to carry on the regulation of respiration. A similar condition obtains with regard to the heart beats. For certain animals, the dog for instance, the vagi carry on an inhibitory tonus. When both vagi are cut, the heart beats are considerably increased in frequency; when only one vagus is cut, the rate does not change. Here again a single vagus nerve is sufficient to carry on that inhibitory tonus. Still more striking is the following fact. After cutting both vagi, the animal dies within a day or two from aspiration pneumonia, whereas when only one vagus is cut, the animal not only survives the operation, but is for all purposes apparently perfectly normal. One vagus nerve then is amply sufficient to carry on all these functions; but the body is provided with two nerves. According to Verworn this should be an example of a violation of the principle of economy in the animal body and its existence should be denied *a priori*.

Further examples of the ample provision of the structures of the body with factors of safety we meet also in the organs of the body which are not built on the bilateral plan, the unsymmetrical organs. We shall mention here first the pancreas with respect to its internal secretion. It is now common knowledge that the complete removal of the pancreas leads to glycæmia and glycosuria. But here we note the fact that if a small part of the gland, say not more than one tenth, is left in the body, no ill effects follow such an extirpation. One tenth of that gland is capable of completely protecting the animal against glycosuria; but the body is, nevertheless, provided with ten times as much.

Another striking example is the liver. This organ has many important functions. It converts the sugar into glycogen; it converts the poisonous ammonia compounds into the comparatively harmless urea. It forms bile which carries out poisons from the body, removes waste products, assists in some way or another in the absorption of fats, aids in the digestion of proteids and what not more. But Ponfick found that the removal of one half of that organ practically does not interfere with the life of the animal, and the successful removal of even three fourths of the organ does not produce symptoms indicating that any of its functions are seriously interfered with. That organ then is provided with an abundance of active tissue considerably in excess of its normal requirements.

Similar striking examples of factors of safety we meet with in the luxurious construction of the gastro-intestinal canal.

The entire stomach or the greatest part of it has been removed in animals and man without interfering with digestion and nutrition. Of the small intestines large parts have been resected without serious consequences. In human beings the largest part removed measured, I believe, over three meters, and Erlanger and Hewlett have studied the metabolism of dogs seven or eight months after the removal of seventy or eighty per cent. of the movable part of the small intestines. Three fourths, then, of the small intestines are almost a luxury to the body. We need, not, perhaps, speak of the fact that surgeons have removed large parts of the colon without ill effects. From the present attitude of bacteriologists and physiological chemists towards the activities of the large intestines one is led to believe that the body might do best without any part of that organ. Be this as it may, it is quite sure that the digestive canal is provided with a good deal more structure than is required for the maintenance of its function.

Here we shall discuss briefly also the luxurious provision of the alimentary canal with digestive ferments. There are two proteolytic ferments: pepsin and trypsin, to which we may add also erepsin, a ferment found by O. Cohnheim in the mucous membrane of the small intestines and which is said to be capable of splitting albumose into amino acids. There are two amylolytic ferments: the ptyalin of the salivary glands and the amylopsin of the pancreas. As to lipolytic ferments, the steapsin of the pancreas is not the only one of that kind which reaches the contents of the digestive canal. Thus several investigators have recently confirmed the statement of Volhard that the fundus of the stomach secretes a lipase which is capable of splitting emulsified fat. Lipase is contained also in the liver and in the bile.

Now there are a number of experiments and clinical facts which go to show that digestion can continue in normal fashion even if one half or at least a good part of these ferments are eliminated from the digestive tract. Older and recent experiments have established the fact that the removal of the salivary glands has no effect upon the digestion. We know, on the other hand, that after removal of the pancreas or in cases of isolated destructive diseases of this organ the digestion of carbohydrates is not disturbed. Normally, therefore, there is a superabundance of amylase in the digestive canal. As to the proteolytic ferments, we have already mentioned that the complete removal of the stomach does not disturb digestion. Furthermore, in cases of achylia gastrica, in which the stomach secretes neither hydrochloric acid nor pepsin, the proteid digestion is apparently normal. On the other hand, we know that the elimination of the pancreas does not affect palpably the proteid digestion. With regard to lipase, clinical pathology was teaching that in cases of disease of the pancreas the stool contained fat, which would seem to indicate that in the absence of the pancreatic lipase no other lipolytic ferment was present in sufficient quantity to split completely the ingested fat. However, in a very recent study of Umber and Brugsch it was shown that the fat-splitting function is carried on even in the absence of the pancreas in a normal way.

We are then surely justified in claiming that the various digestive ferments exist in the alimentary canal in quantities far above the necessities for the digestion of a normal amount of food.

All the numerous organs and complex tissues which we have just passed in review are built on a plan of great luxury. Some organs possess at least twice as much tissue as even a maximum of normal activity would require. In other organs, especially in those with an internal secretion, the margin of safety amounts sometimes to ten or fifteen times the amount of the actual need. An extreme degree of superabundance and actual wastefulness we meet with in the organs and functions having charge of the continuation of the species. Let us illustrate it by the following few data. The ovum exists for the purpose of reproduction. Assuming that the sexual function of a woman lasts forty years and assuming, further, that every ten months of these years would be taken up by a pregnancy, then only fifty ova would be required of the ovary. But assuming even that a regular menstruation is an essential and indispensable part of the sexual function, then five hundred ova would be the maximum that the function of reproduction could use. Nevertheless, we find that the ovary of the new-born female child possesses between 100,000 and 400,000 eggs, and at the time of puberty there are still about 30,000 ova ready to enter upon their possible mission. That is, the ovary contains at puberty sixty times more ova than the body could possibly ever employ. But there is an incomparably greater waste in the provision of the male germ. According to Rohde each ejaculation contains 226 millions of spermatozoa. Now we know that of all these legions only one single spermatozoon is required and only one can be used. What a marvelous waste of living cells for the sake of assuring the perpetuation of the species. But there are some attenuating circumstances. With a velocity of only 0.06 of a millimeter per second, with the dangers of crossing the sea of fatal acid vaginal secretions and with a resistance to the onward progress offered by the cilia of uterine epithelium swaying in the opposite direction, not too many of the storming millions stay in the race and have a chance to reach the goal. At any rate, it is not by economy, but by immense waste of cell life that the chance for continuation of the species is assured.

In striking contrast to the extreme luxuriousness of provision of tissue in the organs previously described stands out the comparative scantiness of cell tissue in some organs—if we may call them so—of the central nervous system. The centers of the medulla oblongata, for instance, present such minute bodies that hardly a part of any center could be injured without endangering the entire function. Any injury to the respiratory center suspends immediately and permanently the function of respiration. The possible existence of some respiratory centers in the spinal cord does not alter the practical result. The same applies to the center of deglutition. The blood pressure, as we shall see later, is provided with quite a large number of safety factors. However, the immediate effect of an injury to the vasomotor center is a dangerous drop in blood pressure, the restitutions and compensations over which the organism commands are not forthcoming until after a long interval. We may point out, however, that the central nervous system is provided externally with factors of safety against two of its main enemies: it is protected by a bony encasement against any physical injury and especially is the medulla oblongata well hidden away, and it is protected by an abundance of blood vessels against dangers of anæmia.

Following the old divisions of the organs of animal life in reproductive, vegetative and animal systems, we may say, perhaps, that the reproductive system is provided most and the animal system is provided least with factors of safety, while in the vegetative system, which in that regard occupies a middle position, those organs which seem to be less well differentiated, like the organs for internal secretion, seem to be provided with a larger surplus of tissue.

The complex apparatus of circulation is well provided with factors of safety. In the first place, the animal body possesses a good deal more blood than it requires for its work. It is known by experimental evidence and clinical observations that nearly one half of the blood can be withdrawn without serious consequences to the life of the animal. As a further factor of safety in this regard we might register the ability of the blood to recover its loss very rapidly.

Furthermore, the capacity of the entire system of blood vessels in a completely relaxed state is again much greater than the volume of blood of the body. It is this difference between the volume of blood and the volume of the vessels which greatly facilitates the circulation of the blood and the proper nutrition of the various organs of the body. On the basis of this difference large quantities of blood can be thrown at once and with ease into the splanchnic region, into the skin or into the working muscles. After a local injury or infection in a very brief time for the sake of repair or defence hyperæmia sets in, and vessels which were not noticeable before become fairly visible. An instance of a similar order is the wide-spread institution of collateral circulation. Around an anemic focus blood vessels which previously were hardly visible become full and large to meet the threatening danger of necrosis of the neighboring anemic tissues. All these devices which spring into activity only under special exigencies, are manifestly factors of safety and are made possible by a superabundance of bloodvessels.

The difference between blood volume and capacity of vessels is an indispensable factor of the circulation, and its permanence is assured by many devices. Thus, for instance, any artificial increase of the volume of blood is immediately corrected through the chief eliminating organs, or through the secretory glands, or even by throwing some of the surplus serous fluid temporarily into the lymph spaces and serous cavities. Œdema, ascites and hydrothorax are sometimes not parts of the affliction, but means of repair.

Furthermore, existence of the difference between vascular capacity and quantity of blood is made possible only by a wonderful mechanism which controls in every part of the body the mutual adaptation of blood and vessel—the so-called vasomotor apparatus. It causes the dilatation of the vessels in the part of the body which requires and is to receive more blood, at the same time causing a constriction of the vessels in a part which can spare some of its blood. This mechanism is so important that it is again guarded by an abundance of factors to assure its safety. There is a vasomotor center in the medulla oblongata; when this is destroyed, a number of vasomotor centers in the dorsal medulla assume control; when they are eliminated, the sympathetic ganglia take over the command, and when they too drop out, the vascular wall itself attends to the proper regulation and adaptation of the capacity of the vessels to the volume of blood.

Finally, the chief motor mechanism of the circulation, the heart, is a clear instance of an organ provided with a superabundance of volume and force. Normally it is in a state of tonus and receives only a moderate volume of blood which it throws into the aorta with no great hurry and with an expenditure of only a moderate amount of energy. But at any moment it is ready to receive many times the usual volume of blood, is ready to double or treble the rate of its beats and is capable of developing nearly any amount of energy which the situation might require of it. It is a wonderful, prompt, adaptive motor mechanism with a good reserve of force.

We have, then, in the circulatory system many instances of provisions with factors of safety to assure nutrition of all parts of the body in all states and conditions. An abundance of blood, a superabundance of blood vessels, a vast provision of factors for the safety of the adaptation of the two to one another and a great reserve of motor force for transportation and distribution of the blood.

The multiple mechanisms existing for the care of the vasomotor apparatus lead us to the following considerations. The internal motor organs of the body like the gastrointestinal canal, the heart, the uterus, etc., are provided with central motor innervations as well as with local motor mechanisms. In all cases it has been shown that the movements of the organs continue also after the severance of the connections with the central nervous system. Thus the heart continues beating after section of both vagus and accelerator nerves, the peristalsis of stomach and intestines continues after cutting the vagi and the splanchnics, and pregnancy and delivery take a normal course after complete destruction of the spinal cord. On the basis of these facts it is now generally assumed that the extrinsic innervations of these organs have only a regulating function, while the real motor function is invested in peripheral devices, be they of neurogenic or of myogenic character. This conclusion is obviously based on the supposition that the function of an organ is carried on only by a single mechanism. Hence the fact that the motor work is carried on after eliminating the extrinsic nerves seems to be sufficient evidence that they can not form an integral part of the motor function.

These conclusions are fallacious. There is an abundance of instances in which one and the same function is cared for by more than one mechanism. But we need only refer to the vasomotor apparatus. It was known before and it has been very recently conclusively demonstrated again by Magnus that, after eliminating the influences of the sympathetic and the central nervous system, the blood pressure is well taken care of by the peripheral mechanism of the walls of the blood-vessels. Nevertheless, nobody doubts that the vasomotor centers are integral parts of the vasomotor mechanism. Why this difference of views for the different organs of the body?

The subject is evidently an important one; but we shall not enter into a further discussion of it. The remarks were made to illustrate the importance of the conception that in the animal body one function is not infrequently cared for by more than one mechanism. It is capable of profoundly affecting the views on many vital biological problems.

We shall cite a few more instances in which two or more parallel mechanisms exist for the accomplishment of one function. I may be permitted to mention in the first place the function of deglutition. As was shown by us about twenty-five years ago, fluids and semifluids are squirted down from the mouth to the cardia by the force of the contraction of the mylohyoid muscles, but they can also be carried down by the peristalsis of the œsophagus. Of the latter there are again, as I have recently shown, two kinds: a primary peristalsis which runs independently of the integrity of the œsophagus and a secondary peristalsis which is closely connected with the integrity of the tube and which is more resistant to certain detrimental influences. It will probably be shown before long that the œsophageal wall alone is also capable of contributing to the function of carrying the food down to the stomach.

The function of the pancreatic secretion seems to be an instance in which mechanisms of a different type are sharing in its management. It has long been established that the pancreatic secretion stands under the influence of the central nervous system. Recently it was discovered by Bayliss and Starling that an intravenous injection of secretin causes a considerable increase of pancreatic secretion. Secretin is an extract made of the duodenal mucosa with an addition of hydrochloric acid. It is assumed that this substance is produced normally when the acid chyme comes in contact with the mucosa of the duodenum, and that by its absorption into the circulation it is one of the normal causes of pancreatic secretion. Now the effect of the secretin seems to have nothing to do with the nervous system, since the injection is active even after all connections with the nervous system are destroyed. On the other hand, in cases of achylia gastrica, in which the stomach is devoid of all secretion, the pancreatic secretion is apparently normal, as the digestion of proteids remains undisturbed. But since in these cases there is no secretion of hydrochloric acid, secretin ought to be absent; here the pancreatic secretion is probably attended to properly by the other partner in the management of the function, that is, by the central nervous system.

A double management of partners of a different type exists probably also for the mammary secretion. There is sufficient evidence that the secretion of milk is under the influence of the nervous system. Nevertheless, the secretion continues after all nerves going to the mammary glands are cut. The milk secretion in the latter case is probably kept up by a stimulation through an internal secretion provided by the reproductive organs. Internal secretion is probably a coexisting factor in many functions of the body.

Furthermore, there are instances in which one function is cared for by two separate organs. The function of digestion of proteids in the alimentary canal is carried on by two separate organs with a different chemical activity: the pancreas and the stomach. The trypsin of the pancreas digests proteids in an alkaline medium, while the pepsin of the stomach is active only in an acid medium.

An arrangement of a similar character we meet with in the organization of the function of the defence of the body carried on by the white cells against foreign invaders. This cellular army of defense is made up of two types: the microphages, the polynuclear leucocytes whose abode is in the bone marrow; and the macrophages, the large mononuclear cells which have their barracks in the lymph nodes and lymphoid tissue. According to Opie one of the effectual weapons of these warriors is their intracellular proteolytic ferments. But the ferment of the microphage is active in an alkaline medium, while that of the macrophage requires for its activity an acid medium.

As factors of safety we may consider also the assistance which one organ lends to another or the vicariation of one organ for another. For instance, the assistance which the sweat glands render to the kidney in the process of elimination of a surplus of water, or the vicariation of the mucous membrane of the intestinal canal in the process of elimination of urea. Such mutual assistance of the organs is a widespread institution in the animal body and assures the safety of many vital functions.

Returning to the organs which are provided with a large surplus of active tissue, the question confronts us: which is the mode of distribution of the normal activity of an organ among its luxurious tissues? Since the activity of such organs, as we have seen, is far below the capacity of their tissues, the distribution could occur only in two ways. Either some parts of the tissues work to their full capacity, while the other parts remain idle, being only in readiness for emergencies—like the unemployed vice-president of some organization —or all elements of the organ take equal part in the work, each tissue element employing only a fraction of its capacity for work. The last alternative is probably the more frequent mode of distribution. There are, for instance, probably no totally inactive glomeruli and tubules in the kidneys, no inactive liver cells, no thyroid epithelial cells entirely without colloidal substance, but the epithelium of the glomeruli and tubules work only one half of their capacity, the islands of Langerhans work less than one tenth, the vesicles of the thyroid about one sixth of their capacity, etc. For the muscles of the heart it is generally assumed that all the fibers take part in every contraction, but that they work normally only a fraction of their capacity. On the other hand, there are organs in which surely parts of the tissue do not take active share in the work, unless called upon under special circumstances. In the ovaries, for instance, surely only one ovum becomes fertilized, while all the others are only on the waiting list. An instructive instance is the mode of distribution of work among the respiratory muscles. In normal inspirations, for instance, we find only the diaphragm alone at work. When somewhat deeper breathing is required, the inspirations are supported by the levatores costarum and the scaleni. Furthermore, in labored respirations also the sternohyoid and the posterior superior serrati become engaged in the work, and when the difficulties become still greater, still other groups of muscles enter into the struggle. In other words, the different groups of muscles which are designated to do the work of inspiration are not engaged in it in the manner of partners of equal standing, but enter upon their duties as a series of vice-presidents or rather as a series of reserve forces. On the other hand, in the diaphragm probably all the muscle fibers are engaged in the work of each inspiration at all times, employing only a fraction of their capacity in normal or shallow inspiration and working to their utmost capacity in dyspnœa or asphyxia. We see, therefore, in one and the same function both modes of distribution of work well represented, one muscle steadily at work with all fibers, like a heart, adapting the degrees of their energies to the various requirements of their work, and a number of groups of other muscles, acting as graded reserve forces, idle but ready for emergencies—instructive examples of luxurious factors of safety.

In the foregoing we have brought forward a sufficient number of instances in which various parts of the living organism are provided with a superabundance of material and energy to warrant the comparison of the organism with a machine with regard to the provision with factors of safety.

One of the fundamental differences between living organisms and human-made machines is that the former carries in it the germ for self-propagation, while machines have to be made by human hands. As a further difference between the two constructions we may perhaps consider the phenomenon of self-repair. Possibly the phenomenon of self-repair in the organism is closely allied with the phenomenon of self-propagation. The same source which provides the organism with a mechanism for a reproduction of the entire body provides its parts with a mechanism for regeneration of these parts. Reproduction and regeneration might have a common cause. At any rate, self-repair distinguishes the organism from the machine. If parts of a machine yield to stress and the factors of safety become exhausted, the machine would surely break down, unless it is repaired by human hands, just as it is made by human hands. As far as I know, no machine has yet been invented which is provided with devices for a continual self-repair. In the living organism self-repair is a wide-spread function of living tissues and organs. It is a dormant force, a reserve force, which springs into immediate activity as soon as any injury is inflicted. It is a factor of safety peculiar to the living organism. It manifests itself in the forms of regeneration and hypertrophy of tissues and organs, and also in the functional forms of inflammatory reaction, of substitution, vicariation and adaptation. And here it is interesting to observe that self-repair does not set in only when the margin of safety is exhausted, when there is an actual need for repair, but already when only the integrity of the factors of safety is encroached upon. Self-repair is a factor of safety also for the protection of the factors of safety. When, for instance, one kidney is removed, the hypertrophy of the secreting elements begins a few hours later, although the urinary secretion was hardly impaired. It is an attempt to reprovide with luxurious tissue. The liver cells regenerate, the thyroid, the adrenals and other organs hypertrophy and regenerate even when the preceding injury was not extensive enough to affect the function of these organs. It is, as stated before, an attempt to restore the factors of safety. A heart working above normal becomes hypertrophied even if it has not yet met with any obstacles; it is a provision in time against possible shortcomings; it is a repair of the factors of safety. This is a very interesting field, but it would lead us too far to enter upon a detailed discussion of the various aspects of the subject. We would only call attention to two exceptions. One is the very scanty repair which takes place in the organs of reproduction. But the affluence is here so immense that the organs may safely forego the benefits of self-repair. The other exception concerns the nerve ganglia; nerve cells as a whole do not regenerate. We have learned above that the ganglionic masses of the central nervous system are scantily provided with factors of safety. Here we learn that they are also deprived of the great aid afforded by regeneration. There is some functional self-repair in the central nervous system. Other centers assume the work of the lost ones; adjacent tissues become educated to the work; dormant centers of the opposite hemispheres wake gradually up to their new missions. But all these substitutes are insufficient satisfactorily to replace the lost function, not to speak of a provision for factors of safety.

Here we must recall that the lack of regeneration applies only to the nerve cells. The nerve fibers, on the other hand, especially those of the peripheral nerves, show rather a very active regeneration.

The foregoing review shows, I believe, conclusively, that the tissues and organs of the living animal organism are abundantly provided with factors of safety. The active tissues of most of the organs exceed greatly what is needed for the normal function of these organs. In some organs the surplus amounts to five, ten or even fifteen times the quantity representing the actual requirement. In the organs of reproduction the superabundance and waste of tissue for the sake of assuring the success of the function is marvelous. Furthermore, the potential energies with which some organs, like the heart, diaphragm, etc., are endowed, are very abundant, and exceed by far the needs for the activities of normal life. The mechanisms of many functions are doubled and trebled to insure the prompt working of the function. In many cases the function of one organ is assured by the ready assistance offered by other organs. The continuance of the factors of safety is again protected by the mechanisms of self-repair peculiar to the living organism. We may then safely state that the structural provisions of the living organism are not built on the principle of economy. On the contrary, the superabundance of tissues and mechanisms indicates clearly that safety is the goal of the animal organism. We may safely state that the living animal organism is provided in its structures with factors of safety at least as abundantly as any human-made machine.

The safety of a mechanism is increased, as we have stated before, also by an economic handling of the expenditure of its energy. The expenditure of energy by the living animal organism consists chiefly in the work which it performs, that is the contractions of the muscles. Of the involuntary work of the body it is only the action of the heart and the respiratory muscles of which we possess a knowledge of some available facts. The heart, although capable of doing a great amount of work, is normally kept down to perform only the most indispensable duty. The inhibitory tonus exercised by the vagi prevents the heart from beating too rapidly and too strongly, when it is not required; and the vascular reflexes carried from the heart or aorta to the vasomotor centers regulate the vascular circulation so as not to offer too much resistance on the one hand, and not to fill up the heart with too much blood on the other hand.

The respiration is normally carried out only by one muscle, the diaphragm, and this works only with a fraction of its capacity, the distention of the lungs producing an inhibitory stimulus preventing the muscle from overaction.

The contractions of the skeletal muscles being regulated chiefly by the will, offer insufficient opportunities for a study of the normal regulation of expenditure of energy emanating from this source. There are, however, two facts which are instructive and deserve to be mentioned. One is the provision of the muscle with the sense of fatigue setting in with overexertion; it might serve as a guard against overwork, against exhaustion of the muscles. The second fact is the provision of the muscular innervation with inhibitory impulses for antagonistic muscles; it prevents harmful or even only unnecessary contractions. In other words, it prevents the muscles from an unnecessary expenditure of energy.

While the facts are not many, they are sufficient to indicate the tendency of the organism to be economical in its expenditure of energy.

We now arrive at the examination of the principles of governing the supply of the organism with energy. A machine is provided with fuel far above the necessity for the performance of the expected minimum work; it has to be in readiness for unforeseen exigencies. How about the organism? The supplies for the animal machine consist of inorganic salts, water, oxygen and food. Our knowledge of the laws governing the supply and expenditure of water and inorganic salts for the animal organism are still too imperfect to be utilized here for the elucidation of our problem. We have to restrict our discussion to the supply of food and oxygen. The supply of food is influenced so much by the will of the animal that it is difficult to obtain facts permitting only one interpretation. For instance, the amounts of food taken by men in all parts of the world can not be taken as the normal quantity which the body requires, because, as Chittenden and his school say, this amount is dictated by habit and not by actual necessity. The latter found, as stated before, that with a proteid diet lower than the one employed in the current diet of man, a number of men continued their normal life without special incidents. As a result of this observation these investigators assume that the minimum proteid diet is the normal one and advocate its adoption as a standard diet. The finding that men can continue to live with a certain minimum is a fact; the assumption that this minimum is the actual requirement of the organism, is, however, only a theory, and a theory which decides that in contrast to a human-made machine the animal machine should be provided with a minimum supply of energy just sufficient for the average daily incidents and daily work.

Neither can we, on the other hand, look upon the facts which were brought together in this lecture as an absolute proof that the animal's supply of energy ought also to be provided on the same plan of superabundance. It may be claimed that the animal's welfare is best cared for by observing stringent economy in the supply of its energy.

Luckily, however, the supply of oxygen to the organism is a process practically entirely independent of the will, and therefore a fact or two which we find here may well throw some light upon our problem.

One fact here is indeed instructive. It is a frequently made and well established observation that the oxygen of the inspired air may be reduced to about one half of its normal amount without causing any ill effects whatsoever. The oxygen of the atmospheric air amounts to about 21 per cent., and it may safely be reduced to about 11 per cent. or 10 per cent. Nature then supplies oxygen to the animal body in an abundance amounting at least to twice the maximum quantity which the normal condition of life may require.

Furthermore, even with an atmosphere greatly reduced in oxygen the body is capable of attending to work so strenuous that it may cause a consumption of oxygen perhaps five times the amount normally used up during rest or light work. This occurs, as was demonstrated in the interesting experiments of Zuntz and his co-laborers, in climbing mountains and carrying at the same time considerable loads at altitudes with a barometric pressure of less than 500 millimeters of mercury.

We should also remember another instructive and characteristic fact, namely, that the venous blood is comparatively rich in oxygen, possessing often nearly two thirds of that present in the arterial blood, which means that the oxygen carried in the arterial and capillary blood is greatly in excess of the requirements of the cellular tissues.

Finally, another interesting point is that labored breathing sets in long before the tissues are in actual need of oxygen. Dyspnœic breathing is a device to cause a refilling of the exhausted surplus of oxygen by a more efficient pulmonary ventilation. The hard-working skeletal muscles which consume an undue amount of oxygen produce at the same time a substance which stimulates the respiratory center to greater activity and thus to a more liberal provision of oxygen. This is again a sort of self-repair of the loss to the factors of safety.

All this is sufficient evidence that as far as oxygen is concerned the supply of the body with energy is certainly not conducted on the principle of stringent economy. On the contrary, abundance is the guiding rule here, as it is in the provisions of the body's structures.

We now again return to the question of

supply of food. The presence of an abundant supply of glycogen and fat in all animal bodies seems to me to be a sufficient indication that carbohydrates and fats are not supplied on the principle of stringent economy. Fuel material is here abundantly stored up not so much for its immediate use but essentially for use in unforeseen exigencies. As far as I know the claim has not yet been raised that these savings deposits are due only to acquired habits of ingesting too much of the mentioned forms of food.

With regard to the proteid diet, however, the question of the normal supply, as we have repeatedly mentioned, is not above discussion. In a recent review of the subject by Benedict one of his precise statements reads: "dietary studies all over the world show that in communities where productive power, enterprise and civilization are at their highest, man has instinctively and independently selected liberal rather than small quantities of protein." Chittenden, on the other hand, says: "all our (experimental) observations agree in showing that it is quite possible to reduce with safety the extent of proteid catabolism to one third or one half that generally considered essential to life and health." And then adds: "It is obvious * * * that the smallest amount of food that will serve to maintain bodily and mental vigor * * * is the ideal." As valuable as the facts which Chittenden and his co-laborer found may be, they do not make obvious their theory that the minimum supply is the optimum—the ideal. The bodily health and vigor which people with one kidney still enjoy does not make the possession of only one kidney an ideal condition. The finding that the accepted standard of proteid diet can be reduced to one half can be compared with the finding that the inspired oxygen can be reduced to one half without affecting the health and comfort of the individual. But nobody deduces from the latter fact that the breathing of air so rarefied would be the ideal. Chittenden suggests that a greater use of proteid might be the cause of many ills, for instance of gout and even of tuberculosis and cancer. I shall not attempt to discuss the merits of this theory as far as the causation of tuberculosis and cancer is concerned. As to the causation of gout, one of Chittenden's most able supporters, Otto Folin, has pointed out that at best this could be claimed only for eating crude meat but not for an ingestion of protein in general, because the latter becomes converted into harmless urea, as Folin says. I would add that if we should avoid eating meat because some of us might sometimes get gout, we should surely avoid eating carbohydrates because it sometimes leads to diabetes, and avoid eating fats because it often leads to various mischiefs. What then shall we eat with absolute impunity?

But I wish to recall here one fact, namely that the administration of too large a dose of thyroid extract leads to a pathological condition similar in character to that of Graves's disease. The normal body nevertheless possesses, as we have shown above, a great surplus of thyroid tissue without causing any thyroidism. That some isolated metabolic product might do some harm when artificially incorporated into an animal is far from being fair evidence that this normal product of the animal mechanism does harm there when in its normal connections. Metabolic products are present in great abundance in all healthy individuals without causing mischief.

The situation seems to me to be this. All organs of the body are built on the plan of superabundance of structures and energy. Of the supplies of energy to the animal we see that oxygen is luxuriously supplied. The supply of carbohydrates and fats is apparently large enough even to keep up a steady luxurious surplus. For the supply of proteid we find in the actual conditions of life that man and beast, if they can afford, provide themselves with quantities which physiological chemists call liberal. This may or may not be the quantity which nature requires and approves of. Experiments have shown that a number of men subsisted on the half of such quantities. This latter might be an indispensable minimum, just as there is an indispensable minimum for all other luxuriously endowed provisions of the animal organism, and the liberal ingestion of proteid might be another instance of the principle of abundance ruling the structures and energies of the animal body. There is, however, a theory that in just this single instance the minimum is meant by nature to be also the optimum. But it is a theory for the support of which there is not a single fact. On the contrary, some facts seem to indicate that nature meant differently. Such facts are, for instance, the abundance of proteolytic enzymes in the digestive canal and the great capacity of the canal for absorption of proteids. Such luxurious provision for digestion and absorption of proteids is fair evidence that nature expects the organism to make liberal use of them. Then there is a fact that proteid material is stored away for use in emergencies, just as carbohydrates and fats are stored away. In starvation nitrogenous products continue to be eliminated in the urine which, according to Folin, are derived from exogenous sources, that is from ingested proteid and not from broken-down organic tissues. An interesting example of storing away of proteid for future use is seen in the muscles of the salmon before they leave the sea for the river to spawn. According to Miescher the muscles are then large and the reproductive organs are small. In the river where the animals have to starve the reproductive organs become large while the muscles waste away. Here in time of affluence the muscles store up nutritive material for the purpose of maintaining the life of the animal during starvation and of assisting in the function of reproduction. This instance seems to me to be quite a good illustration of the rôle which the factor of safety plays also in the function of the supply of the body with proteid food. The storing away of proteid, like the storing away of glycogen and fat, for use in expected and unexpected exceptional conditions is exactly like the superabundance of tissue in an organ of an animal, or like an extra beam in the support of a building or a bridge—a factor of safety.

I therefore believe that with regard to the function of supply of tissue and energy by means of proteid food nature meant it should be governed by the same principle of affluence which governs the entire construction of the animal for the safety of its life and the perpetuation of its species.

Before concluding I wish to add the following remark. It seems to me that the factors of safety have an important place in the process of natural selection. Those species which are provided with an abundance of useful structure and energy and are prepared to meet many emergencies are best fitted to survive in the struggle for existence.

S. J. Meltzer
Rockefeller Institute for Medical Research

SCIENTIFIC BOOKS

SOME RECENT BIBLIOGRAPHIES OF GEOLOGY

Two very valuable bibliographies of interest to all workers in geology have recently been issued. The first is Mr. F. B. Weeks's 'Bibliography and Index of North American Geology, Paleontology, Petrology and Mineralogy' for the years 1901–5, inclusive. It constitutes

Bulletin 301 of the U. S. Geological Survey, and includes approximately 6,400 titles. This certainly speaks well for the industry, at least, of American geologists. In glancing through the pages of the book one is struck with the large number of papers relating to economic geology. At the same time the amount of work done in paleontology, petrology, and the various abstract branches of the science, seems to have been only relatively and not actually less than in any preceding period of equal length. The bibliography follows in style Mr. Weeks's previous and well-known annual contributions.

The second report constitutes Bulletin 6 of the new (fourth) series of the Ohio Geological Survey. It consists in the main of a subject index to the publications of the various geological surveys of Ohio and is the work of Alice Greenwood Derby. It is supplemented by a brief and incomplete bibliography of publications relating to the geology of Ohio, other than those of the State Geological Survey, prepared by Mary Wilson Prosser. There is an unusually interesting preface prepared by the late state geologist, Edward Orton, Jr.

The Ohio bibliography differs entirely in character and scope from Mr. Weeks's paper noticed above. It is none the less well conceived and will be exceedingly useful. It is one of the unfortunate features of state geological survey work that the investigations are rarely continuous, and the publications are issued in various different forms and series from year to year. It is exceedingly difficult to keep track of them, and even more difficult to secure complete sets. In Ohio publication began in 1836, and there have been four successive and distinctly organized surveys. The reports have been prepared, printed and distributed under laws which have been changed from time to time, and despite the large aggregate sum spent in publications, many important libraries find it impossible to purchase full sets of the reports. Under the circumstances a complete index of the reports is particularly welcome. If it had included even brief abstracts, such as Mr. Weeks includes in his bibliographies, it would have been even more welcome. As it is, the bulletin will be of great service pending the publication of a complete dictionary catalogue, which will doubtless be some time necessary.

H. F. BAIN

URBANA, ILL.,
February 19, 1907

Mars and its Canals. By PERCIVAL LOWELL. Illustrated. New York, The Macmillan Company. 1906. Pp. xv + 393. 8vo.

'To G. V. Schiaparelli, the Columbus of a new planetary world, this investigation upon it is appreciatively inscribed'—so reads the dedicatory page of this handsomely printed volume, which is a storehouse of observation and theorizing concerning those features whose study Schiaparelli was so instrumental in inspiring. Whatever may be any astronomer's opinion regarding the degree of credibility to be assigned to various observations of the surface features of Mars, and whatever may be that astronomer's opinion of the validity of Lowell's deductions from those observations; no reader of this book can fail to recognize the serious earnestness of its author and his sincere belief in the scientific conclusiveness of all the varied arguments advanced to account for surface markings and phenomena that they present.

While this book is published as a popular exposition of the most recent investigations, it presents practically all that is known, or thus far suspected, presumably, concerning this planet and its inhabitants. It is divided into four parts.

Part I. is a description of its *natural features, i. e.,* those bearing closest resemblance to the earth in form and development. Here are outlined the advantages of good-seeing and the need to go to out-of-the-way parts of the earth to find transparent atmosphere: a bird's-eye view of past Martian discovery; a description of the polar caps, the white spots, the climate and weather, mountains and clouds, color-tinted areas, and vegetation. This part closes with a summary, occupying pages 159–170, in which are explicitly laid down and numbered 'thirty-nine articles of Martian scientific faith' presented as definite, undeniable facts of accurate observation. The rest of the summary is devoted to showing how these articles dove-tail together in mutual confirmation.

Part II. describes what the author calls 'Non-natural Features,' by which he means those which have the least analogy to features on the earth, and which differ most from what our terrestrial experience would lead us most naturally to expect. In this category he places the canals, and describes their system and their gemination, and presents arguments to substantiate their objectivity as opposed to the diplopic, or the interference, or the illusion theory. A full account of the double canals is given, also of the canals in the dark regions, of the oases, of the 'carets' on the borders of the great diaphragm; and a description of Lampland's success in photographing thirty-eight canals, the doubling of Nilokeras and a snowfall, together with the more prominent 'continents,' 'seas' and 'oases.'

Part III. deals with the 'Canals in Action,' under the headings Canals: kinematic, Canal Development Individually Instanced, Hibernation of the Canals, Arctic Canals and Polar Rifts, Oases: kinematic. The purpose of this section and also of Part IV. is to show that the canals are real water arteries for the distribution of the melting polar caps to the arid equatorial regions, and to the constitution and purpose of the canals and oases in this scheme, with arguments in support of the hypothesis that life, sentient and vegetable, does exist on Mars.

The book is copiously illustrated, and closes with an index of nine pages.

Herman S. Davis

SOCIETIES AND ACADEMIES

THE TORREY BOTANICAL CLUB

The second stated meeting for the year 1907 was called to order at the Museum Building of the New York Botanical Garden at 3:30 o'clock P.M. on January 30, 1907, with Vice-president Professor L. M. Underwood in the chair. Twenty-three persons were present.

The scientific program was as follows:

Experiences on the Island of Jamaica during the Earthquake of January 14, 1907: Dr. M. A. Howe.

A trip for the collection and study of marine algæ and other plants brought Dr. Howe to the island of Jamaica on December 14, and he was on the island during the recent earthquake. An interesting account was given of his experiences at that time

New or Rare Mosses from Jamaica: Mrs. N. L. Britton.

Mrs. Britton exhibited some of the most interesting mosses collected in Jamaica, showing several genera and subgenera, not heretofore known in the West Indies, and several new species, and also indicated some reductions of names to synonyms. Specimens of types of Jamaican species were also shown from the Mitten Herbarium and one of Miss Taylor's drawings of a new species and subgenus.

The Probable Function of Tannin in Galls: Dr. Melville T. Cook.

The origin, chemistry and uses of tannin have been studied very extensively, but other phases of the subject have received comparatively little attention. This is especially true concerning the functions which it serves in the plant. It is usually very abundant in diseased tissues, such as insect galls, fungus galls, fungus spots, etc. In insect galls it is developed very early and in some cases it appears to result in the gall-makers moving to other parts of the plant. It is also formed in fungus-galls, frequently completely surrounding the point of rupture. In such fungus spots as those produced by *Cercospora* the successive circles are due to the depositing of tannin within the tissues. The author has made extensive studies on the anatomy of both insect and fungus galls and is now conducting a series of physiological experiments.

C. Stuart Gager,
Secretary

SPECIAL ARTICLES

THE LIMITATIONS OF ISOLATION IN THE ORIGIN OF SPECIES

Any adequate consideration of the bearing of the geographical distribution of organisms upon the problems of evolution, especially of the origin and preservation of incipient species, should take into consideration all types of environment and all groups of organisms. The discussion which has continued for some time in the pages of this journal with especial reference to the significance of isolation on the origin of species has drawn its data very largely from the distribution of land and fresh-water vertebrates and has been in the main carried on by systematists who are specialists in these fields. But no insignificant part of the total organic evolution and specific differentiation has been accomplished in other phyla and in other environments. The data of distribution from the littoral, pelagic and abyssal areas of the sea, of the microfauna and flora of fresh water, and of the lower as well as the higher groups of organisms, from widely ranging as well as narrowly limited species and especially from groups in which a high degree of specific differentiation has been attained, such, for example, as the Diatomaceæ, the Radiolaria and the Coleoptera, all have some bearing on the problem unless, as some of the physiological school would have us believe, all wisdom is derivable only from pedigree cultures.

Some familiarity with the microfauna and flora of fresh water and with the pelagic fauna of the Pacific has brought to my attention a series of facts which, with considerable uniformity, indicate a degree of *coincident distribution of related species* which stands in strong contrast with the wide-spread isolation of related species and subspecies of birds and mammals to which attention has been drawn in previous discussions.

The range of material available for demonstration of this tendency is indeed great. The stimulus of the Kiel school of planktologists has led to the investigation of scores, if not hundreds, of lakes and streams, and the publication of extensive lists of their fauna and flora. Recent oceanic explorations in the several oceans have thrown a flood of light on the distribution of pelagic organisms.

The investigation of the microfauna and flora of fresh water, both limnetic and littoral, has demonstrated that its constituent organisms are to a very large degree cosmopolitan. As an illustration of this fact one has but to compare the lists from various localities. The abundant species are very widely represented. The number of rare species reported from any locality is largely a function of the thoroughness and the extent of the examination. About 50 per cent. of the protozoa and rotifers reported from the Illinois River have also been reported from the Elbe, and about 60 per cent. of the rotifers from the Oudy. The differential species, barring a considerable percentage resulting from synonymy, are almost wholly of known wide range but of relative rarity, and may be expected in most aquatic environments of the north temperate zone on long search. As a result of this cosmopolitan distribution most of the known species of a genus are wont to be found in a single locality. An excellent illustration of this phase of the question appears in Penard's splendid monograph of the fresh-water Rhizopoda in which he lists, from Geneva alone, 216 of the 232 known species of this group, and 91 of these from a single locality! He found certain recognizable habitats, each with its more or less peculiar fauna, such, for example, as the sphagnum, the sylvicolous, and abyssal faunas. These include, however, only forty-nine species limited to any special habitat. The genus *Difflugia*, for example, with thirty-seven species, has but nine whose distribution is restricted to a particular habitat; the others may occur in any type of habitat.

In the autumnal months from September to November, streams and lakes rich in organic matter exhibit a marked increase in the ciliate protozoa, especially of the Holotricha. This is especially marked in the Illinois River, and at such times many species of the genera *Prorodon, Lionotus, Lacrymaria, Didinium, Coleps, Nassula* and *Chilodon* occur coincidently. Species of other genera of ciliates such as *Stentor, Euplotes, Vorticella, Zoothammium* and *Epistylis* occur coincidently at this season. Thus Dr. Volk has recently found in the Elbe from September 9 to October 11, 73 per cent. of the *total* number of valid species, of the genera above named, admitted by Bütschli in his Thierreich monograph. The species which Dr. Volk lists from the Elbe near Hamburg exhibit a high degree of coincident distribution. His data indicate that about 85 per cent. of the recorded species in the genera named are found in the *same* habitats and that 80 per cent. of these were taken at some time in the period named on the same day. Within the genus *Vorticella,* represented by eighteen species, the average percentage of coincident occurrence on the same day in all the collections was 50 per cent.; in *Stentor,* 75 per cent. Among these instances of coincident distribution of the species of a genus are numerous cases of the *coexistence of the most closely related species in the same habitat.*

The fresh-water Rotifera are notoriously cosmopolitan in distribution. About 92 per cent. of the 246 species reported by Professor Jennings from the United States are found on other continents. The species of the group exhibit in common with the fauna and flora with which they are associated, certain well-defined restrictions in distribution, due to environmental factors combined in various types of habitat, such as open water, and shore vegetation. The genus *Brachionus* is found predominantly in open water of warm shallow ponds and streams where it occurs in enormous numbers during a large part of the year. After eliminating synonyms arising in large part from failure to examine literature or to ascertain the seasonal form-cycle, there remain at least about twelve common forms of specific value in this genus along with a considerable number of so-called variants. One of these species, *B. dorcas,* is predominantly a winter species, and another *B. militaris,* predominantly a shore and vegetation-loving species. The remaining forms are all typical members of the limnetic fauna of warm shallow waters and *usually together throughout a large part of the summer,* in many parts of the world. In common with practically all other organisms of the limnetic fauna and flora these species of *Brachionus* run a rhythmical course of fluctuating recurrent cycles of increase and decrease in numbers of three to six weeks' duration. A single instance drawn from the records of the Illinois State Laboratory of Natural History of my unpublished statistical work upon the plankton of the Illinois River will suffice to illustrate the character of these rhythmical fluctuations and the relations which they bear to the breeding periods of *Brachionus.* During the rise of the 'pulse,' parthenogenesis prevails, and at its maximum and during its decline sexual reproduction appears as indicated by the presence of male eggs, males and winter eggs. The imperfections of the plankton method, the difficulty of specific identification of male rotifers and detached eggs introduce a considerable degree of incompleteness in these statistical records. The trend of the statistics remains, however, significant.

BRACHIONUS ANGULARIS.—NUMBER PER CUBIC METER ILLINOIS RIVER, 1896.

Date.	Females with Partheno-genetic Eggs.	Females with Male Eggs.	Females with Winter Eggs.	Males.	Total, Eggs and Adults.
July 29	4,560	0	0	0	27,072
Aug. 1	7,747	0	0	0	69,342
Aug. 8	18,696	0	0	0	81,320
Aug. 12	29,106	103,950	594	0	773,982
Aug. 21	9,496	3,561	0	1,187	161,432
Aug. 29	3,775	0	0	0	28,355
Sept. 5	2,385	0	0	0	16,536

Within the breeding period of *B. angularis* covered by this table, *Brachionus pala, B. amphiceros, B. bidens* and *B. bakeri* were also found with male or winter eggs or both, and *B. cluniorbicularis, B. melhelmi, B. militaris, B. mollis, B. punctatus, B. rhenanus, B. rubens* and *B. tuberculus* ran a like cycle of smaller amplitude, but no males or winter eggs were found attached to the individuals counted. No effort was made during the collection of these statistics to determine whether these remaining species were breeding sexually or not. It is quite possible that they were. The list includes a number of instances of most closely related couplets. Opinions differ as to the specific, subspecific or varietal standing of some of the forms in the list.

We have then in *Brachionus* coincident distribution of nearly all the species of the genus and contemporaneous sexual reproduction in five species or varieties, in this instance, with the possibility that the remaining eight may also be in sexual activity. There is little possibility of any horizontal stratification separating the species here, as the waters of the Illinois River are shallow (three to ten feet) and are stirred by current and diurnal movements. These same species occur together, moreover, in shallow bottom-land lakes but a few inches in depth.

Closely related species and varieties of *Brachionus* thus live together in the same habitat and breed at the same times, with no external barriers to prevent their swamping out.

The fresh-water Copepoda afford an excellent test for the contemporaneous breeding of closely related species having coincident distribution, since the eggs are carried for some time by the female during their development and the breeding periods of the several species can thus be determined. There are six species in the subgenus *Cyclops* which occur in the Illinois River and are often found breeding in the same seasons. Two of the most closely related, *C. edax* Forbes and *C. leuckarti* Claus, are found breeding coincidently in August. The other members of the subgenus, *C. viridis* Jurine and *C. bicuspidatus* Claus have overlapping breeding periods in the spring, and the two varieties of *C. viridis*, v. *insectus* Forbes and v. *brevispinosus* Herrick breed coincidently throughout the summer. *C. serrulatus* Fischer and *C. prasinus*, closely related species of the subgenus *Eucyclops* have a wide distribution in the United States and Europe and are frequently found in the same collections. *Diaptomus pallidus* and *D. siciloides*, two most closely related species of Calanidæ, are found together in about equal numbers, breeding at the same seasons in the Illinois River.

The limnetic fauna and flora thus afford numerous instances not only of the coincident distribution of closely related species, but as well of their contemporaneous breeding. The extent to which this factor of contemporaneous breeding prevails among the organisms of fresh waters and the full force of its bearing upon the problem of isolation are best understood when the fact is borne in mind that the whole ensemble of organisms constituting the plankton, runs throughout the year, year in and year out, this rhythmical series of recurrent cycles. In this most, if not all, of the constituent plants and animals have coincident periods of rapid increase followed by one of subsequent declines offering, in cases of sexual reproduction, repeated opportunities for the interbreeding of related forms, and the swamping out of incipient species arising by the slow accumulation of minute fluctuating variations. There appears, however, to be no lack of specific differentiation among these organisms. Isolation by external factors seems to play a small part, if indeed any, in specific differentiation in this type of habitat.

The pelagic life of the sea abounds in seeming instances of the coincident distribution of closely related species in an environment presenting a minimum degree of diversification and a relative absence of barriers. The incompleteness of our knowledge of the horizontal and vertical distribution of pelagic organisms and of their breeding seasons leaves here much to be desired in the data for a discussion of this problem. There is great specific differentiation among pelagic diatoms, Radiolaria, Foraminifera, Dinoflagellata, Copepoda and Amphipoda. Frequently many species of the same genus occur in the same region, witness the fifty species of *Coscinodiscus* found by Karsten in the Antarctic plankton of the Valdivia Expedition, largely at a depth between 60 and 40 meters.

The genus *Ceratium* of the dinoflagellates is one of the most diversified and its species are among the most variable of marine organisms and are mainly of wide distribution. In the plankton of the Pacific immediately at the surface, that is in the upper 2–3 meters, it is no unusual thing to take 25–30 species in a single 14-inch net.

There are three species of *Ceratium* which are usually coincidently abundant in the Pacific off San Diego which are most closely related, so closely in fact that their distinctness has frequently been overlooked by casual workers on this genus. They are the cosmopolitan *C. furca* (Ehrbg.), and *C. lineatum* (Ehrbg.), and the minute *C. eugrammum* (Ehrbg.). The species are evidently closely related, differing only in certain proportions of the midbody, in the degree of ventral excavation and in size. The examination of a wide range of material and a statistical study of their proportions has convinced me of their distinctness. Their resemblances are so plainly marked that one is forced to conclude that they are branches of a common stem or co-mutants. *C. furca* and *C. lineatum* are widely distributed in temperate and tropical seas and the occurrence of *C. eugrammum* in both the Mediterranean and in the Pacific is suggestive of a similar range. Its small size enables it to slip through the mesh of even a fine plankton net. This fact, together with its resemblance to *C. lineatum,* doubtless explains its comparative absence in published records of distribution. These species are abundant at the same seasons, and are taken repeatedly at the same levels in the same net. Their pelagic habit is suggestive of a pelagic origin and their coincident distribution over wide areas would seem to afford abundant and oft-repeated opportunities for swamping out incipient species by interbreeding of the diverging forms with parental stock, provided their origin took place by the gradual accumulation of minute fluctuating variations.

The lists of plankton organisms in European seas published by the Conseil permanent pour l'exploration de la mer, as well as our records for the past four years at San Diego, show many instances of the coincident distribution of *Ceratium macroceros, C. longipes,* and *C. intermedium,* three closely related species, all having open tips to the antapical horns and more or less postindentation, but differing from each other in size, form of the shoulders and the angular divergence of the horns. These species are so closely related that they have been much confused in the past, but our best specialists in this group are now agreed that the species are distinct. They are the most closely related species of their section of the genus to which they belong and they have a coincident and apparently coextensive distribution in temperate seas.

Another striking instance of coincident distribution is found in the species of the *C. tripos* group, including the closely related *C. azoricum, C. arcuata, C. heterocamptum, C. bucephalum* and *C. curvicorne,* which are very often found together in the plankton of the Pacific and extensively throughout much of the Atlantic as Cleve has shown in his 'Seasonal Distribution of Atlantic Plankton Organisms.'

Dividing twenty-seven of the more common species of *Ceratium* in groups of most closely related species and determining their distribution in surface waters of the Pacific in twenty-one typical collections, we find that the percentage of coincident distribution of the species of the several groups actually realized in the collections ranges from 48 per cent. to 68 per cent. on an average.

Unfortunately for my discussion, we know neither the strata nor the seasons in which the sexual reproduction of these organisms occurs, if, indeed, any exists.

It will doubtless be suggested by defenders of the universal potency of isolation in the origin of species that the instances of coincident distribution of related species which I have here cited are those of organisms which multiply predominantly by asexual methods or at least by parthenogenesis, that sexual reproduction is entirely, or at least relatively, absent and that this in itself constitutes an effective isolation and prevents the swamping out of new forms. In reply to this attention should be directed to the admirable researches of Dr. Schaudinn and of Professor Richard Hertwig, and especially to the results of investigations into the life histories of the *Sporozoa* and the *Foraminifera* which have brought to light the wide-spread occurrence of typical sexual reproduction in so many of the protozoa. So striking have been these results that it is safe to say that a full knowledge of life histories will reveal sexual reproduction in all protozoa. Over against the apparent infrequency of sexual reproduction in the protozoa and many fresh-water organisms should be placed the brevity of the life cycle and the considerable number of sexually produced generations which are possible every year. *Polyarthra platyptera,* a common cosmopolitan rotifer, produces, in the Illinois River, male and winter eggs throughout most of the year at the close of parthenogenetic cycles of 3–6 weeks duration. It is not improbable that other organisms of the same habitat run a similar course and exhibit a corresponding proportion of sexual reproductions. Opportunities for swamping out incipient species are present, therefore, in these organisms, at least in so far as the existence of sexual reproduction offers them, in limnetic and pelagic organisms as well as in the fauna and flora of the land or in animals of higher organization. The work of Maupas and Calkins on the recurrence of conjugation in ciliates is significant as indicating the necessity of sexual reproduction among unicellular organisms.

Evidence of the coincident distribution of related species of higher pelagic organisms in which amphigony is the only form of reproduction may be found among the chætognaths. Other groups of pelagic organisms should be examined on this point. The publication of Fowler's excellent monograph on the chætognaths in the reports of the "Siboga" expedition, in which he reviews the matter of their distribution, affords some pertinent data of prime significance because the group is a sharply circumscribed one with but few species, the distribution of most of which is fairly well known. There are but three genera, *Sagitta, Krohnia* and *Spadella* in which the author recognizes twenty, three and two species, respectively. The two species of *Spadella* are members of the epiplankton and their areas of distribution overlap. In the case of *Sagitta* there are twelve species which belong to the epiplankton of warm temperate and tropical waters. In this group are both cosmopolitan and apparently restricted species. The remaining eight species belong to colder or deeper waters (5) or are of undetermined range (3). Of the twelve species at least ten are known to occur in the Indo-Australian or Indo-Pacific regions, where they exhibit coincident or overlapping distribution.

The species of *Sagitta,* though relatively few in number, are often so similar in structure as to be separated with great difficulty even by a specialist. One of the most closely related groups in the genus is that formed by *S. serratodentata,* a cosmopolitan eurythermal species ranging in the epiplankton from the latitude of the Straits of Magellan to the equator, and by *S. bedoti, S. ferox* and *S. robusta* from the epiplankton of the Indo-Pacific region. These four species occur together *repeatedly* in the Siboga collections of surface plankton. A second group of related species is formed by *S. enflata, S. hexaptera* and *S. pulchra* which likewise occur together in the same collections often in large numbers. The first two species of this section are ones of wide range and the last an Indo-Austral one.

S. neglecta and *S. regularis,* two closely related neritic species from the Malay Archipelago, form a third group having a coincident distribution. There are possibly two instances, *S. furcata* and *S. planctonis,* and *S. bedoti* and *S. sibogæ,* where related species have a contiguous distribution in epiplankton and mesoplankton. The data are too scanty for definite conclusion.

It appears from Fowler's records that, as a rule, the *most closely related* species of this genus have, not a *contiguous,* but a *coincident* distribution. In the present condition of our knowledge it is impossible to state whether these species breed at the same seasons or not. Shipley in the Cambridge Natural History states that *Sagitta* breeds throughout the year. Fertilization is presumably external in many species, though known to be internal in one and eggs are pelagic, but there are no geographical barriers which intervene to prevent the swamping out of incipient species. Provided there are in these related species of *Sagitta,* no differential breeding seasons, and no differential levels or temperatures at which extrusion of ova and sperm occur, no assortive assemblings of the individuals of the given species during specialized breeding seasons and no close fertilization, we are compelled to fall back upon the assumption of a sterility of the cross between an incipient species and its parent stock or preferential mating of like forms for an explanation for the origin and continuance of these closely related species in a coincident environment. This is the solution which Darwin proposed to meet this difficulty. De Vries finds that the *coincidently* appearing new elementary species of *Œnothera* attain their full constancy at once. Isolation plays no part in their origin or continuance.

CHARLES ATWOOD KOFOID
UNIVERSITY OF CALIFORNIA

NOTE ON A TERTIARY BASIN IN NORTHERN ALASKA[1]

BEDS of Tertiary age are known in various widely separated regions of Arctic and sub-Arctic America. They occur generally in basins of limited area in the older rocks. Such basins are rather widely distributed in Alaska. Brooks[2] has recently reviewed the literature relating to those in Alaska. A number of isolated Tertiary basins are known in the islands of the Arctic Archipelago[3] and others have been reported in the Northwest Territory[4] east of northern Alaska. The age of nearly all of the Tertiary beds which are known in Arctic America has been determined from plant remains occurring in them, so that we have a very fair knowledge of the plant life of northern America in Tertiary times, but a very meager knowledge of the invertebrate fresh-water fauna which lived in some of the Tertiary lakes of Arctic America. The discovery of such a fauna by the writer during the past summer along the Porcupine River in northeastern Alaska seems, therefore, worthy of record.

[1] Published by permission of the Director of the U. S. Geological Survey.

[2] 'The Geography and Geology of Alaska,' professional paper U. S. Geological Survey, No. 45, pp. 237–244, 1906.

[3] Low, A. P., 'Cruise of the Neptune,' 1906, pp. 226–229.

[4] McConnell, R. G., 'Report on an Exploration in the Yukon and Mackenzie Basins, Northwest Territory,' Can. Geol. Surv., Vol. 4, n. ser., Rept. D, 1890.

Camsell, C., Report on the Peel River and Tributaries Yukon and Mackenzie, Can. Geol. Surv., Vol. XVI., 1906, Pt. C, pp. 27–28.

About one hundred miles above its junction with the Yukon the Porcupine River enters the Yukon Flats, an alluvial plain without topographic relief, through which the Yukon River flows for 200 miles. Before entering the Yukon Flats the Porcupine traverses for about 25 miles a belt of limestones of Paleozoic age having a north and south trend and giving rise to low mountains and hills. Between this belt of elevated country and a similar but wider zone of topographic relief near the international boundary extends a comparatively flat basin having a width of about 25 miles. The north and south extent of this basin, which Maddren has called the Coleen Basin,[5] after the river draining its northern portion, is unknown. There is good reason to believe, however, that its north and south extent is much greater than its width. The Porcupine traverses this basin in wide sweeping meanders. The migration of the channel of the river along parts of its course through this basin has left in places low banks bordered by recent silts. An older set of sediments, however, constitutes the bulk of the floor of the Coleen Basin. These older sediments are well exposed on the largest meander in this portion of the river, known as the Fish Hook bend, which shows continuous bluffs for two or three miles, 40 to 100 feet high, composed mainly of finely laminated shale or clay. The dominant color of these beds is light lemon yellow, which is varied by patches of yellowish green, pink, and brownish. At the upper end of Fish Hook bend, on the west bank of the river, the following section was measured:

	Feet.
1. Fine sand, soil and muck (top)	1–5
2. Coarse gravel and sand	10
3. Dark carbonaceous clay and old forest bed	0–2
4. Coarse gravel and sand	15
5. Soft, finely laminated, drab-colored clay shale, with large ironstone concretions in upper part, containing fresh-water bivalves	70

The fossils which were secured from the ferruginous concretions occurring in divi-

[5] 'Smithsonian Exploration in Alaska in 1904 in Search of Mammoth and other Fossil Remains,' *Smithsonian Misc. Coll.*, Vol. 49, 1905, p. 12.

sion 5 of the section were referred to Dr. W. H. Dall, who reports that "One resembles *Unio onariotes* Mayer from the Kenai formation, another *Anadonta athlios* Mayer of the same beds, but they are probably not identical. The beds are probably Oligocene or upper Eocene, like those of Kenai." Considerable interest attaches to these fossils in connection with their bearing on the distribution in Tertiary times of the Naiades, a group represented in the present streams and lakes of lower latitudes in North America by several hundred species.

Maddern[6] and McConnell[7] have reported two basins similar to the Coleen, but larger, higher up the Porcupine a short distance east of the international boundary. One of these has an approximate length of 100 miles and a width of 60 miles. No fossils were obtained by McConnell from these upper basins, but the description which he gives of the beds exposed, corresponds so closely to the sections observed by the writer, that it is highly probable that the age of the beds in the basins on the two sides of the boundary is the same. McConnell[8] expressed a similar opinion concerning the equivalence of the beds in question, but presented no paleontologic evidence of the age of the beds in either of the basins which he described.

E. M. KINDLE

U. S. GEOLOGICAL SURVEY,
WASHINGTON, D. C.
February 26, 1907

[6] *Smithsonian Misc. Coll.*, Vol. 49, 1905, p. 14.

[7] Ann. Rept. Geol. Nat. Hist. Surv. Canada, n. ser., Vol. 4, 1890, p. 128.

[8] *Ibid.*, pp. 128, 132.

QUOTATIONS

THE UNIVERSITY OF MAINE

THE movement to remand the State University of Maine to its original purpose of an agricultural college and school of mechanic arts has failed. The University has expanded with a college of liberal arts and has been giving the bachelor's degree, and this the senior colleges of liberal arts in the State, Bowdoin, Bates and Colby, consider to be crowding an already overstocked market for higher education. Consequently, when the University came to the Legislature this year for an appropriation equal to two fifths of a mill to the valuation of the State, or about $150,000 a year, with a backing of the majority report of a committee appointed by the last Legislature, and then accepted as a substitute for this an appropriation of $65,000 a year for two years and $90,000 for new buildings, the three colleges appealing to the committee of the Legislature on the subject, united under the leadership of President Hyde, had attached to the appropriation as a condition the discontinuance of the University's courses leading to the B. A. degree. In the House this amendment to the appropriation was defeated by the cyclonic vote of 123 to 12. In the Senate, however (which had made a special investigation with a recess committee of the whole subject), the vote was much closer, being, in fact, only 17 to 13 in favor of the University's retention of the right to confer the B. A. degree * * * .

In the heat of the debate the dignity of the university and the quality of its scholarship have been sometimes rather roughly used. The statement made by President Hyde and repeated by ex-Senator Potter of Brunswick, an overseer of Bowdoin, that the liberal arts courses at the University of Maine are inferior to those of the other three colleges is pretty well disposed of by the statistical facts that there are eleven professors at the university holding doctor's degrees conferred by the highest institutions of learning at home and abroad, including Harvard, Johns Hopkins and Cornell in this country and Berlin and Heidelberg universities, a total greater than is found in the faculties of the other three colleges together. Mr. Carnegie, it is believed for the first time, gave the university last year the money for a library building absolutely without conditions, and by energy, thrift and the use of local materials, almost given to the university, a building has been erected for his $55,000 equal in appearance, it is said, to one costing twice as much. Evidently this sturdy young growth from the Federal land grant known as the Morrill education fund is past all danger of being up-rooted, and now needs only to be judiciously tended and watered.—*Boston Evening Transcript.*

CURRENT NOTES ON LAND FORMS

DRAINAGE CHANGES IN CALIFORNIA

J. C. Branner describes 'A Drainage Peculiarity of the Santa Clara Valley [Cal.] affecting Fresh-water Faunas' (*Journ. Geol.*, XV., 1907, 1–10), in essence as follows: A stream flows westward from the Mt. Hamilton group into the middle of the longitudinal (N.W.–S.E.) Santa Clara Valley of the Coast range, and there builds up a great alluvial fan which forms a divide on the valley floor. The stream at present flows northwest from the fan by Coyote Creek to the southern end of the Bay of San Francisco. Another creek heads in the longitudinal valley near the fan and flows southeast to join Pajaro River, which runs westward into the Bay of Monterey. A slight radial shifting of the stream on the fan would transfer it to the Pajaro system. There is good evidence that such shifting has occurred in the past, probably repeatedly, so that the waters from Mt. Hamilton have found their way alternately into the two bays. Such stream changes would permit a mingling of the faunas of the Coyote and Pajaro basins. Should a long time then elapse without further changes in drainage, the unified faunas might spontaneously and gradually diverge. At present the fishes of the two rivers are remarkably alike in most respects; some points of difference are taken to indicate the beginning of spontaneous divergence.

It appears, however, that the faunal peculiarities common to these two rivers are noted also in other rivers flowing into the two bays. As the mouths of the rivers are now separated by salt water and as their heads are far apart, a second hypothesis must be advanced to account for the community of forms, which Branner believes to require direct fresh-water connection at some former time. A former elevation of the coast, permitting the rivers of each bay to unite in a main river system, is believed to afford a satisfactory explanation for the phenomena observed. Each of the two main river systems would then have had its own fauna, until changes on the alluvial fan, as above described, had mingled them in a single fauna common to both basins. Submergence would then dismember or betrunk the two systems, yet the isolated rivers would still have similar faunas.

Independent evidence is given of the elevation and submergence here postulated. The amount of elevation is believed to have been great. In view of this it would be interesting to know whether the possibility has been considered that, during such uplift, the two main river systems might have united in one, permitting a mingling of faunas independent of the stream changes on the fan at the divide.

D. W. J.

THE PENEPLAIN OF BRITTANY

E. de Martonne, lately professor of geography in the University of Rennes and now in that of Lyons, gives an effective description of the peninsula of Brittany, illustrated with expressive block diagrams and photo-plates ('La pénéplaine et les côtes bretonnes,' *Ann. de Géogr.*, XV., 1906, 213–236, 299–328). Brittany is a district of greatly deformed ancient rocks, worn down for the most part to a peneplain, but retaining here and there certain residual reliefs of moderate altitude; the whole gently up-warped in Tertiary time, and now more or less dissected in a second cycle of erosion. The chief residual reliefs or linear monadnocks follow the east-west structural trend somewhat north of the middle of the peninsula. In spite of their moderate altitude (250 meters), they are sparsely inhabited; some of the peasants there are still ignorant of the French language and speak only Breton. The peneplain, admirably developed and preserved north of the residual hills, is sharply dissected by young valleys towards the bold coast; but when the valleys are followed southward towards their heads they open out on the upland. The peneplain on the southern side of the peninsula is more destroyed by revived erosion, because larger areas are here occupied by relatively weak rocks, some of which have been almost reduced to local peneplains of a second generation. The southern coast is less bold than the northern, because the peneplain slopes gently southward under the sea. A recent gain of the sea on the land has drowned the distal parts of all the streams, and carried vigorous tidal currents into the incised meandering valleys. Along the southern part of the shore line thus determined, the sea has already formed numerous smooth-beached sand reefs; but little advance to such maturity is seen on the ragged western promontories and bold northern coast, where marine action is still chiefly destructive.

It is gratifying to see so systematic a treatment of a physiographic province on the basis of structure of rock masses, process of sculpture and stage of development, stated in the compact terms of the cycle of erosion and illustrated with typical block diagrams which combine structure and form in so simple and suggestive a manner. It is at the same time indicative of the stage of physiographic development of the readers of the *Annales de Géographie*—the most scholarly geographical periodical in France—to note that the transformation of valleys into arms of the sea by drowning is here presented in a more or less argumentative manner, as if it still needed discussion and demonstration before it could gain acceptance. It is also significant that Brittany is here described as having an Appalachian structure and development, as if this new world example of a corrugated structure, peneplained, elevated and redissected, were so well established that it served as a type for the description of a somewhat similar example in the old world.

There are two points on which supplement may be made to de Martonne's very readable article. First, that although the sea works furiously, with heavy storms and strong tidal currents, on the western and northern coast, the outer shore line there is still in a young stage of development, marked by an increase of irregularity and raggedness over that of the initial shore line; hence a recent date must be given to the depression by which the valleys were drowned and the initial shore line of the present cycle was formed. Second, when the recent depression of the region occurred, it presumably caused the submergence of an extensive lowland, worn down on a large area of weak rocks north of Brittany in the second cycle of erosion. Submergence thus formed the western part of the English channel, and brought the sea against the northern marginal slope of the hard-rock upland of the earlier peneplain, a little outside of the present cliff line. When explained in this way, the northern cliffs of the peninsula do not, in spite of their height, indicate a great consumption of the land by the sea, as has been supposed by some authors.

W. M. D.

PHYSIOGRAPHIC TYPES

THE address delivered by the late Professor Israel C. Russell on 'Physiographic Problems of To-day' at the Congress of Arts and Science, Universal Exposition, St. Louis, in 1904, is brought again to mind by its recent publication in the proceedings of the Congress (Vol. IV., Boston, Houghton, Mifflin & Co., 1906, 627–649). One of its most suggestive passages is concerned with 'ideal physiographic types,' by which the author meant "complete synthetic examples of * * * physiographic forms, which will serve the rôle of well-defined species in the study of the surface features of the earth. Ideal types may be likened to composite photographs. They should combine critical studies of many actual forms, within a chosen range, and in addition be ideally perfect representations of the results reached by specific agencies operating under the most favorable conditions. * * * A well-arranged catalogue of ideal types would be an analytical table of contents to the history of the evolution of the features of the earth's surface, and constitute a scheme of physiographic classification. * * * The selection of idealized physiographic types * * * has for its chief purpose the reduction of endless complexities and intergradations to practicable limits. It is a method of artificial selection so governed that, while no line in the chain of evolution may be lost to view, certain links are chosen to represent their nearest of kind and serve as types." When this suggestion is well carried into practise, geography will have made a great advance.

W. M. D.

SYSTEMATIC PHYSIOGRAPHY

A SERVICEABLE article by Hettner, professor of geography at Heidelberg, on 'Das Wesen und die Methode der Geographie' (*Geogr. Zft.*, XI., 1905, 545–564, 615–629, 671–686), revives Ritter's conception of geography as the science that is concerned with the material filling of terrestrial spaces—'die dingliche Erfüllung der Erdräume'—and emphasizes the importance of the causal notion which has been coming into greater and greater prominence in the last half century, as fuller explanation has been found for the earth's physical features, and as fuller meaning has been given to the distribution and behavior of plants, animals and man by the philosophy of evolution. He recognizes also the advantage that comes from a scientific terminology in making short and clear descriptions possible; yet he expresses a doubt as to the possibility of describing various geographical features such as valleys and towns, in generic terms, because such features possess so many details which can be portrayed only by individual description. The Neckar Valley is taken as an example to illustrate this conclusion; it is briefly described as 'a *Durchbruchstal* formed in a climate without the cooperation of ice action; we can perhaps still find even a few more generic features, but everything else is individual.' This is hardly more than begging the question, for the possibility of giving an adequate account of the Neckar Valley in terms of generic features can not be so lightly tested. The study of the development of generic features in the class of incised meandering valleys, to which the Neckar Valley belongs, has already proved the repeated or systematic occurrence of various minor elements whose recognition greatly increases the power of description. (See an article by the undersigned on 'Incised Meandering Valleys,' *Bull. Geogr. Soc. Phila.*, July, 1906.) In view of the notable progress of this kind in recent years there is every reason to expect a still greater progress in the years to come: there is indeed no direction in which a physiographer may labor more profitably than in contributing to this progress. Instead of practically giving up the problem, and abandoning the method of systematic or generic description of geographical features as impracticable because insufficient, as Hettner suggests, it is our plain duty to make every effort to develop it farther than its present stage, in the manner suggested by Russell in the preceding note; and there is good ground for hope that systematic description may yet be so far advanced, especially if carefully chosen adjectives are consistently used to modify the generic nouns which represent standardized type forms, that whatever local supplement is needed will be not so well given by an unsystematic verbal account of individual features as by reference to a good map, where many items that are fatiguingly represented in words are very easily shown graphically.

W. M. D.

THE NEW GEOLOGICAL SURVEY OF BRAZIL

The Diario Official, the official organ of the federal government of Brazil published at Rio de Janeiro, in its issue of January 22, 1907, prints the text of the decree establishing the Brazilian Geological Survey or, as it is called officially, the 'Serviço Geologico e Mineralogico do Brazil' (the geological and mineralogical service of Brazil). This decree was approved January 10, 1907, by the president of the republic, Affonso Augusto Moreira Penna, and by the minister of industry and public works, Miguel Calmon du Pin e Almeida, and has already gone into effect.

The following instructions are translated from the same issue of the *Diario Official:*

Instructions for the Operations of the Serviço Geologico e Mineralogico do Brazil

Article I.—The following are the principal objects of the geological service:

1. To carry on a scientific study of the geological structure, mineralogy and mineral resources of the territory of the republic, having

especially in view the development of the mineral resources and of the surface and underground waters and the collection of such information regarding the geologic and physiographic nature of the country as may serve as a basis for the determination of routes of communication and other public enterprises, especially such as may tend to relieve the effects of drouths.

2. To maintain a laboratory and museum of geology and mineralogy and to collect, classify and arrange for exposition in this country and in the chief foreign centers specimens accompanied by proper information suitable to give those who may be interested as complete knowledge as possible concerning the geology, mineralogy and mineral resources of Brazil; and to have made such chemical, paleontological and other investigations as may tend to the fulfillment of the principal objects of the service.

3. To prepare and publish maps, plans, diagrams and drawings or photographs for the illustration and elucidation of the investigations, reports and other publications of the service.

4. To organize and publish statistics of the mineral production and of the mining and metallurgical industry of the country; to study facts relating to water-supply both for irrigation and for domestic and industrial purposes; to study the question of artesian and other wells, and also matters relating to the mines and mining enterprises in the country and systematically to make known its mineral wealth.

5. To furnish data and information upon questions relating to lands and mines and also upon all questions concerning the mining industry and other objects of the service whenever called upon by the federal government, or, with the government's authorization, when called upon by the state governments or by private parties.

Article II.—The appointment of the technical and administrative assistants of the service, with the exception of the chief, will be made by the minister.

The chief of the geological and mineralogical service will be appointed by decree.

Article III.—It shall be the duty of the chief of the service:

1. To appoint and dismiss such assistants as are not subject to appointment by the minister, to indicate their salaries and to fix the wages of technical assistants.

2. To organize, direct and fiscalize the work of the service and to assign the duties of the assistants.

3. To call upon the proper authorities either personally or through his representatives for whatever may be necessary in the prosecution of the work.

4. To furnish, whenever required to the ministry, information regarding expenses incurred and results accomplished, and to make an annual detailed report regarding the affairs of the service.

5. To make provisions in urgent cases for any omissions in the present instructions, and subsequently to submit his act for the approval of the government; also to indicate who shall act in his stead in cases of impediment.

Article IV.—The technical personnel composed of specialists of recognized fitness shall carry on the studies and work of the service in accordance with the expressed instructions of the chief.

Article V.—The secretary together with the clerk shall do the writing and keep the accounts of the service and of such other work relating to it as the chief may direct.

Articles VI., VII. and VIII. relate to expenses and the salaries, while article IX. provides for the further development of the service.

Under this decree Professor Orville A. Derby was appointed chief of the service, and the chief has asked for the appointment as assistants of the following Brazilian geologists:

Dr. Miguel Arrojado R. Lisboa, to have charge of the investigation of the gold, iron and manganese deposits in the state of Minas.

Dr. Luiz Filippe Gonzaga de Campos, who will probably undertake geological studies on the Rio Purus in the Estado do Amazonas.

Dr. Francisco de Paula Oliveira will proceed with his studies of the coal deposits of Santa Catharina and Rio Grande do Sul.

Professor O. A. Derby, the chief of the service, is a native of the state of New York, having been born at Kelloggsville in 1851. He entered Cornell University in 1869, graduated in 1873, and took his master's degree in 1874. He accompanied Professor Hartt to Brazil in 1870, 1871 and again in 1872. In 1874 Professor Hartt went to Brazil, leaving Mr. Derby as assistant in charge of the department of geology at Cornell. A geological survey of Brazil (Commissão Geologica do Brazil) was inaugurated in 1875 with Professor Hartt as chief, and on this Mr. Derby was appointed assistant geologist. He reached Rio de Janeiro in 1875, and he has lived in Brazil ever since. He was connected with the geological commission as long as it lasted; upon its suspension and after the death of Professor Hartt he was appointed director of the geological section of the national museum in Rio de Janeiro. In 1886 he was made director of the geological and geographical survey of the state of São Paulo, a position which he retained until 1905, when he resigned.

Among the authorities upon Brazilian geology Professor Derby is *facile princeps.* His papers on the subject number something over one hundred, and other writers upon Brazilian geology have also drawn largely upon his rich store of information. In addition to his own direct contributions he has been instrumental in getting the cooperation of many of the best specialists in the world to work up and describe special collections. For example, the 'Cretaceous Paleontology' of Brazil by Dr. C. A. White and the 'Paleozoic Faunas of Pará' by Dr. John M. Clarke are monumental contributions to the geology of Brazil that were made possible by Mr. Derby's efforts and cooperation. The well-known writings upon Brazilian mineralogy and petrography by Dr. E. Hussak were also made possible by Professor Derby's appointment of that able geologist to a position on the São Paulo survey. Among Mr. Derby's most noteworthy direct contributions to geological literature are his papers on the origin of diamonds, on the nephelene rocks of Brazil, and on the geology of the lower Amazonas.

Dr. Gonzaga de Campos is a native of the state of Maranhão, where he was born in 1857. After completing the preparatory course in the Polytechnic School of Rio de Janeiro, he entered the newly established School of Mines at Ouro Preto as one of its first students, graduating with honors in 1880. He did private work for a year, when he was called as assistant to the geological Survey of the state of São Paulo, then being carried on under the direction of Professor Derby. He has published ten important papers upon Brazilian geology. Of these papers, one upon the coal deposits of Santa Catharina, another upon the diamond deposits of Agua Suja of Minas, and a third upon the bituminous shales of Marahú in Bahia are especially worthy of mention.

Dr. Miguel Arrojado R. Lisboa is a native of the city of Rio de Janeiro, where he was born in 1872. He is the grandson of the Baron of Japurú, who was some time Brazilian minister at Washington. His technical training was received at the School of Mines at Ouro Preto, where he graduated in 1894. After graduation he was employed as geologist by Barão de Capanema and later he entered the topographic corps engaged in mapping the state of Rio de Janeiro. From 1898 to 1900 he studied at the University of Berlin and in Paris and, returning to Brazil, opened an office as mining engineer and consulting geologist in Rio de Janeiro. Dr. Lisboa is the author of about a dozen papers upon the geology and mining industries of Brazil. The most important of these are upon the Brazilian manganese deposits, the monazite sands, and the iron industry of Brazil. The January, 1907, number of the *American Journal of Science* contains an article by Dr. Lisboa upon facetted pebbles found by him in the interior of Brazil.

The other assistant, Dr. Francisco de Paula Oliveira, is also a native Brazilian, and a graduate of the School of Mines at Ouro Preto. He is an active and prolific writer, being the author of seventeen papers upon the geology and mineralogy of Brazil. It is expected that additional assistants will be appointed in the near future.

The Brazilian people are to be congratulated upon the establishment of this important service. The development of the great mineral resources of that country and the growth of a healthy, hopeful and helpful interest in geology may now be looked for with confidence.

The character, the professional high standing and the unselfish patriotism of the men entrusted with the survey are a guarantee of the abundant and trustworthy results to be expected.

J. C. Branner

Stanford University, California,
March 7, 1907

THE FAYÛM EXPEDITION OF THE AMERICAN MUSEUM

In the Middle and Upper Eocene fluvio-marine formation of Northern Egypt most important paleontological discoveries were made between 1902 and 1905 by Mr. Beadnell, of the Egyptian Geological Survey, and Mr. C. W. Andrews, of the British Museum of Natural History. From being the terra incognita of paleontology Northern Africa suddenly sprang into prominence as the center of origin and evolution during the Eocene and Oligocene periods of four great groups of mammalia whose early history had previously been entirely unknown. These were, in order of importance, the Proboscidia or mastodons and elephants, the Sirenia or manatees and dugongs, the Hyracoidea or tree and rock conies and the Zeuglodontia or Archæoceti, or primitive toothed whales. Together with the primitive, or rather the early forms of these mammals, because certain of them are already highly specialized, occur a few members of two other faunæ, namely, of the upper Eocene of France, and secondly a purely African contingent including chiefly the giant horned quadruped appropriately named *Arsinoïtherium.* This animal held an adaptive position in the faunæ complex somewhat similar to that of *Dinoceras* in our western tertiaries. Large collections of these mammals were made by the Egyptian Survey for the Cairo Museum and by the British Museum, on which was based the admirable memoir by Dr. C. W. Andrews published by the British Museum some months ago.

This fauna has a peculiar interest for the department of vertebrate paleontology in the American Museum because three of these great African or Ethiopian groups of mammals sooner or later reached North America, namely, the Zeuglodontia in the Eocene of Alabama and Georgia, the Sirenia in the Miocene of the western coast, the Proboscidea in the Middle Miocene of the western tertiaries. The subsequent American phases are all represented in the museum collections, and it was obviously desirable to trace the ancestry back to the earliest known stages. A further cause of personal interest to Professor Osborn was the fact that in an address before the New York Academy of Sciences in 1900 he had predicted that three or more of these groups would most probably be discovered in Africa. The publication of Andrews's memoir made the field and the subject free for scientific research by other workers and after considerable correspondence and inquiry into the probabilities of success the project of an expedition was approved by the president and by the director of the American Museum. With his usual liberality President Morris K. Jesup decided to defray the chief expenses of a three or four months' expedition, and preparations were made to leave New York on January 5 and to begin work in the Fayûm as early as possible in order to take advantage of the cool months of the Egyptian winter. Professor Osborn selected as his assistants Mr. Walter Granger and Mr. George Olsen, of the museum staff. President Roosevelt, Mr. Joseph H. Choate and Director Charles D. Walcott sent letters to Lord Cromer and other officials of the English protectorate supporting the chief objects of the expedition.

The party reached Cairo on January 24, expecting to occupy ten days or more in outfitting with camels, water tanks, supplies and men. But Director H. G. Lyons, of the Egyptian Geological Survey, entirely altered this estimate by most liberally placing all the necessary equipment from the survey at Professor Osborn's disposal and further facilitating the work of preparation in every possible manner. Captain Lyons cooperated still further by detailing Mr. Hartley T. Ferrar, formerly geologist of the British Antarctic Expedition, to accompany the party. Mr. Ferrar's experience in dealing with the natives subsequently proved to be of great service. Natives who had been out with Messrs. Beadnell and Andrews were engaged. The heavy equipment was sent round by rail to Tamia, on the northern edge of the Fayûm oasis, there to be transported by caravan two days' journey, or thirty miles, to the nearest of the bone-bearing localities. It thus came about that exactly one week from the time of arrival in Cairo the expedition started by caravan from the Gizeh pyramids a few miles west of Cairo.

On the journey south the caravan followed the line of the pyramids of Gizeh, Abusir, Sakkara, Deshûr and Lisht. This gave an opportunity of observing the methods of excavation on a large scale by native labor. At Gizeh Dr. Reissner is excavating for the Boston Museum of Fine Arts, with a very large force of men and boys, and at Lisht Dr. Lythgow, another American, is just beginning his explorations for the Metropolitan Museum of Art. The natives are believed to be direct descendants of the builders of the pyramids of five thousand years ago and the tools and carrying baskets have remained unchanged. Wages have risen rapidly in recent years and boys now receive from two to three piasters a day, while the skilled men receive five piasters, or twenty-five cents. At Sakkara are the workings of the Egyptian Museum under Dr. J. E. Quibell. The meeting here proved to be a most fortunate one because Dr. Quibell placed at the disposal of the American Museum party twelve of his best workmen, who were engaged at once to proceed to the bone beds.

The bone beds lie exactly forty miles in a southwest direction from Cairo. The route taken by the party, following the southward line of the pyramids and then turning due west, covered seventy miles, or twenty-eight hours' caravan. A freighted caravan moves so precisely at the rate of $2\frac{1}{2}$ miles, or 4 kilometers, an hour that all desert distances in Egypt are estimated in caravan hours. On the evening of February 5, one month from the date of departure from New York, the party reached the most easterly 'bone pits' as indicated on Mr. H. E. Beadnell's admirable survey map of the Fayûm district. Some of the Egyptians had arrived two days before and had already begun excavation. Two days later the Sakkara force of natives arrived and the camp presented the most picturesque and imposing aspect. Stretched out on the gradually shelving bench of upper Eocene sand were eleven tents. Twenty-six camels were traveling to and fro on the three days' distant water supply. Their groans filled the early morning air.

The topography is extremely interesting. The Fayûm is a very ancient, perhaps late Pliocene, depression, lying closely contiguous to the Nile, which in Pleistocene times converted the basin into a great lake substantially similar in outline to the Lake Moeris of the Ptolemies. Control of the inflow and reclamation of the alluvial bottom began under the ancient Egyptians until now the shallow and brackish Birket el Kurun (Lake of the Horns) stretches 41 meters below sea level and 15 miles east and west as the last and ever-diminishing vestige of the ancient lake. It is fed by the waste of the irrigation waters. The easterly end of this lake is twenty-six miles west of the Nile. Rising from the north shore of the lake and overlooking it like the tiers of an amphitheater are the benches of Middle and Upper Eocene age, the lower steps purely marine, the upper littoral and fluviatile, which finally rise tier above tier, 341 meters, to the summit platform of the Libyan desert. In a gentle northeasterly and southwesterly direction these benches extend for a distance of 45 surveyed miles, beyond which the Eocene is unsurveyed. This noble section has long attracted the attention of German geologists and the fossils of its lower marine members, as studied by Schweinfurth, Blanckenhorn and Zittel, afford proof of a close synchronism with the Middle and Upper Eocene of the Paris Basin. In the southwest is the famous 'Zeuglodon Valley' full of the skeletons of these ancient and aberrant whales. These animals as well as the Sirenians from the lower members of this great formation have been known since 1879. In 1898–1901 Messrs. Beadnell and Andrews discovered a number of land mammals in these marine or littoral beds, and in the latter years a turning point in the history of paleontology was reached by the discovery of *Palæomastodon* teeth in the upper fluviatile beds. The working of this level between 1901 and 1904 by the Cairo and British Museum parties, and less thoroughly by collectors for other museums, has been principally on a bench 160 meters above sea-level, about 40 meters in thickness, and with a fossiliferous exposure of not over ten miles east and west. Evidence of very thorough prospecting and of excavation on a large scale is everywhere apparent. The bone-bearing stratum is evidently of fluviatile origin, consisting of cross-bedded and often brightly colored sands, sometimes intermingled with clay concretions. All can be worked with a light pick and shovel or with the broad-edged mattox which the natives prefer. Bones are often found broken, more seldom entire; they occur in pockets from 30 to 100 feet in diameter. Several of these pockets or 'bone pits' are reserved by the Egyptian survey but the privilege to work them further was kindly extended by Director Lyons to the American Museum. The conditions of deposition are altogether unlike anything in the American Western Eocene, but resemble as closely as possible the Lower Pleistocene, Sheridan or Equus Beds of our west.

While Professor Osborn and Mr. Ferrar were engaged in a survey of the whole region, simultaneous excavation of two of these 'bone pits' was begun at once under the direction of Mr. Walter Granger and Mr. George Olsen, of the museum party. It soon became apparent that the six workmen from the Helouan stone quarries who had previously been out for the Cairo and British Museums were too impatient to find large bones and too careless with delicate objects, whereas the twelve workmen from Sakkara entered into the search with great care and intelligence, and although they had never hunted for fossil bones before showed an enthusiastic reverence for a delicate object which was delightful to watch as expressed in the display of smiling rows of ivory white teeth at a word of encouragement. The labor question was solved and the problem now became one of extensive excavation, of handling and removing great quantities of sand, of uncovering and taking up the sometimes firm, sometimes extremely delicate and fragile bones, of keeping a very sharp lookout for small objects, and especially for the smaller animals of the period which have thus far not been discovered. Coarse river-deposited sands are evidently not favorable for the preservation of very small objects, but very careful methods of search were soon rewarded by the discovery of small jaws of the creodonts or primitive carnivora of the period, and even more interesting rodents, which had hitherto not been found. At the end of ten days the system of stripping and quarrying was thoroughly established and progressing as rapidly as the slower work of treating the bones with solutions of gum arabic and shellac could follow. Limb bones, teeth and jaws of *Arsinoïtherium, Palæomastodon, Saghatherium, Ancodus, Pterodon* began to appear in encouraging numbers and in fairly good condition.

The members of the excavating party were ever stimulated by the hope that the much-desired skull of one of these mammals might come to light. Skulls are, however, very rare. Of the primitive elephants only two fragmentary skulls of *Palæomastodon* and two of the more primitive *Mœritherium* have been found after many years' work. One of each was found by accident while surface prospecting. Two skulls of *Palæomastodon* are reported to have been found by a collector who did not understand how to preserve them. Of *Arsinoïtherium* six skulls have been secured; three of these, all very perfect, are in the Cairo Museum, one in the British, a fifth is in the Stuttgart Museum, a sixth, said to be the largest and finest, was destroyed in transit. Of these six skulls four were found in surface prospects and two in quarrying.

The region has been so thoroughly prospected since 1901 that the chances of easily securing fine surface prospects are very remote. The bones weather out in pure white color, are very conspicuous and can be seen a long distance. In fact, the halcyon days of easy collecting have passed, just as they have passed in our western tertiaries. A party will only succeed through thorough, systematic and prolonged search and excavation. On these lines and with this expectation the work of the American Museum has been established by Professor Osborn on a two or three months' footing or as long as the weather is tolerably cool. A train of eight camels is constantly moving to and fro, keeping the camp supplied with water, a three to four days' round journey. Mr. Walter Granger, assisted by Mr. George Olsen, is left in charge. It is hoped that with the aid of fifteen selected workmen, not only a representative collection of these very important mammals may be secured, but considerable additions may be made to our knowledge, especially of the smaller mammals of the Upper Eocene period in Northern Africa.

H. F. O.

Cairo, February 25, 1907

SCIENTIFIC NOTES AND NEWS

M. Pierre Eugène Marcellin Berthelot, the eminent chemist, died in Paris on March 18, at the age of eighty years. M. Berthelot was permanent secretary of the Paris Academy of Sciences. He was a life member of the French senate and had been minister of public instruction and minister of foreign affairs. The Chamber of Deputies, after making an appropriation for a public funeral, adjourned in his memory.

Mr. C. G. Abbot, who had been for a number of years Secretary Langley's principal assistant in the Astrophysical Observatory of the Smithsonian Institution at Washington, and latterly its acting director, has been appointed director of the observatory, and Mr. F. E. Fowle, Jr., hitherto junior assistant, has been appointed aid.

Professor W. K. Brooks, of the Johns Hopkins University, will join Dr. A. G. Mayer, the director of the Tropical Marine Laboratory of the Carnegie Institution, at Nassau in April, for zoological research in the deep waters of the Bahamas.

The organizing secretary of the Section on Embryology of the Seventh International Zoological Congress is Professor E. G. Conklin, of the University of Pennsylvania. Pending his return from a short trip to the Bahama Islands, he desires to call the attention of workers in the fields of normal and experimental embryology to the opportunities which will be offered for the presentation of important papers in these subjects, and to request their cooperation in making the meetings of this section highly successful.

Dr. Francis Henry Smith, professor of natural philosophy in the University of Virginia since 1853, has retired from active service.

Professor Gætano Lanza, head of the department of mechanical engineering of the Massachusetts Institute of Technology, has received a decoration from the king of Italy, conferred by the Italian consul at Boston, Dr. Gustavo Tosti, at a banquet given in his honor.

The London Society of Dyes and Colors has founded in honor of Sir William Perkin a Perkin medal to be conferred for scientific and industrial work connected with the dyeing industries.

The centenary of the Imperial Operating Institute, a department of the University of Vienna, was celebrated on March 15. Many distinguished surgeons attended the evening proceedings, which took the form of a Lister festival, in honor of Lord Lister's eightieth birthday.

M. Henri Poincaré has been appointed a member of the council of the Observatory of Physical Astronomy at Meudon, in the room of the late M. Moissan.

The Technological Institute in Vienna has conferred an honorary doctorate of engineering science on Baron Auer von Welsbach.

The University of Glasgow will confer its doctorate of laws on Sir George Watt, author of the 'Dictionary of the Economic Products of India'; M. Emile Boutroux, professor of philosophy and director of the Fondation

Thiers, Paris; John Norman Collie, Ph.D., F.R.S., professor of organic chemistry, University College, London; Signor Ulisse Dini, professor of mathematics in the University of Pisa; Professor Adolf Harnack, director of the Royal Library, Berlin, and author of many works on ecclesiastical history; M. Henri Poincaré, professor of mathematics and astronomy at the Sorbonne; emeritus Professor of Physiology John Gray McKendrick, and Principal Donald Macalister, of the university, and recently lecturer in medicine at Cambridge University.

PROFESSOR F. W. PUTNAM, of Harvard University, has recently received word from Dr. W. C. Farabee, head of the Peabody Museum South American Ethnological Expedition, which started December 17, 1906, of its safe arrival at Arequipa, Peru, where the Harvard Observatory is situated. The expedition will make its headquarters at Arequipa, from which place the members will make excursions of several months' duration in order to study the manners and customs of the South American Indians of that region.

DR. OTTO LUMMER, professor of physics in the University of Breslau, who came to this country to give a course of lectures at Columbia University, gave an address before the Philosophical Society of Washington on March 21. The subject of the address was 'The Determination of the Temperature of the Sun, and Recent Solar Theories.' The meeting was held at the Bureau of Standards and was followed by a reception.

THE annual lecture of the Michigan chapter of the Society of the Sigma Xi was given on March 22, by Professor W. E. Castle, of Harvard University, his subject being 'The Experimental Study of Heredity.'

DR. WILLIAM R. BROOKS, director of the Smith Observatory and professor of astronomy at Hobart College, Geneva, N. Y., delivered an illustrated lecture at Clark University on March 15, his subject being 'Other Worlds than Ours.'

AT the meeting of the Botanical Seminar of the University of Nebraska, on March 22, Dr. Roscoe Pound delivered an address on the life and work of the late Dr. Otto Kuntze, whose death at San Remo, Italy, occurred on January 28, 1907. The address, which was of the nature of an 'appreciation,' will be published in an early number of SCIENCE. The second paper read was by Dr. Edith Clements, on 'Stability and Evolution,' and was based on her studies of particular species in the Rocky Mountains.

MISS CRUICKSHANK has presented to the University of Aberdeen a handsome window in the Library at Marischal College, in memory of her father, the late Dr. John Cruickshank, professor of mathematics from 1817 to 1860, and librarian from 1844 to 1860; and two portraits in oils—one of Sir Archibald Simpson, the architect of Marischal College; the other of James Ferguson, the astronomer.

PROFESSOR W. H. BAKHUIS-ROOZEBOOM, who carried forward to an important extent the work on physical chemistry inaugurated by Willard Gibbs, and who succeeded Professor van't Hoff at Amsterdam in 1896, died on February 8, at the age of fifty-three years.

M. MARCEL BERTRAND, professor of geology in the Paris School of Mines, died on February 25, at the age of sixty years.

DR. GUIDO KRAFFT, professor of agriculture in the Technical Institute at Vienna, died on February 22 at the age of fifty-two years.

MR. NOBLE HARTER, known to psychologists for an important piece of research work on the telegraphic language, carried on in conjunction with Dr. W. L. Bryan, died in South Pasadena, Cal., on February 23.

THE deaths are also announced of Dr. Mathias Marie Duval, professor of histology at the Paris Faculté de Médicine and at the Ecole d'Anthropologie; of Professor Y. Y. Tswetkoff, professor of mathematics at the Moscow Forestry School, and of Professor Karl Mayer-Cymar, the paleontologist of Zurich.

THERE will be a civil service examination on April 17–18 for the position of scientific assistant in the Department of Agriculture, at salaries ranging from $600 to $2,000 per annum, depending upon experience and qualifications. As a result of this examination certification will be made to fill a vacancy in the position of scientific assistant qualified in seed testing, in the Bureau of Plant Industry, at $900 per annum.

There will be a civil service examination on April 24–25 to fill positions as assistant geologist and geologic aid in the Geological Survey, at salaries ranging from $1,000 to $1,600 a year. The department estimates that ten appointments will be made at an early date as a result of this examination, the salary depending upon qualifications and experience. In certain cases where only a portion of the time of the appointee can be given to the work of the department, the compensation will be upon a per diem basis for the service rendered. Appointments to the position of temporary field assistant will also be made from the resulting eligible list as far as possible. The compensation of such positions ranges from $60 to $100 per month, and the length of employment from three to seven months. At the same time there will be an examination for the position of geologist in the Philippine service at a salary of $1,800.

Funds have been donated by Mr. William C. Sproul, state senator, of Chester, Pa., for the purchase of one of the largest telescopes on the Atlantic Coast for Swarthmore College. The exact amount of the gift or the size of the telescope is not known, but the instrument will be quite as efficient as the government's telescope at Washington or the University of Virginia's telescope at Charlottesville, which are the two largest instruments in the east. The telescope will be in charge of Dr. John A. Miller, professor of mathematics and astronomy. Senator Sproul is a member of the board of managers and has been active in the advancement of the institution since his graduation in 1891.

There has been placed in the case in the paleontological museum of the University of Kansas a fine skeleton of the extinct *Bison occidentalis*. This unique specimen was collected by Mr. H. T. Martin in Give County, Kans., and mounted by him. It has an extreme length of 122 inches and a height of 79½ inches.

It is said that a compromise has been effected by which the city of Philadelphia will receive $1,000,000 from the estate of Dr. Thomas W. Evans for the establishment of a dental institute and museum.

The commissioners for the 1851 Exhibition have appropriated a site on their estate at South Kensington for the Institute of Medical Sciences. It is understood that the site will be reserved for one year, during which it is hoped that the additional sum of about £30,000 required to build and equip the institute may be obtained.

The fourth International Congress of Mathematicians will meet at Rome, from April 6 to April 11, 1908. The membership fee is twenty-five francs, and the general secretary on organization is Professor G. Castelnuovo.

The eighth meeting of the Association of Teachers of Mathematics of the Middle States and Maryland will be held at Teachers College, Columbia University, New York, on April 6, under the presidency of Professor Edwin S. Crawley, of the University of Pennsylvania.

The House of Commons on March 23, by 150 to 118 votes, rejected the bill proposing to introduce the metric system into Great Britain.

According to the daily papers, letters from Messrs. Ernest Leffingwell and Elmar Mikkelsen, containing the first news from the Anglo-American polar expedition since the expedition left Alaska, have been received at Portland, Ore., by Mr. H. A. Andree, assistant in the local weather bureau. The letters were dated November 21 and November 23 and were carried over the ice fields to Point Barrow, where they were delivered to the Canadian mounted police. Mr. Leffingwell states that the ship, the *Duchess of Bedford*, was frozen in solid ice, about 200 miles off Point Barrow and that the preparations were at that time almost completed for starting the expedition into the unknown country lying to the north.

It is the purpose of the Geological Survey of Canada to distribute a number of surplus volumes of its publications. In so doing, it is proposed to allow all persons, libraries, societies, etc., now on the distribution list, the opportunity of completing their sets of these publications. The number of each publication in stock varies, and the volumes will be distributed in order of the receipt of the applications, until exhausted. Applications should be addressed to Mr. A. P. Low, director, Ottawa.

The Biological Laboratory of the Brooklyn Institute of Arts and Sciences at Cold Spring Harbor, Long Island, will hold its eighteenth session, beginning on July 3, and continuing for six weeks. Courses are offered in field zoology by Dr. Chas. B. Davenport, Dr. H. E. Walter, of Brown University, and Dr. W. M. Wheeler, of the American Museum of Natural History; in bird study, by Mrs. Walter; in comparative anatomy, by Dr. H. S. Pratt, of Haverford College, and Mr. D. S. Hartline, of the Pennsylvania State Normal School; in invertebrate and general embryology, by Dr. A. L. Treadwell, of Vassar College, and Mr. H. E. Jordan, of Princeton University; in cryptogamic botany by Dr. D. S. Johnson, of Johns Hopkins University, and Mr. H. H. York, of the University of Texas; in plant ecology by Dr. E. N. Transeau, formerly professor of botany at Alma College and now of the Carnegie Institution of Washington, and Mr. W. S. Cooper, of Johns Hopkins University; in microscopic methods by Mrs. Davenport. Opportunity is afforded for original investigators who wish to work in private rooms. Persons who wish to make use of the laboratory should correspond with the director, Dr. C. B. Davenport, Cold Spring Harbor, N. Y.

The Biological Survey of Michigan in charge of the state geologist has just issued a report covering the Terrestrial Pulminata of the state by Bryant Walker, copiously illustrated.

We learn from *Nature* that on March 5 a deputation representing the Anthropological Institute, the British Science Guild and other scientific bodies, waited upon the Prime Minister to urge the establishment of a national anthropometric survey. Mr. R. C. Lehmann, M.P., who introduced the deputation, said that, in the first instance, the survey should have for its object the periodic measurement of children and young people in schools and factories. Besides this, a comprehensive survey of the general population of the whole country should be undertaken. The sum asked for is £4,000 or £5,000. The need for such a survey was described by Dr. D. J. Cunningham, Mr. J. Gray, Dr. Gow, Sir Lauder Brunton and Dr. A. C. Haddon. In his reply to the deputation, Sir Henry Campbell-Bannerman confessed that he has been much impressed by the arguments adduced as to the great lack that there is in this country of knowledge of the quality of the population. It is obviously desirable to have a record of the kind proposed in order to be able to study the changes in the condition of the people at large as a guide to action in administration and in legislation regarding it. Any test applied to the condition of the inhabitants of any district is a test of their surroundings, of the mode in which they live, and the circumstances which affect their health and utility, and therefore this can not be an unimportant thing. It is very desirable to avoid any impression that a sort of experiment is to be practised upon the poor children in the common schools. Whatever is done to the poor ought to be done also to the rich, and the application of the system ought to be universal. In fact, it will cease to have its proper value if it is confined to the poor schools, which are a little more at the disposal of the government and the authorities than the great schools, such as Westminster and others. Results are wanted referring to the whole population, so that comparison may be made between different districts and different occupations. The sum mentioned for the survey is a modest amount, but a great many modest sums make up a large sum. But the mere question of cost is not likely to stand in the way of a great scheme of this sort if the government is satisfied on full consideration—which shall be given to it—that the time is ripe for this new enterprise.

The Royal Academy of Sciences of Turin, in accordance with the will of its former associate, the late Senator Thomas Vallauri, will award a prize to the scientific investigator, of Italian or any other nationality, who between January 1, 1907, and December 31, 1910, shall have published the most important work in the domain of physical science, taking that term in its largest sense. The prize will be of the value of $5,500.

UNIVERSITY AND EDUCATIONAL NEWS

By the will of Francis P. Furnald, of New York City, Columbia University will receive on the death of his widow $300,000 for a dormitory, to be called Furnald Hall.

By the will of the late Captain Thomas P. Salter, of Portsmouth, New Hampshire, Dartmouth College will, on the death of his sister, receive the residue of his estate, estimated at about $250,000.

By the will of the late Mrs. James W. Queen $10,000 is bequeathed to Princeton University to found two scholarships in memory of the late James W. Queen.

By the will of the late General Samuel E. Merwin, of New Haven, $5,000 has been bequeathed to Yale University for the uses of the medical school.

The removal of the Connecticut Agricultural College at Storrs to a position nearer the center of population is being considered.

The Massachusetts Agricultural College will hold a Teachers' Summer School in Agriculture with Professor F. A. Waugh as dean.

The New York *Evening Post* states: In the Museum of Natural History of Vassar College much work is being done in rearranging and remarking the collections. A special collection illustrating all species of birds found in the southeastern part of New York State has been arranged in the interest of the members of the college who wish to study the local birds.

There is a movement to establish a second Danish University at Aarhuus, the capital of Jutland.

The council of the University of Manchester have decided to institute two new lectureships—one in economic zoology and one in economic botany.

Three hundred teachers from Toronto and a hundred from Brantford, London, Hamilton and other points will visit New York, Washington, Philadelphia and Baltimore during Easter week.

At the University of Virginia appointments have been made as follows: Dr. Stephen H. Watts, of Johns Hopkins University, professor of general surgery and director of the University Hospital; Dr. Thomas L. Watson, of the Virginia Polytechnic Institute, professor of economic geology; Dr. R. M. Bird, of the University of Missouri, collegiate professor of chemistry, and Dr. Arthur E. Austin, of Boston, adjunct professor of physiological chemistry.

Professor A. M. Soule, dean and director of the Virginia Polytechnic Institute at Blacksburg, has accepted a similar position in the Agricultural College of Georgia.

Mr. Clarence G. Derick, of the Massachusetts Institute of Technology, has been appointed laboratory and research assistant to Professor Noyes, for the coming year, at the University of Illinois.

The Alice Freeman Palmer fellowship at Wellesley College, which is of the value of $1,000, has been awarded to Miss Helen B. Cook, who will study psychology in Germany.

Mr. A. P. Parsons, formerly of the University of Minnesota, has been appointed lecturer in mineralogy and petrography in the University of Toronto.

Dr. Hermann Klaatsch, who is at present in Australia, has been appointed associate professor of ethnology in the University of Breslau.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, APRIL 5, 1907.

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE SECTION B—PHYSICS

THE annual meeting of Section B, Physics, of the American Association for the Advancement of Science, was held in Fayerweather Hall of Columbia University, in New York City, on December 27, 28 and 29, 1906. The annual meeting of the American Physical Society occurred at the same time and place; each society nominally held a separate meeting for the transaction of business, but all of the sessions for the reading of papers were joint sessions.

The presiding officers were Professor W. C. Sabine, of Harvard University, vice-president and chairman of Section B, and Professor Carl Barus, of Brown University, president of the American Physical Society. The other officers of Section B who were present were the retiring vice-president, Henry Crew; the secretary, D. C. Miller; member of the council, E. F. Nichols; member of the general committee, William Hallock; members of the sectional committee, Henry Crew, W. C. Sabine, D. C. Miller, E. B. Rosa (elected at this meeting to serve for five years), G. F. Hull, E. L. Nichols; press secretary, W. S. Day.

For the next meeting, to be held in Chicago, in convocation week of 1907–8, the officers, so far as now determined, are:

Vice-president and Chairman of Section B—Dayton C. Miller.

Retiring Vice-president—W. C. Sabine.

Members of the Sectional Committee—W. C. Sabine, Dayton C. Miller, A. D. Cole, G. F. Hull, F. E. Nipher, E. L. Nichols, A. Trowbridge, E. B. Rosa.

Secretary of Section B—A. D. Cole, Ohio State University, Columbus, Ohio.

On December 28, the retiring vice-president, Professor Henry Crew, of Northwestern University, gave a most interesting and important address on 'Fact and Theory in Spectroscopy'; this address was printed in full in SCIENCE for January 4, 1907.

Probably the sessions of Section B have never before been attended by so large a number of representative physicists; at some of the meetings more than one hundred and fifty persons were present. The papers were many (nearly fifty in number) and were, upon the whole, of a very high order of merit. The meeting was universally pronounced a most successful one.

There was no distinction between the programs of the Physical Society and of Section B; the abstracts and titles of the papers of both societies are given below.

Effect of a Magnetic Field upon the Ionization in a Closed Vessel: W. W. STRONG, Johns Hopkins University.

According to present views, atoms contain charged particles which are in rotation. In magnetic substances such as iron or oxygen more of these charged particles rotate in one direction than in another, and these orbits of rotation lie in the same or in parallel planes. In a magnetic field atoms would turn so that their planes would be perpendicular to the field. The effect of the field would then be to decrease the velocity of these charged particles, but not to change their radius of rotation. This would make the atom more stable. One could expect that the natural ionization in a closed vessel would be decreased by the application of a strong magnetic field. This was the effect looked for.

The electroscope consisted of a sheet-iron box of some three litres volume containing air at atmospheric pressure. The charged electrode consisted of a wire bent into the arc of a circle at one end. The gold leaf was attached to a rod at a point slightly different from the center of the electrode arc. The length of the gold leaf being slightly less than the radius of this arc, at different positions its end would be at different distances from the charged electrode. By this means the gold leaf could be made very sensitive to changes of voltage of the charged electrode. The gold leaf was earthed.

By using a large electromagnet the rate of leak was found to be changed as much as thirty parts in a hundred at times although this change was not found to be constant. The greatest effect was found when the field was turned on for the first time. An example of the readings are as follows, they being made consecutively:

Time.	Leak in Scale Divisions of Micrometer Microscope.	Rate of Leak in Scale Divisions per Second
Field on, 663 secs.	.737	.00111
Field off, 613 secs.	.876	.00143
Field on, 515 secs.	.540	.00105
Field off, 520 secs.	.835	.00161

The writer can not attribute this change as due to any other effect than that of a magnetic field on the natural ionization. It may be possible, however, that there is some other explanation.

Projections of the Globe Appropriate for Laboratory Methods of Studying the General Circulation of the Atmosphere: CLEVELAND ABBE, United States Weather Bureau.

The general circulation of the atmosphere is controlled by the general distribution of land and water, and by the insolation, with its resultant temperature, evaporation and clouds. In the analytical treatment of this problem, beginning with d'Alembert, Ferrel and Erman, as well as in the more elegant works of Helmholtz, Oberbeck and Margules, it has always been considered necessary to simplify the problem by assuming a uniform surface and uniform coefficient of resistance for the whole globe, as also a uniform condition of dry air. In this shape the problem is already very complicated, and it is likely that the profound meteorological problem of the deduction of the actual winds from the laws of mechanics, will for a long time be too difficult for pure analysis; but on many occasions I have stated my belief that a solution may be arrived at in an experimental way.

We may represent any portion of the globe, with its atmosphere, by a horizontal plane surface, covered with some heavy gas or liquid, such as a thin layer of carbonic-acid gas, or alcohol, or water, which is to be set in steady rotation, and may be warmed from below in such a way as to approximately imitate the actual isotherms of the lower atmosphere. Special areas of high and low temperature can easily be imitated by electrical resistance coils. These rotating areas are to be covered by plates of glass, rotating with them.

Various theorems relative to the similarity of such a model to the atmosphere of the earth were first published by Helmholtz in 1873, 'On a Theorem Relative to Movements that are Geometrically Similar in Fluid Bodies,' and these ideas were further applied by him in memoirs on atmospheric motions, in 1888 and 1889; and further developments have lately been given by Lord Rayleigh. But in applying these ideas to our two polar projections we stumble upon a great difficulty, namely, that the maps are not true representations of the spherical surface of the earth.

In the present case we should like to preserve the equality of surface areas; and to preserve equality of distances, since we have to compare velocities; and above all, we should like to preserve the equality of the moments of inertia. That which best satisfies all desiderata seems to be Airy's 'projection by balance of errors,' published by him in 1861. The combination of Airy's development with Helmholtz's method of mechanical similarity should enable us to interpret our laboratory experiments.

I consider it extremely desirable that these experiments should be made on a large scale, with due regard to all numerical, statistical and mechanical details, in some laboratory where the study of meteorology is prosecuted as a branch of mathematical physics.

Some New and Useful Data in Reference to the Moisture of the Air: HENRY EMERSON WETHERILL, Philadelphia.

This paper gave an outline of researches upon the relative humidity of the air, as determined with a new cobalt chloride scale and test paper, and referred to the use of a special instrument for measuring the moisture of the body in different diseases.

References were made to new psychrometers for correcting hygroscopes and to studies of the measurement of the perspiration, in malarial and other fevers, as carried out in the Philippines, Panama and elsewhere; and also to a cobalt hygroscope depending upon the change of weight of the test papers with humidity. The weighings of this change suggested the production of an instrument that might be called the hygrobaro, and one that would be of service to the weather bureau.

The Compensated Two-circuit Electrodynamometer for Alternating Current Measurements of Precision: Edward B. Rosa, Bureau of Standards.

If an electric current pass in series through a shunt S and the fixed coil F of an electrodynamometer, and a small derived current, taken from the potential points ab of the shunt flow in series through the resistance R and the moving coil, a deflection will occur proportional to the square of the current and inversely as the resistance, R. The current may be either direct or alternating. Hence if a known direct current, measured by means of a potentiometer, standard cell and standard resistance, be used first to get the constant of the dynamometer, the value of an alternating current can then be obtained. It is shown in the paper mathematically that any self-inductance in the shunt S or the fixed coil F does *not* produce an error in the alternating current measurement, but that the self-inductance of the moving coil and any Foucault currents in the fixed coil or in any neighboring conductor or metallic parts *do* cause an error. It is shown how to detect such an error and to compensate for it, so that the deflected readings of the instrument are the same as though these sources of error were absent. The mathematical theory is verified by experiment, and alternating currents up to 500 amperes are thus measured with precision. The compensation holds good at different frequencies, and the error of a Kelvin balance, due to frequency, is *measured.*

The Power Factor and Temperature Coefficient of Mica Condensers: E. B. Rosa and F. W. Grover, Bureau of Standards.

Absorption and leakage in a condenser are a cause of expenditure of energy when the condenser is placed on an alternating-current circuit. This energy is, of course, smaller for good condensers than for poor ones, and for very good condensers is extremely small, so small that the heat resulting is inappreciable unless the voltage is higher than can safely be applied to the condenser. In the comparison of condensers by means of an alternating current bridge, it is shown in the paper how the phase angle of the currents through the two condensers can be compared, the difference indicating which is the better condenser. In an air condenser the current is 90° in phase ahead of the electromotive force acting on the condenser, and if a mica condenser is compared with an air condenser and shows a difference in phase of 10′, the angle for the mica condenser is 89°50′. The power factor is then cos 89°50′ = .0029. If the power factors of standard mica condensers are determined, other condensers may be compared with them and their differences (+ or —) being directly measured, giving the power factors of the condensers under test. This is the best single test of the quality of a condenser, and capacities measured in this way are not subject to the error due to the leakage to which direct-current measurements are subject.

The temperature coefficient of a mica condenser will be affected by the paraffine in which it is embedded. Some grades of paraffine melt at a relatively low temperature and soften and expand rapidly at still lower temperatures. The temperature coefficient of the condenser will be small and nearly constant up to a certain temperature and then rather suddenly increase greatly. The temperature at which this sudden increase occurs is sometimes low enough to occur in summer weather without any additional heating. A good mica condenser should be embedded in high-grade paraffine, so that its temperature coefficient may remain small through all ordinary ranges of laboratory temperatures.

There is a great difference among condensers as to power factor and temperature coefficient, and a condenser should never be purchased without a knowledge of these constants as well as a knowledge of its capacity.

The Diffraction of Electric Waves of Short Wave-length: A. D. Cole, Ohio State University.

Peculiarities observed in a quantitative study of the reflection of electric waves seemed to be due to diffraction and led to this study. These peculiarities were: (1) A sudden increase in the amount of energy passing through a slit of increasing width when it becomes more than a quarter wave wide, (2) the amount of energy reflected from a narrow mirror was found to be greater than that passing through a slit of the same width provided each is but a small fraction of a wave-length wide, (3) irregularities noticed when screens were introduced to cut off direct radiation.

The earlier investigations of Trouton, Zehnder and Righi were carried out with apparatus which did not give quantitative results. A modified form of Klemenčič thermo-receiver made it possible to study the distribution of refracted energy quantitatively. A Righi exciter, actuated by an induction coil and Wehnelt interrupter, was used for generating the waves.

The lateral distribution was studied and the result exhibited by curves for the following cases: (1) A slit about three fourths of a wave-length wide showed a broad spreading of energy, with a central 'bright band' having a dark band and weaker bright band at each side. Their locations agreed with the optical formula, $\lambda = a \sin \theta$. (2) A wider slit showed interference bands nearer together. (3) Case of energy distribution behind a thin metallic edge placed on the axis of a plane wave front. (4) Similar with the edge displaced laterally.

Trouton's discovery that the system of nodes and loops formed by reflection at perpendicular incidence from a small plane mirror is shifted outward if the mirror is made with dimensions of a wave-length or less was verified and the amount of the shifting measured for several cases.

The study of diffraction is being continued.

Final Report on Ether-drift Experiments: Edward W. Morley and Dayton C. Miller.

At the Philadelphia meeting an account was given of experiments to detect ether drift. These observations gave no indications of a drift of the ether. It has been suggested that the negative results are due to the influence of the heavy stone walls of the building within which the apparatus was mounted. The interferometer has, therefore, been mounted on high ground near Cleveland, and covered in such a manner that there is nothing but glass in the direction of the expected drift. It was much more difficult to make observations in this location than in the building; satisfactory observations could only be made on a cloudy evening following a cloudy day, when the temperature changed very slowly. The temperature effects could never be entirely eliminated. The conclusion from many observations is that there was no indication of a drift of the ether through the interferometer. The expected drift would produce a displacement of the interference fringes of 1.53 wave-lengths; the above result is probably certain to one eightieth part of the whole.

The Optical Analogue of Certain Electrical Experiments: William B. Cartmel, Harvard University.

The recent experiments of Messrs. Blake and Fountain show that the amount of electric radiation transmitted by sheets of glass, may be increased by covering the glass with regularly spaced strips of tinfoil, and this led the present writer to investigate the possibility of making a thin film of transparent substance more transparent by covering it with a much thinner film of metal.

A film of dye was deposited upon one half of a plate of glass, and silver was deposited on half the glass but in a direction at right angles to the dye film. One could thus compare silvered dye with silvered glass. The film of silver was so thin that it was impossible to see any difference between the part of the glass that was silvered and the part that was not, but where the glass was covered with both silver and dye, less light was transmitted than when the glass was covered with dye alone. Similar effects were obtained with combined films of silver and selenium.

The explanation is that the phase change at the top surface of the silvered dye film is different from that at the top surface of the bare dye film. The rays reflected from the back surface of the dye will therefore interfere in a different way with the ray reflected from the top surface, in the two cases, and for certain thicknesses of the dye film, the intensity of the light reflected from the film will be diminished by silvering, and there will be a correspondingly increased transmission.

A Preliminary Communication concerning a New Fundamental Principle of the Kinetic Theory of Gases: Luigi d'Auria, Philadelphia.

In this paper the author proves that the square of the speed of an elastic sphere forced to oscillate between two elastic plates perpendicular to the direction of its motion while one of the plates approaches the other with speed which is very small compared with that of the sphere, varies inversely with the length of its free path. The author observes that in a medium composed of numerous elastic spheres moving in all directions in a bounded space, each sphere can be considered as if moving with the mean square speed in a path equal to the mean free path of all the spheres; and, therefore, in accordance with his new theorem, in such a medium the mean square of the speed would vary inversely with the mean free path.

Combining this result with the expression for the mean free path, which, according to Clausius, varies inversely with the density of the medium and the square of the diameter of one of the spheres, and observing that the pressure of the medium upon unit area varies with its density and the mean square of the speed of the spheres, the author arrives at the equation $pv^2 = \text{constant}$, independent of thermodynamic considerations, in which p is the pressure per unit area and v the volume of unit quantity of the medium. Thus in a medium composed of elastic spheres in motion in a bounded space, the pressure per unit area of the bounding surface would vary inversely with the square of the volume.

If the above medium represents a gas, we must have also $pv^\gamma = \text{constant}$, in which γ is the ratio of the specific heats, and therefore, $\gamma = 2$. According to Clausius and Maxwell, for such a gas the ratio of the specific heats would be 5/3, and this is too small to account for the ratio of the specific heats of mercury vapor found by Kundt and Warburg which ranges from 1.631 to 1.695, since the theory requires that this ratio should be considerably less than the theoretical value.

The result $\gamma = 2$ leads to the new fundamental equation $pv = E$ in which E represents the energy of agitation of the gas, and the author shows that this equation accords with the principle of the conservation of energy, and that, therefore, the equation $pv = 2/3E$, which results from the investigations of Clausius and Maxwell, can not be correct.

On the Variation of the Heat of Mixture with Concentration and Temperature: B. M. CLARK.

When two liquids are mixed, there is in general either an absorption or a generation of heat, the amount of heat depending upon the relation of the liquids present, and upon the temperature at which mixture occurs.

The variation of the heat of mixture with temperature—for any one definite concentration—bears theoretically a simple relation to the specific heats of the components of the mixture and the specific heat of the resultant mixture, and the present experiments were begun with a view to determining experimentally the numerical value of this relation in the case of some of the simpler solutions.

Observations were made on the following liquids:

Glycerin—Water,
Glycerin—Ethylalcohol,
Glycerin—Methylalcohol,
Phenol—Water,
Isobutylic Alcohol—Water,
Anilin—Xylol,
Anilin—Toluol.

The values obtained for the heat of solution at various temperatures show that for the liquids tested the thermodynamic relation

$$\partial Q/\partial t = K - K',$$

holds within the limits of experimental error. The value $\partial Q/\partial t$ known as the temperature coefficient of the heat of mixtures varies in some mixtures with the temperature.

Anilin xylol, anilin-toluol gave evidence of a temperature coefficient equal to zero, *i. e.*,

$$\partial Q/\partial t = 0.$$

Numerical values and curves will be given in a paper to appear shortly.

A Color Mixer: ALBERT B. PORTER, Chicago.

The simplest means of mixing and matching colors is undoubtedly Maxwell's color-top. It is, however, defective in that one can not continuously vary the positions of the slotted discs while seeking a color-match, but must usually stop the top many times, readjust the discs, and again set up the rotation, before a satisfactory match is secured.

In the present instrument the slotted discs remain stationary while their image is rapidly rotated. This is effected by viewing the discs through a right-angled prism which rotated with its hypothenuse parallel to the axis of rotation and to the line of sight. As the slotted discs remain stationary, their angular exposure can be easily and continuously varied until the best possible color-match is secured.

On the Nature of Optical Images: ALBERT B. PORTER, Chicago.

A consideration of the method by which light is propagated is sufficient to show that an ordinary optical image is merely a particular case of an interference pattern. This may be easily shown experimentally by using as an object a coarse, black-line grating illuminated by a parallel beam of monochromatic light passing through the grating and then through a convex lens. On the far side of the lens a system of sharply defined interference fringes is formed which can be seen with an eye-piece, or, intercepted on a screen, at any point over a considerable range along the axis. Somewhere in this system of fringes is the geometrical image of the grating, but it is visually quite indistinguishable from other sections of the fringe system. If the angle of incidence of the light falling on the grating is changed, the whole fringe system shifts to one side or the other except in the focal plane, where it remains stationary. This shows that the focal plane of the lens is merely the plane in which the interference fringes formed by light of all incidences coincide, and that the so-called geometrical image is really a superposition of coincident interference patterns; while the usual absence of a sharp image outside the focal plane is due to the more or less uniform illumination caused by the overlapping of fringe systems formed by light coming from various points in the source. When the grating is illuminated by a parallel beam of white light the effects are similar, except that outside the focal plane the interference fringes are colored. This shows that the focal plane is also the plane of achromatic interference, *i. e.*, the plane in which the fringes due to light of various wave-lengths coincide.

On the Conductivity of the Air caused by Certain Compounds during Temperature-Change: Fanny Cook Gates.

Investigations on the conductivity of the air, caused by the presence of the sulphate of quinine under certain conditions, indicate that it accompanies hydration and dehydration, although phosphorescent effects to which it may be directly due, appear at the same time. The phenomenon is most easily observed during the cooling of the quinine from something over 100° C. to room temperature.

Heretofore, the only other substance known to produce a similar effect is cinchonine, which like quinine, hydrates and phosphoresces upon cooling. Both substances are so complex in structure that the exact chemical changes to which they are subject are studied with difficulty.

During the investigations described in the present paper, a large number of substances were heated to about 150° C., and the conductivity of the surrounding gas was tested while they cooled. Of all the substances tested, none was found to give so large an effect as that which results from the sulphate of quinine, but a very definite and marked conductivity was found to accompany the cooling of anthracene, and to a less degree that of grape-sugar and æsculin.

The Transmission of Röntgen Rays through Metallic Sheets: J. M. Adams.

For some years after Röntgen's discovery of the X-rays, it was a matter of doubt whether the absorption of the rays in metallic sheets was accompanied by the development of heat. To investigate this question by means of an instrument different from those already used for this purpose, a radiomicrometer was constructed. The metals forming the thermal couple were constantan and copper. At one of the junctions was placed a small disk of thin platinum to receive the rays, the other junction being shielded from them. The sensitiveness of this radiomicrometer was such that a radiation of 5.6×10^{-8} gm. cal. per sec. per sq. cm. of its sensitive surface produced a deflection of one scale division.

Evidence of heat developed in the platinum by the absorption of Röntgen rays in it was readily obtained. The necessity of making correction for the incomplete absorption of the rays in the platinum of the instrument, together with the well-known fact that the character of Röntgen rays is changed by passage through substances, made it seem desirable to investigate the phenomena of the transmission of the rays through metallic sheets more fully than has been done heretofore.

The general law of the absorption of the rays in a metal, *viz.*, that each successive equal increment of thickness is less effective as an absorbing medium than the one preceding it, was confirmed by experiments with the radiomicrometer.

The dependence of the absorbing power of a given metallic sheet upon the intensity of the rays incident upon it was examined for sheets of silver, platinum, copper, tin, and aluminium, and in every case it was found that the effectiveness of a sheet as an absorbing medium is independent of the intensity of the incident rays.

It was found that the effect of the *surfaces* of metallic sheets upon transmission is small in the case of copper and of aluminium. To show this, a laminated plate of the metal in question was prepared, equal in total thickness to another solid plate of the metal. These two plates were interposed in turn in the path of the rays, and produced equal reductions of the deflection of the radiomicrometer.

The transmission of a beam of Röntgen rays through a metallic sheet has generally been supposed to render the beam more penetrating toward a second sheet of the same or of any other metal than the original beam was.

Evidence of transformation of one sort of ray into another, in transmission through a metallic sheet, was sought, with a negative result.

The Mutual Inductance of a Circle and a Coaxial Helix. The Lorenz Experiment and the Ayrton-Jones Absolute Electrodynamometer: E. B. Rosa.

The constant of the Lorenz apparatus for the absolute measurement of resistance and of the Ayrton-Jones electrodynamometer for the absolute measurement of current require the calculation of the mutual inductance of a circle and a coaxial helix, the circle in the first case being the edge of the rotating disc and in the second case one end of the suspended coil, the helix being the fixed coil carrying the primary current.

Jones obtained an expression for the mutual inductance in question which is a very tedious and difficult one to use in numerical calculations. Lorenz's expression is an algebraic series less difficult but also less accurate than Jones's. Using an expansion in zonal harmonics, I have obtained an impression in the form of an algebraic series, similar to Lorenz's but more accurate, which is far more convenient to use than Jones's and agreeing with the latter to less than one part in a million (in a particular numerical test) thus being amply accurate for the most refined experimental work. This expression is obtained on the assumption that the current is distributed over the solenoid in a uniform current sheet, whereas Jones's expression assumed the current flowing in a helix. Their agreement is a confirmation of the theorem that a spiral distribution of current is equivalent to a current sheet.

Mutual Inductances for Laboratory Use: E. B. Rosa.

The most accurate as well as the most convenient method of calibrating a ballistic galvanometer is by means of a mutual inductance, through the primary of which a measured current flows. Some laboratories use a standard solenoid with a secondary wound within or without, calculating the mutual inductance of the primary and secondary coils from their dimensions. Such a solenoid made large enough and carefully enough to give the mutual inductance with fair precision is both bulky and expensive, and not very portable. Being of considerable length, its magnetic field often extends to a considerable distance and may disturb other work. On the other hand, suitable primary and secondary coils may be wound on a thoroughly seasoned wood spool (or a marble spool) only 10 to 12 cm. in diameter and afford, when calibrated, an accurate and convenient mutual inductance for the calibration of galvanometers that is cheap, portable and reliable. Such inductances are so inexpensive and occupy so little space that one can be left with its secondary in circuit with the galvanometer, ready to be used for calibrating the latter at any time. They can be made in the laboratory at trifling cost and can be sent by mail or express to the Bureau of Standards and calibrated at very slight expense. Specifications for such coils will shortly be published by the bureau.

The Mutual Inductance of Two Coaxial Coils: E. B. Rosa.

Absolute measurements of resistance have been made by Rowland, Glazebrook and others, using two coaxial coils the mutual inductance of which was computed from their dimensions. By the methods employed by Kohlrausch and Rayleigh the mean radius of such coils can be obtained with great precision by comparing them with larger coils wound with a single layer of wire, the mean radius of which may be determined with sufficient accuracy by direct measurement. The formulæ employed by Rowland and Glazebrook in calculating the mutual inductances of their coils are both approximate, and do not agree with one another closely unless the coils have very small sections or are rather far apart. Weinstein and Stefan both gave formulæ for use in calculating the mutual inductance of such coils, but they do not agree with one another as accurately as is desirable for precision work. I have revised and corrected Weinstein's formula and derived a new formula starting where Stefan began, these two formulæ agreeing very closely and proving more accurate than any of the others. These formulæ are sufficiently accurate for the most refined work in the absolute measurement of resistance.

Remarkable Optical Properties of Carborundum: Lewis E. Jewell.

Some transparent blue plates of carborundum, found in 1904, gave by preliminary measures with a microscope a very high value for the refractive index.

Later, in material received from the Carborundum Company of Niagara Falls, were found some transparent and colorless plates, some of which were fairly thick. The polarization of the plates was studied with the polariscope and one specimen of twinned crystals produced a well-defined, transparent, colorless prism where the plates were joined.

Measurements with the spectrometer were made and the refraction proved to be greater than that of the diamond, and the dispersion more than twice as great.

Spectrograms were obtained of the light transmitted through the plates and a thickness of one thirtieth of a millimeter absorbed practically all light having a wavelength less than 4,000 Ångström units.

Interference bands in the spectrum of light transmitted through their parallel plates gave greater values of the dispersion than the measurements with the spectrometer. Spectrograms have also been obtained of the light reflected from the surfaces of the crystals and the reflective power, which is extremely high, differs very little in the visible and ultra-violet portions of the spectrum until a wavelength of about 2,400 is reached, beyond which there seems to be an absorption band.

Very remarkable markings have been found upon some plates, the most remarkable of which take the form of spirals, some of which are hexagonal and others triangular, changing to hexagonal farther out. Most of the spirals are, however, circular; some of them are so close together and so regular as to form brilliant spectra, even to the sixth or eighth order; the lines in some cases are as close together as thirty or fifty thousand to the inch. Many of these markings are extremely beautiful.

Sometimes there are two outer coatings

to the plates, the outer an oxidation product giving the brilliant surface colors which are due to a thin film. Sometimes there is another layer with an exceptionally brilliant silver-like surface.

The Absorption of Some Solids for Light of Short Wave-length: Theodore Lyman.

The grating spectroscope which the author has used in his measurement of the hydrogen spectrum between 1,650 and 1,250 Ångström units, though well adapted for the determination of wave-lengths, is not especially fitted for rapid work in other branches of the subject. A prism spectroscope would be preferable for many purposes on account of its short light path and the superior intensity of its spectrum.

The first purpose, therefore, of the present research was a purely practical one, namely, to find some substance from which the prism and lenses of such an instrument might be constructed. White fluorite is the only known substance fitted for the purpose, and it was hoped that a less costly substitute might be discovered.

In order to test rapidly the transparency of a great number of substances an instrument of special type was constructed. In it the light from a discharge tube fell upon a concave mirror and was thrown by it through a fluorite prism on to a screen coated with willimite. The whole apparatus was enclosed in an air-tight case which could be exhausted and filled with hydrogen. The specimens to be examined were introduced into this case in such a manner that eight of them could be interposed in succession between the mirror and source of light without opening the apparatus. The extent and intensity of the spectrum on the fluorescent screen was observed through a glass window.

The substances examined were: Colored fluorites, quartz, topaz, gypsum, celestite, rock salt; barite alum, colemanite, sugar (rock candy), borax adularia, calcite, chrysoberyl, sanidin, anagonite, and apophyllite. The results are:

I. No substance shows so great transparency as white fluorite.

II. The transparency of colored fluorites varies through a considerable range, but some light green specimens are nearly as good as the white variety.

III. Colored fluorites may be deprived of their color by heating. The process does not materially alter their transparency.

IV. Quartz in thicknesses of about 1 mm. is transparent to λ 1,500. The absorption increases with such rapidity with the thickness that prisms and lenses would cut the spectrum off at about λ 1,750 for all practical purposes.

V. Of the remaining substances Ceylon topaz is the best, being transparent to λ 1,560.

VI. Rock salt is transparent to λ 1,750 only.

Geometrical Theory of Radiating Surfaces with Discussion of Light Tubes: Edward P. Hyde.

Theoretical photometry assumes two general laws of radiation. (1) The law of variation of the intensity of illumination of a surface in face in inverse proportion to the square of the distance of the surface from the luminous source is merely a statement of a geometrical property, if the rectilinear propagation of light is assumed. (2) Lambert's law of variation of the intensity of a luminous surface in direct proportion to the cosine of the angle of emission is an empirical law based primarily on the observation that a uniformly bright sphere, when viewed at a distance, appears as a uniformly bright disk. It would seem to follow from Kirchhoff's law that Lambert's cosine law can be true only for a black body, but no satisfactory experiments have been made, so far as the writer knows, to test the law empirically in its application to glowing surfaces.

These two laws, the inverse-square law and the cosine law, are applicable to the infinitesimal elements of a radiating surface, and large errors may result if they are assumed to apply to an extended source as a whole. Particularly is this so in the case of the inverse-square law, which underlies the great majority of photometric measurements.

In each of the above two cases the expression is deduced for the illumination at different distances in a single plane or along a single line symmetrical with respect to the radiating surface. In the case of a uniformly bright strip of infinite length but of finite width it is not difficult to derive the expression which will give the illumination at any point in space. Instead of illumination, however, the more general term *specific luminous flux* is substituted. At every point in space there is some definite direction in which the flux of luminous energy is a maximum. The quantity of luminous energy which in one second flows normally across a surface of unit area placed perpendicular to the direction of maximum flux is defined as the *specific luminous flux* at the point. It is a vector quantity, and the component in any direction equals numerically the difference in illumination on the two sides of an infinitely thin material screen placed perpendicular to the direction.

Two examples are given of the errors incident to assuming for a finite surface the inverse square law, which only applies to the elements of the surface; the above case of an infinite strip is used to show the errors incident to assuming for the strip as a whole the cosine law, which is true only for the elements of surface of the strip. The value of ϕ_0 at different distances in a direction normal to the strip at its middle point, and in a direction making an angle of 45° with the normal at the middle point, are calculated. For any definite distance the former multiplied by cos 45° would equal the latter if the cosine law, which has been assumed for the elements of surface, applied to the surface as a whole. The difference between the two values gives the errors for the distance used, and by plotting the errors for different distances a curve of deviation from the cosine law is obtained.

On the Magnetic Properties of Heusler's Alloys: J. C. McLennan.

In this paper the author describes some experiments made during the past year by Messrs. Dawes, McTaggart and Robertson, and Mr. L. B. Johnston, on the magnetostriction and permeability of Heusler's alloys of varying composition.

From measurements on both phenomena the alloys are shown to be in an exceedingly unstable condition when freshly made, and from observed changes in their magnetic behavior the conclusion is drawn that profound modifications are made in their structure through the lapse of time as well as by their being subjected to changes in temperature and to repeated magnetizations and demagnetizations.

In connection with the phenomenon of magnetostriction the gradual shortening observed by Austin with rods of the alloys subjected to long-continued high magnetic fields is shown to disappear when the rods have reached a stable condition after being repeatedly magnetized.

In a series of rods containing the same amount of manganese with varying amounts of aluminium the greatest elongation was observed with rods in which the manganese and aluminium were present in the ratio of their atomic weights.

The paper concludes by emphasizing the suggestion made by Hill that possibly the magnetic properties of these alloys at room temperatures are largely determined by the temperatures from which they have been cooled, and that by suitably heating samples of the alloys to different temperatures and then chilling them their magnetic properties at these temperatures may be ascertained, just as the structures of other alloys at different temperatures have been investigated in this way by Neville and Heycock.

On the Magnetic Susceptibilities of Mixtures of Salt Solutions: J. C. McLennan and C. S. Wright.

In this paper the authors give some measurements on the magnetic susceptibilities of solutions of manganese, aluminium, and copper sulphates in water, and several mixtures of these solutions made with the object of obtaining information which might be of service in explaining the behavior of the magnetic alloys, recently discovered by Heusler. The method followed in measuring the susceptibilities is that suggested by Kelvin, in which a glass cell of the solution investigated is placed in a strong magnetic field and the susceptibility deduced from the pull exerted on the solution by the field.

The magnetic susceptibility of water was found to be -7.33×10^{-7}. Measurements on a series of salt solutions gave the following molecular susceptibilities:

		M_6
Manganese sulphate	$MnSO_4$	$+.01491$
Copper sulphate	$CuSO_4$	$+.00153$
Aluminium sulphate	$Al_2(SO_4)_3$	$-.00018$
Aluminium nitrate	$Al_2(NO_3)_6$	$+.00002$
Aluminium chloride	$Al_2(Cl)_6$	$-.00005$

A set of measurements on solutions of manganese sulphate of different concentrations showed that the molecular susceptibility of the salt was independent of the concentration.

On Magnetic Shielding: A. P. Wills.

Assuming the results of a previous paper, giving the 'shielding ratio' for a set of three concentric spherical iron shells and for a similar set of cylindrical shells, the following problem was discussed: Given the innermost and outermost radii of the system in each case, what values should the remaining four radii have in order that the shielding shall be a maximum? Starting with the expression giving the 'shielding ratio' (the ratio of field impressed to field within innermost shell), derived in the paper referred to above, the conditions for a maximum of the expression, under the conditions imposed, are examined; and it is found that approximately the best conditions are obtained when the radii of the shells are in geometrical progression. This holds for both spherical and cylindrical systems.

Models Illustrating the Motion of a Violin String: Harvey N. Davis.

The function $u(x, t)$ which gives the displacement, at the time t, of a point x units from one end of a violin string, can be represented graphically by a surface with the x, t plane as a base-plane. If the units were properly chosen, this graph would be the surface which the string would generate if, while it vibrated, it were also carried along in a direction perpendicular to the plane of its vibration; and in any case, a section of such a surface parallel to the x axis represents, usually on a magnified scale, the configuration of the string at some corresponding time, $t=t_0$, while one parallel to the t axis represents the displacement of a corresponding point, $x=x_0$, as a function of the time.

Five surfaces of this kind, modeled in three dimensions, were shown, representing, one the general Helmholtzian solution and the other the motion of a string bowed at points 1/5, 2/5, 2/7 and 3/8 of its length from one end. These particular cases were chosen because, besides being typical, they have some interest in connection with Young's and Krigar-Menzal's observations on the absence or dominance of certain overtones when a string is bowed at or near one of their nodes.

The Motion of a Violin String under Light Bowing: Harvey N. Davis.

This paper discusses the influence which the pressure of the bow upon the string has on the resulting vibration form. For each bowing speed there is, for comparatively great pressure, a considerable range within which the only effect of a change in the pressure is a slight corresponding change in the position of equilibrium about which the string vibrates, the vibration form being always that described by Helmholtz and others, and the amplitude remaining the same. For smaller pressures both the amplitude and the vibration form change with the pressure. In particular, if the pressure is below a certain critical value, determined partly by the materials and condition of the apparatus and partly by the bowing speed, no vibrations can be maintained. For pressures slightly greater than this critical pressure there are no overtones, the time graph of the displacement of the point under the bow being the sine curve tangent at its point of greatest slope to a line representing the speed of the bow. As the pressure is increased beyond this value, the bowing speed remaining constant, the mode of vibration goes over continuously into the Helmholtzian form.

On Distributions of Nuclei and Ions in Dust-free Air: Carl Barus.

I have recently found it desirable to gather my data together for comparison. There is, in fact, a serious discrepancy between Mr. C. T. R. Wilson's results and mine when reduced to the same scale. Mr. Wilson's supersaturations for negative ions and cloud are distinctly higher, which certainly can not mean that my fog chamber is in these regions inferior to his own. Thus in moderately ionized dust-free air my condensations begin at a drop of about 18.5 cm. from 76 cm. as compared with 20.5 in Wilson's apparatus; similarly my fogs begin at the drop 20.3, Wilson's at 27.7. Furthermore, at low ionization even the vapor nuclei of dust-free wet air become efficient in the presence of ions. It seems impossible, therefore, that any positive ions should fail of capture. The question is to be asked why I catch the negative ions, etc., at an apparently much lower supersaturation than C. T. R. Wilson. I have entertained doubts whether the inertia of the piston in his apparatus is initially quite negligible; whether in any apparatus the computed adiabatic temperatures were actually reached. Nobody has proved it, and the case is worse for tubes. Moreover, in every apparatus there must be a limit at which the smaller nuclei of a graded system can no longer be caught in the presence of the larger nuclei. But I do not believe that the real discrepancy will be found in any of these misgivings. It seems to me to be inherent in this; in Wilson's apparatus the results are given from the observed volume ratio v_1/v of adiabatic expansion; in my method the results follow from the observed pressure ratio p/p_1. It seems questionable whether the customary constants by which one passes from one group of data to the other are really applicable, to wet air at very low temperatures. Moreover, when the exhaust cock in my apparatus is opened for

$t = 0$ .25 .5 1 2.5 5 ∞ sec.

The isothermal pressures of the fog chamber (cæt. par.) read

$\bar{p}_2 =$ (57.8) 53.2 52.7 52.0 51.5 50.9 50.4 cm., so that in the first quarter second of opening the final isothermal pressure p_3 (chambers communicating) is already reached to more than 60 per cent., and my smallest fog chamber holds over 6 liters. One naturally asks whether the importance of this in its bearing on the measurements of the ratio of specific heats k/c, has ever been adequately appreciated.

Fluorescence Absorption: E. L. Nichols and Ernest Merritt, Cornell University.

Energy Necessary to Ionize a Molecule by Impact of Negative Electrons: Bergen Davis, Columbia University.

The Transformation into an Electric Current of Radiation Incident on a Moving Surface: Bergen Davis, Columbia University.

The Standard Cell: F. A. Wolff and C. E. Waters, Bureau of Standards.

The Equilibrium of Mercurous Sulphate and Mercury and Cadmium Sulphate: F. A. Wolff and C. E. Waters, Bureau of Standards.

The Distribution of Energy emitted by a Righi Vibrator: C. R. Fountain.

The Constants in Gas-viscosity: Willard J. Fisher, Cornell University.

Production of Radium by Actinium: Bertram B. Boltwood.

Production of Radium from Actinium: E. Rutherford.

The Influence of Electrical Fields upon Spectral Lines: G. F. Hull, Dartmouth College.

Helion, a New Incandescent Lamp Filament: H. C. Parker and W. G. Clark.

The Magnetic Rotation of Sodium Vapor at the D lines: R. W. Wood.

Flourescence Spectra of Mercury Vapor: R. W. Wood.

Hydraulic Analogy of the Welsbach Mantle and other Radiators: R. W. Wood.

The Shielding of a Highly Sensitive Galvanometer: E. F. Nichols and S. R. Williams.

On the Temperature of the Mercury Arc: Charles T. Knipp.

A Study of the Reversible Pendulum: John C. Shedd and James A. Birchby.

Wave-metrical Measurements with Wireless Telegraph Circuits: G. W. Pierce.

The Electrical Properties of Carborundum: G. W. Pierce.

Dayton C. Miller,
Secretary of Section B

SCIENTIFIC BOOKS

Qualitative Analysis as a Laboratory Basis for the Study of General Inorganic Chemistry. By William Conger Morgan, Ph.D. (Yale), Assistant Professor of Chemistry in the University of California. New York, The Macmillan Company; London, Macmillan & Co., Ltd. 1906. Pp. xiv + 351.

That the last word as to the best method of teaching chemistry has not yet been spoken is evidenced by the number of new text-books in general and analytical chemistry. Such a multiplicity of new books may be from a financial standpoint unsatisfactory to authors and publishers, but it reveals an activity and healthy independence on the part of teachers of chemistry. Most if not all these books are written, not to sell, but to bring out the writer's views for his own classes.

The latest book on qualitative analysis is that by Dr. Morgan, and is to some extent along new lines. Most teachers of chemistry in colleges are confronted with a difficulty arising from the chemistry of fitting schools. Comparatively few students present themselves for entrance to college well grounded in general chemistry, especially as viewed from the modern physical chemical standpoint, and yet these men are too advanced to be put in a class which is open to beginners. They have often studied much descriptive chemistry, but are not ready for analytical chemistry as ordinarily taught. Confronted by this difficulty at the University of California, Dr. Morgan has prepared this book, in which "the scheme of qualitative analysis is made to serve as a means of correlating the apparently independent experiments of general chemistry." It is intended primarily for those, "the exigencies of whose vocational courses render it impracticable" for them "to devote more than one year to general inorganic chemistry and to qualitative analysis as well." The author well says in the preface: "Instruction in science should endeavor to equip the student with principles rather than facts, and, what is of still greater import, it should train him in the use of these principles and in the application of them in explaining the phenomena of general experience."

The book is divided into four parts: General, Descriptive, Analytical, Appendix. Part I. is an outline in sixty pages of the principles of chemistry from the standpoint of physical chemistry, presupposing a knowledge on the part of the student, of general chemistry as taught in schools. The topics Dissociation, Equilibrium and Mass Action, Hydrolysis, Repression of Ionization, Solute and Solvent, and Oxidation and Reduction, are treated with satisfactory clearness, and the judicious use of black-letter for important topics and italics for important principles should greatly assist the student. The abandonment of the sign of equations and substitution therefor of the arrow to indicate the general direction of the reaction is a recent innovation, and has much to commend it, and may save the student from having later to reverse the early acquired notion that a single equation expresses quantitatively all that takes place in a reaction. It serves also to emphasize the important conception of equilibrium.

Part II. comprises about half the book. At first sight, this part, which is a study of the reactions of the elements, would seem to follow the plan of many older text-books on qualitative analysis, which weary the student with a mass of test-tube reactions, before he comes to any application of their use. In this book we have rather a systematic study of the elements in the order of their occurrence in the periodic table, with special reference to their ions. The method of treatment may be illustrated by the topic Arsenic. This opens with a brief description of the element; then follows its oxidation and solution; the existence of arsenic ions; its reduction; the Bettendorf, Marsh and Reinsch tests; the relation of the arsenious kathion to the hydroxyl, carbonate, sulfid, sulfate and chlorid anions; arsenic compounds; arsenious acid and arsenites; salts of thioarsenious acid; arsenic acid and the reactions of its ion with barium, silver, magnesium and molybdate ions; salts of the thioarsenic acids. One great advantage of this method of study is that the student gains a comprehensive view of the element in all its compounds, and that the sequence of elements according to the periodic table shows him the relation the element bears to its neighbors. Further, those compounds and reactions chosen as illustrations are the ones he meets in analytical chemistry. This division of the book, as far as I can recall, attempts something unique; how it will work practically in other hands than the author's remains to be seen, but it reads as if it would prove successful.

Part III., on qualitative analysis, opens with solution and preliminary examination of solids. Then follow tables for separations of bases, each with a full discussion conveniently arranged below and on the opposite page. The analysis of acids is similarly arranged with the discussions opposite the tests. Mention should be made of two or three commendable procedures in the course of analysis. In the basic analysis the detection and removal of interfering substances in the filtrate from the hydrogen sulfid precipitate are excellently treated; aluminum, chromium and iron are precipitated as basic acetates, and the chromium oxidized with sodium peroxid; cobalt is removed from the nickel-cobalt solution by potassium ferricyanid in the presence of ammonia and a little alum solution, to assist in collecting the cobalt ferricyanid for filtration;

the nickel is recognized in the filtrate by adding a little piece of solid caustic soda. In the acid analysis the acids are classified by the character of their barium and silver salts, and their most characteristic reactions well discussed.

Part IV., the appendix, contains the preparation of reagents, specific gravity and solubility tables, and considerable physical data. In the strength of reagents, it is gratifying to see that another convert has been added to the comparatively few teachers who have adopted the Reddrop system of normal reagents. The great advantage of the system is that the student knows the strength of the reagents he is using, and soon comes to avoid the use of great excess. Unconsciously he becomes familiar with the elements of volumetric analysis. The strengths recommended by the author differ a little from those originally suggested by Reddrop. For dilute acids and alkalies, $4N$ solutions are used, and for salts $N/2$ generally. The ordinary reagents are N. In this laboratory $5N$ for acids, $5/2\,N$ for alkalies, and $N/5$ for most salts have been found convenient.

The press work of the book is excellent and typographical errors are very few. There is a complete index.

Jas. Lewis Howe
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SCIENTIFIC JOURNALS AND ARTICLES

The March number (volume 13, number 6) of the *Bulletin of the American Mathematical Society* contains the following articles: Report of the Thirteenth Annual Meeting of the American Mathematical Society, by F. N. Cole; Report of the December Meeting of the Chicago Section, by H. E. Slaught; 'The Decomposition of Modular Systems Connected with the Doubly Generalized Fermat Theorem,' by E. H. Moore; 'Systems of Extremals in the Calculus of Variations,' by Edward Kasner; 'A Necessary Condition for an Extremum of a Double Integral,' by Max Mason; Shorter Notices; Nielsen's Handbuch der Theorie der Gammafunktion, by Virgil Snyder; Jouffret's Mélanges de Géométrie à Quatre Dimensions, by Peter Field; Lanner's Neuere Darstellungen der Grundprobleme der reinen Mathematik im Bereiche der Mittelschule, by D. E. Smith; Reformvorschläge für den mathematischen und naturwissenschaftlichen Unterricht, entworfen von der Unterrichtskommission der Gesellschaft deutscher Naturforscher und Aerzte (Zweiter Teil), by J. W. A. Young; de Peslouan's N. H. Abel, sa Vie et son Oeuvre, by Florian Cajori; 'Notes'; 'New Publications.'

The April number contains: Report of the February Meeting of the American Mathematical Society, by F. N. Cole; 'The Construction of a Field of Extermals about a Given Point,' by G. A. Bliss; 'Some Particular Solutions in the Problem of n Bodies,' by W. R. Longley; 'On the Matrices of Period a Power of p in Jordan's Linear Congruence Groups, Modulo p^{α},' by Arthur Ranum; 'On the Construction of an Integral of Lagrange's Equations in the Calculus of Variations,' by D. C. Gillespie; 'Algebraic Numbers and Forms' (Review of Bachmann's Allgemeine Arithmetik der Zahlenkörper and König's Einleitung in die allgemeine Theorie der Algebraischen Grössen), by L. E. Dickson; 'Notes'; 'New Publications.'

SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY, NEW YORK SECTION

The fifth regular meeting of the session of 1906–07 was held at the Chemists' Club, 108 W. 55th Street, on March 8.

Pursuant to the amendment to the by-laws of the section adopted February 8, the annual election of officers, to assume their duties at the close of the June meeting following, was held with the following result:

Chairman—H. C. Sherman.
Vice-Chairman—F. J. Pond.
Secretary and Treasurer—C. M. Joyce.
Executive Committee—Virgil Coblentz, G. C. Stone, C. H. Kiessig, Durand Woodman.

The chairman called attention to the great loss to the society occasioned by the untimely death of its honorary member, Henri Moissan, and Dr. C. A. Doremus presented an obituary notice.

Professor S. C. Prescott, of the biological department of the Massachusetts Institute of Technology, spoke 'On the Utilization of Micro-organisms in Industrial Processes.'

This wide subject could only be considered very briefly during the short time available and the lecture was devoted to an outline of the advances made in the fermentation, milk and canning industries through the technical application of the science of biology.

A historical sketch was given of Pasteur's studies of the diseases of wine and beer and how these finally led to the use of pure yeast cultures, in the brewing industry. The advantages of the further use of pure cultures in wine making were pointed out and their application in the manufacture of other fermentation products such as spirits, vinegar, lactic and butyric acid was suggested.

Plates were shown which indicated in a striking manner the vastly increased purity of milk brought about by the use of proper sanitary precautions in the barns of the small farmer where much can be accomplished by intelligent inspection.

In the food-preserving industries, great advances have been made by isolating the germs producing decay and finding experimentally what treatment will destroy them. For example, the use of superheated steam in sterilizing cans of corn before sealing will accomplish the result in ten minutes instead of the four hours required when boiling water was used, whereas the latter readily sterilizes preparations of fruits. The use of such knowledge as this has brought about a considerable improvement in the quality of the finished products.

The inoculation of soils with nitrogen-fixing bacteria was discussed, and some very favorable results obtained in experiment stations were described. The bad showing made with the use of cultures sent out by the Department of Agriculture was attributed to their becoming partly, and at times completely, sterile during the drying process preparatory to shipping.

Professor Prescott showed numerous slides to illustrate the different subjects discussed and his lecture was much appreciated by a large attendance.

C. M. Joyce,
Secretary

SECTION OF GEOLOGY AND MINERALOGY, NEW YORK ACADEMY OF SCIENCES

At the monthly meeting of the academy on January 7, Dr. E. O. Hovey presented notes on a recent visit to the volcanoes of Toluca, Colima and Popocatepetl in Mexico, illustrated by a series of lantern slides. The section then proceeded to an examination, in adjoining rooms, of the exhibits of geology, paleontology and mineralogy in the New York Academy of Science's exhibition, under the guidance of committeemen in charge of the exhibits.

At the monthly meeting, February 11, Dr. John M. Clarke, state geologist of New York, gave an informal description of the geography of the Atlantic Devonian, with lantern illustrations. Professor James F. Kemp also offered notes on mineral localities visited during the summer of 1906 in Canada and Mexico, and exhibited a collection of specimens of minerals and ores.

At the monthly meeting, March 4, the session was devoted to a description of Letchworth Park (Glen Iris), the new State Reservation on the Genesee River, New York, recently presented to the state of New York by Mr. William Pryor Letchworth.

Professor A. W. Grabau first described the geology and scenery of the Upper Genesee falls and gorges.

In the first part of the paper the speaker discussed studies made by him for some years on the drainage systems of central New York in preglacial time. It was pointed out that all the characteristics of the ancient valleys indicate a southward drainage in late Tertiary time. In all cases where the valleys are traceable they unite southward into trunk streams, a condition wholly inexplicable on the supposition that these valleys were formed by northward-flowing streams. This is readily seen by an inspection of the topographic sheets as well as of the magnificent geologic sheets of this section recently published by the state survey. Where the connection is broken, this can generally be shown to be due to drift deposits.

The following drainage systems were tentatively outlined, the outline being presented as a report of progress rather than as a final settlement in any one case.

On the west, the Wyoming (Warsaw) valley probably had the Dale valley, now occupied in part by the Little Tonawanda, as a western branch, joining it north of Warsaw. The Warsaw valley is still believed to have been continuous with the Upper Genesee valley, above Portageville, by way of Glen Iris, as outlined by the speaker in 1894 and earlier. The valley of Silver Lake joined the Warsaw valley somewhere near Silver Springs. A narrower valley, now occupied by the Genesee from Gibsonville to St. Helena, is continued by a buried gorge from that place to Portageville, where it joins with the Warsaw-Glen Iris valley and another valley from the northwest, to continue southward in the large valley now occupied by the Upper Genesee.

The Canasseraga valley, now occupied in part by the Genesee, was cut by an independent stream. This is the largest valley of the region and was that of the master stream. The Nunda-Cashaqua valley, generally held to have been the former path of the Genesee, is probably only an inner-lowland type of valley, carved on the contact between Portage shales and Chemung sandstones. It may have been in part a tributary of the Genesee at Portageville. The Canasseraga, above the junction of the Cashaqua, is as broad and flat-bottomed as below that point, and was certainly continuous throughout, being carved by a single stream, the Tertiary Canasseraga, as suggested nearly fifteen years ago by the speaker. This river, flowing southward, received as a tributary the Conesus, the valley of which is broad and open to Scottsburg. Hemlock and Canadice rivers joined southward, receiving another branch near Springwater, the united series joining the Canasseraga by way of Wayland. Honeoye and Canandaigua rivers, joined near Naples having another eastern branch in West River. Originally this series may have drained southward by way of Cohocton, but may later have been captured by a branch of the Canasseraga. This proposition, however, needs careful study. Another branch of this system seems to have been the Flint, the valley of which, traceable for twenty miles or more, points toward the Cohocton outlet. Another system is represented by the two branches of Keuka Lake, which have other branches uniting with them southward.

Other systems are represented by the valleys of the more eastern lakes. So far as the study has proceeded, these valleys could only have been formed by a southward drainage, as outlined in Bulletin 45, New York State Museum.

The remainder of the paper consisted of a description of the gorges and falls about Portage, illustrated with lantern slides. The successive stages in the development of the lower falls received special attention.

Dr. George F. Kunz then presented a plan of development of the park as a means for scientific education. Both papers were illustrated with series of beautiful lantern slides.

Alexis A. Julien,
Secretary of Section

THE TORREY BOTANICAL CLUB

The meeting of December 11, 1906 was called to order at 8:15 o'clock P.M., at the American Museum of Natural History, with President Rusby in the chair. Eight persons were present.

The scientific program was as follows:

'*Some Hawthorns of the Vicinity of New York City*': Mr. W. W. Eggleston.

Species and variations of *Cratægus* growing within the vicinity of New York City were described and illustrated by herbarium specimens.

'*Centers of Distribution of Coastal Plain Plants*': Roland M. Harper.

One of the most familiar phenomena of plant distribution is that neighboring areas of equal extent often differ considerably in the number of species they contain. And it usually happens that a region with a rich flora (if a large enough area be taken into consideration) contains a considerable number of endemic species, also that many species which are not endemic grow more abundantly or vigorously in such places than in other parts of their ranges.

A well-known example of a center of distribution is the southern Appalachian region, which has the greatest variety of trees to be found anywhere in temperate eastern North America, most of which grow larger there than anywhere else, and many species are now confined to that region, though some of them were doubtless more widely distributed in prehistoric times. Isolated islands and mountain peaks in all parts of the world are also noted for their endemic species.

Our Atlantic coastal plain (shown on map which was exhibited), though in some respects a unit, contains several fairly well defined centers of distribution. Beginning at the northern end, the first center to be considered is the so-called 'pine-barrens' of New Jersey. From the available literature if would seem that the following species are either confined to that region or else are much commoner in New Jersey than in adjoining states.

Schizæa pusilla, Sporobolus compressus, Dichromena colorata, Rhynchospora pallida, R. Knieskernii, R. Torreyana, Xyris fimbriata, X. flexuosa (*torta* of most authors), *Eriocaulon Parkeri, Juncus Cæsariensis, Abama Americana, Helonias bullata, Xerophyllum asphodeloides, Oceanoros leimanthoides, Tofieldia racemosa, Uvularia sessilifolia nitida, Aletris aurea, Lophiola aurea, Gyrotheca tinctoria, Pogonia divaricata, Arenaria Caroliniana, Drosera filiformis, Corema Conradii, Ilex glabra, Hypericum adpressum, Rhexia aristosa, Dendrium buxifolium, Pyxidanthera barbulata, Gentiana Porphyrio, Sclerolepis uniflora, Chrysopsis falcata, Coreopsis rosea.*

Most of these are monocotyledons, and there are more species of Melanthaceæ in the list than of any other one family.

The next well-marked coastal plain center seems to be in the southern corner of North Carolina. The following species are rarely if ever seen more than 100 miles from Wilmington.

Tofieldia glabra, Hypoxis micrantha, Dionæa muscipula, Kalmia cuneata, Coreopsis falcata, Leptopoda Curtisii.

The following species of wider distribution seem to be more abundant within about 50 miles of Wilmington than they are at a distance of 100 to 200 miles in either direction.

Selaginella acanthonota, Pinus palustris, P. serotina, Aristida stricta, Campulosus aromaticus, Dichromena latifolia, Zygadenus glaberrimus, Lilium Catesbæi, Smilax laurifolia, Habenaria blephariglottis, Nymphæa sagittifolia, Amorpha herbacea, Polygala lutea, P. ramosa, Gordonia Lasianthus, Cyrilla racemiflora, Clethra alnifolia, Vaccinium crassifolium, Sabbatia lanceolata, Carphephorus bellidifolius, Aster squarrosus, Marshallia graminifolia.

By far the greatest center of pine-barren plants or perhaps an aggregation of two or more sub-centers, is in Georgia and northern Florida. Probably three fourths if not nine tenths of all pine-barren species can be found in Georgia, at least a dozen are confined to that state, and many more to Georgia and Florida together. In the Altamaha Grit region (the middle third of the coastal plain) of Georgia there are nearly 150 species on sand-hills, about the same in dry pine-barrens, 200 in moist pine-barrens, and 75 in pine-barren ponds. These numbers are undoubtedly larger than for the same habitats in any other state unless it is Florida.

In subtropical Florida there are, of course, many plants not found farther north, but practically all of these center in the tropics, and are therefore outside of the region under consideration.

Going westward from Florida we find in the vicinity of Mobile and Pensacola a center comparable with that in southern North Carolina. To this belong *Myrica inodora, Sarracenia Drummondii, Drosera filiformis Tracyi, Pitcheria galactioides,* and perhaps *Carphephorus Pseudo-Liatris. Chamæcyparis thyoides* and *Sarracenia purpurea,* which are as common within fifty miles of Mobile Bay as they are in New England, seem to be entirely wanting at twice that distance, and do not appear again within two or three hundred miles, as far as known.

Pine-barrens extend as far west as Texas, and there ought to be some species of pine-barren plants confined to Louisiana and Texas, but too little is known of the flora of those parts as yet.

Plants of muddy swamps seem from all accounts to be most numerous in the Mississippi embayment of the coastal plain, from about the mouth of the Ohio River southward. Characteristic species of this region, most of them woody plants, are:

Taxodium distichum, Echinodorus radicans, Arundinaria macrosperma, Hymenocallis occidentalis, Leitneria Floridana, Hicoria Pecan, H. aquatica, Quercus Michauxii, Q. lyrata, Planera aquatica, Celtis occidentalis, Brunnichia cirrhosa, Platanus occidentalis, Cratægus viridis, C. apiifolia, Amorpha fruticosa, Ilex decidua, Acer saccharinum (dasycarpum), Berchemia scandens, Nyssa uniflora, Bumelia lycioides, Adelia acuminata, Trachelospermum difforme, Asclepias perennis, Gonolobus lævis, Vincetoxicum gonocarpos, Bignonia cruciglia, Tecoma radicans, Conoclinium cœlestinum, Mikania scandens, Eupatorium serotinum.

Most of these are not wholly confined to the coastal plain, but they are more common there than elsewhere, and few if any of them ever ascend more than 1,000 feet above sea-level. Going eastward in the coastal plain they become perceptibly scarcer. There are fewer of them in Georgia than in Alabama, still fewer in the Carolinas, and only about half of them reach Virginia, though there is nothing in the climate to hinder them, as far as known.

In contrast to these five or six evident centers a few of the regions with poorer flora may be mentioned.

The coastal plain of Delaware, Maryland and Virginia seems to lack many of the species common to New Jersey and the southern pine-barrens, though some of them will probably be reported when those parts are better explored. South Carolina too seems to be a rather uninteresting state floristically, and there are perhaps no good species confined to it. The upper fourth of the coastal plain of Georgia (*i. e.*, that part outside of the pine-barrens) has quite a diversified topography and vegetation, but practically all the plants growing there range either northward to the mountains or coastward to the pine-barrens.

A part of the Cretaceous and Eocene regions of the coastal plain from western Alabama through northern Mississippi and West Tennessee to Kentucky is remarkable for the paucity of its flora. It is entirely outside of the pine-barrens, and nearly all of its species seem to be common and widely distributed. The same remarks will probably apply to the coastal plain of Arkansas.

The ultimate reason why so many species are found in some parts of the coastal plain and so few in others is still obscure, and perhaps each center will require a different explanation. But the importance of locating these centers is obvious; for any one who wishes merely to collect as many species as possible will save time by confining his operations to the vicinity of known centers, and the possibilities of discovering new species are greater there than in the poorer regions. When the species belonging to each center are more accurately listed it may then be possible to discover their significance.

C. Stuart Gager,
Secretary

DISCUSSION AND CORRESPONDENCE

INHERITANCE OF THE BELT IN HAMPSHIRE SWINE

The Hampshires are a breed of black and white hogs, the white being confined to a belt around the body at the shoulders, sometimes occurring also on the hind feet and the tip of the tail. The breed has recently risen to prominence in the middle west. Its origin is somewhat obscure. Hogs of this peculiar coloring are seen occasionally throughout the south, north to the Ohio River, and even beyond. The white belt in hogs the breeding of which is left to chance behaves in a very erratic manner, usually occurring in only a small proportion of any herd. Even amongst registered Hampshires about 10 per cent. of the progeny is without the belt. Breeders have endeavored to eliminate the 'blacks,' *i. e.*, hogs lacking the belt, but are not yet able to avoid them. The writer recently undertook to collect data from breeders with a view to analyzing the belt character, and thus learning the laws governing its inheritance. The effort has apparently been completely successful. The case is one of exceeding interest, and proves to be a complex case of Mendelian inheritance. There is some probability that it is a case of reversion to a very ancient type related to the Indian tapir, which is somewhat similarly colored. At first it was suspected that the belt was due to a simple Mendelian character. According to the testimony of breeders, when two belted hogs produce black progeny there is a distinct tendency for one fourth of the litter to be black. Also, when a belted animal is bred to a black, two cases are evident: first, the litter may all be belted; second, in some cases there is a distinct tendency for half the litter to be belted and half black. Thus far, we seem to be dealing with a simple character pair. But all breeders agree that black bred to black does not always produce black. In fact, litters of this breeding may be all belted.

Professor Castle accounts for the agouti color in guinea pigs by supposing a third independent character which governs the arrangement of the red pigment in the hair, so that the black and red pigments together, under the influence of this third character, produce the agouti color. We can fully explain the peculiarities of the inheritance of the belt character in Hampshire swine by a somewhat similar assumption. In this case, however, this third character, which governs the distribution of white and black over the body, is a compound character of two factors. Representing the two factors by F and G, and the two colors by W and B, the allelomorphic formula of a pure belted individual is FF, GG, WW, BB. The individuals lacking the belt have white fore feet, so that W and B are present throughout the breed. We may, therefore, omit W and B in our formulæ. If either F or G is entirely absent the belt fails to appear. Representing the absence of either F or G by O, the following nine types are possible, and doubtless occur in the breed:

TABLE I.

a. *FFGG*, producing gametes *FG*.
b. *FFGO*, producing gametes *FG*, *FO*.
c. *FOGG*, producing gametes *FG*, *OG*.
d. *FOGO*, producing gametes *FG*, *FO*, *OG*, *OO*.
e. *FFOO*, producing gametes *FO*.
f. *FOOO*, producing gametes *FO*, *OO*.
g. *OOGG*, producing gametes *OG*.
h. *OOGO*, producing gametes *OG*, *OO*.
i. *OOOO*, producing gametes *OO*.

Types *a*, *b*, *c* and *d* are belted; the others are not.

Table II. shows the proportion of 'blacks' in the progeny of all the possible conjugations of these nine types.

TABLE II.

Percentage of Blacks in the Various Conjugations.

Types	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*
a	0	0	0	0	0	0	0	0	0
b		25	0	25	50	50	0	25	50
c			25	25	0	25	50	50	50
d				43+	50	62+	50	62+	75
e					100	100	0	50	100
f						100	50	25	100
g							100	100	100
h								100	100
i									100

Table II. may be divided into three sections, as shown by the division lines. The first section shows the results that follow when belted hogs of all types are bred together. Of the ten possible types of breeding in this division five result in no blacks, four give 25 per cent. blacks, and one 43+ per cent. Breeders have already recognized all these except the last, which occurs in only one out of ten possible cases.

In the second section of the table are found the results from breeding belted hogs on blacks. Of the twenty cases seven give all belts; eight give 50 per cent. blacks; two give 25 per cent. blacks; two 62+ per cent; and one gives 75 per cent. blacks. The results which occur most commonly have both been recognized by breeders, namely, cases giving 100 per cent. belts, and those giving 50 per cent. blacks. The remaining cases occur so seldom that they have not been recognized.

The third section of the table displays the results from breeding black on black. Of the fifteen cases, eleven give blacks only, two give 50 per cent. blacks, one gives 25 per cent.

blacks and one gives 100 per cent. belts! These results are in accord with the experience of breeders, and explain this experience completely.

How shall the breeder proceed to eliminate the undesirable blacks? If a single dominant unit character were to be bred pure, the way to do this would be to discard both parents when black pigs occur. This would rapidly reduce the number of black pigs produced. But here we desire to perpetuate two dominant characters. The desired result will ultimately be secured by discarding both parents of all black pigs farrowed, but very slowly; for heterozygotes are not always revealed by breeding them together. For instance, types *b* and *c* bred together produce only belted pigs. Neither can we test out the heterozygotes by breeding to blacks, for in many cases this type of breeding gives only belts. We can, however, determine whether an individual is homozygous by breeding to a breed which possesses neither of the factors F and G. Only type *a* thus bred will give all belts. But here a difficulty arises. One or the other of these factors is present in at least four other breeds. This is demonstrated by the fact that when Tamworths are crossed on Chester whites, and when Duroc-Jerseys are crossed on Yorkshires, the belt sometimes appears in part or all the progeny. It is not yet known whether Berkshires and Poland Chinas possess either of these factors.

The presence of the factors F and G in four of our standard breeds accounts fully for the occasional occurrence of 'listed' (belted) hogs in swine of mixed breding, a not uncommon occurrence in sections where mixed breeding is common.

The fact that some breeds possess the factor F and others the factor G, unsuspected until revealed by the numerous breed crosses made by Mr. Q. I. Simpson, hints at the possibility that at some remote time the ancestors of our domesticated swine were belted after the manner of the related Indian tapir, and that the belt disappeared by the gradual weakening of factor F in some cases and of factor G in others. In America, where our swine trace to breeds from all parts of the world, these factors have been brought together again, renewing this striking belt character. It is suggested that the Tamworths and Yorkshires probably carry the same factor, these breeds both coming from England. Chester whites and Duroc-Jerseys originated in America, and possibly inherited the other factor from the Chinese hog, which entered into most of our early breeds.

It is entirely possible that the belt character is more complex than here indicated. However, the difference between belted and black Hampshires evidently relates to two factors only.

The distribution of black and white in Berkshires and Poland Chinas is evidently governed in a manner similar to that above assumed for Hampshires, though it is not known how many factors are concerned. These two breeds are black with white 'points.' Until recently Poland Chinas were black and white spotted. The presence of a few animals in the breed having the black color with white points furnished the factor or factors that ultimately gave the breed its present color type.

It is suggested that the arrangement of color on all spotted animals may be governed in some such manner as that here indicated. The presence of several factors having this relation to color distribution, and scattered promiscuously through a spotted breed, would account for observed facts. The tendency for color spots to appear in definite places, as black on the rump in swine having most of the body white, a very persistent tendency, the localization of spots on fox terriers, and the strong tendency for red hairs to appear about the ears of white cattle, all point in this direction.

W. J. Spillman
U. S. Department of Agriculture

WING VEINS OF INSECTS

An adequate reply correcting all the misstatements occurring in the review[1] in Science of February 8, of my article on wing veins would be beyond the limits of space available for this article, and I shall have to

[1] Pp. 219–229.

be content with the general disclaimer, that it is in the main a criticism of views on venation originating with the reviewer and not held by myself.

Probably a combination of the effects of views strongly opposed to those of the author and a very cursory reading of the work reviewed have brought about the confusion resulting in thus attributing to the author much that he has never believed and in giving to the whole presentation a distortion that makes the theory almost unrecognizable.

Had he read more carefully he would not have failed to have seen, for instance, that the three series under which heads winged insects were discussed, were not presented as natural groups (p. 219), nor that the argument favoring the essential identity of tracheated and non-tracheated veins is the conclusion of the argument against the tracheal origin of veins (p. 220), nor have given what I considered as a possible though unwarranted view as being the basis of the system of independents (p. 221), nor the small table at the end of the chapter on Lepidoptera as the objective point of the whole work (p. 222), nor a score of other equally evident blunders.

I desire, however, at this time to criticize the argument offered by the reviewer: "The very presence of tracheæ," he writes, "between the two membranes of the wing when they are fusing sufficiently accounts for the primary location of the veins" (p. 220). The membranes fuse only after the emergence of the insect and the expansion of the wing. The cuticular differentiation of vein and membrane either in the wing of an existing insect or in the organ from which the wing was produced in the ancestor of winged insects, is dependent upon changes in the epithelial cells before the deposition of chitin preceding the final molt, and not at the time of the fusing of the membranes. This is simply the way differences of external structure of any part of the body are brought about, not during the molting process, but usually before or possibly rarely during the process of the cuticle deposition which precedes the molt.

Possibly it may be supposed that he inadvertently used the word 'membranes' but intended to mean the epithelial lining of the wing pad or of the organ which was the precursor of the wing, since in former articles he has described these cell layers as fusing. They usually simply touch, however, and often come into no definite organic union. The vein cavities also are so much larger than the tracheæ that it is very hard to see how their presence could have been any determining factor. It is not as though the tracheæ mechanically held the epithelial layers apart. Furthermore, why should a longitudinal vein require the presence of a trachea to warn off the approaching epithelial cells, but the crossveins from the beginning remain capable of looking out for themselves?

These later suggestions, however, do not represent that author's present point of view, for he is evidently laboring under the delusion that the wing, unlike any other part of the insect's body, expands at the molting time devoid of cuticle and the cells perform this function only after expanding, for he speaks of the 'differentiation of veins from membrane, by the accumulation of cells about the vein cavities, and the stretching out of those that lie between' (p. 221). Now as long as the wing remains in the wing pad the veins occupy a very disproportionately large space and the cells of the membrane are correspondingly crowded—and in no conceivable sense can they be spoken of as stretching except under the assumption that the wings first expand before these cells are exhausted in cuticle production.

This absolute failure to appreciate the facts in the ontogeny of the wing, which must also have been true in whatsoever organ the wing may be supposed to originate, this fundamental misconception is accountable, I believe, for that author clinging so strenuously to the really unsupportable theory of the tracheal origin of veins. C. W. Woodworth

University of California,
Berkeley, March 1, 1907

DELAYING THE BLOSSOMING OF PEACH TREES BY ETHERIZATION

As a means of avoiding late spring frosts, the writer finds that the season of blossoming of peach trees may be delayed by injecting with saturated solutions of ether in water from an inverted bottle supported above. Etherization should be begun just a few days before the buds begin to open. In the writer's experiments blossoming was delayed eleven days. The trees were not injured by the treatment. Ripening was retarded by two or three days, but the fruit was materially increased in size.

Leafy shoots set in bottles of solutions are far more sensitive to the effect of ether than is the leafless tree, one half of one per cent. causing transpiration almost to cease in less than twenty-four hours. Opening flower buds are more sensitive still, being overcome by one tenth of one per cent. of ether.

V. A. Clark

Experiment Station Farm,
Phœnix, Arizona

AN ILLINOIS STATE ACADEMY OF SCIENCES

The time seems ripe for the formation of a State Academy of Sciences in Illinois, which may aid scientific work throughout the state by banding together the leaders in the various departments of science, by stimulating students in the more isolated communities, and by forming a center for all scientific interests.

The demand for such an organization is great and the opportunity promising. It is hoped that every leader and every worker in science in the state will help the cause by becoming charter members.

Will those interested write to the undersigned suggesting the best time for the first meeting.

A. R. Crook

Museum of Natural History,
Springfield, Ill.

THE UNIVERSITY OF MAINE AND THE STATE LEGISLATURE

To the Editor of Science: There appeared in Science for March 29, 1907, the reprint of an article in the *Boston Evening Transcript* regarding the University of Maine, which contained a few misstatements that should be corrected:

First. The committee appointed by the legislature presented a *majority* report favoring the discontinuance of the liberal arts course, not retaining it as stated by the *Transcript*. The *minority* report recommended its retention.

Second. The vote in the senate was 17 to 13 in favor of discontinuing the liberal arts course instead of retaining it as reported by the *Transcript*.

As the house favored retaining the liberal arts course by a vote of 123 to 12 the result is a deadlock between the two bodies and no knowledge of a settlement has reached the writer at this date.

On several occasions during the discussion before the legislative committee the leaders of the opposition to the University of Maine resorted to personal abuse of the president of the university; on one occasion he being accused of being 'a freebooter, with an ignoble desire to be president of a university.' Such a method of procedure by the opposition certainly does not tend to increase the respect of the people of the state of Maine for the opposing institutions and for their representatives. Such political methods should be beneath the dignity of the representatives of Maine's oldest educational institution, and we hope will receive the censure of its alumni.

P. L. R.

We are informed that the senate of the State of Maine passed on March 25, the appropriation for the University of Maine without withdrawing the right to confer the degree of bachelor of arts.—Editor.

THE ASSOCIATED PRESS AND NEWSPAPER SCIENCE

To the Editor of Science: My attention is directed to a communication signed "C. A." in your issue of March 22. I am certainly amazed that so reputable a paper as Science should lend itself to such a statement without the slightest investigation.

The story respecting Matteucci and the Marchette's comet appeared originally in the *London Daily Mail* and was cabled to the *New York Sun* on February 22. It was denied in the *New York Sun* on February 25. The Associated Press never at any time cabled it to this country or anywhere else.

Of course you may have some ulterior purpose for publishing such a falsehood respecting this organization, but I give you the opportunity to make a correction, assuring you at the same time that I should have more respect for you and your paper if before publishing a falsehood of this sort you would make some effort to ascertain the truth.

Sincerely yours,

Melville E. Stone,
General Manager

We regret having published a communication attributing to the Associated Press the story concerning Marchette's comet. We are glad, however, to find that the Associated Press guards so carefully its reputation for accuracy in its scientific news.—Editor.

SPECIAL ARTICLES

THE FIRST SPECIES RULE FOR DETERMINING TYPES OF GENERA—HOW IT WORKS IN ORNITHOLOGY

As a further contribution to the discussion of methods of fixing types of genera in zoology, an exposition of how the first species rule works when applied in ornithology may be of interest to other zoologists.

It is evident to every one familiar with the intricacies of nomenclature that the uniform enforcement of this rule would result in eliminating many generic names that have become, through a long period of nearly universal and unquestioned use, almost household words in the current literature of zoology, or in their transference to wholly new and more or less repellant associations. So frequently would this happen in the case of Linnæan genera that the promoters of the first species rule are obliged to make, as one of their first conditions for its adoption, an exemption clause for Linnæan genera. It can readily be seen that such an exemption clause would work charmingly in the case of North American birds, and many American ornithologists may be persuaded to swallow the sugar-coated pill thus so thoughtfully prepared for them; but it is hardly probable that such action would be followed by ornithologists at large, and quite improbable that it would meet with approval in other departments of zoology. But no way has been suggested for saving many other genera, equally as well established and as universally current.

As an illustration of how the first species rule would work when applied without restriction, a few Linnæan genera may be cited. Of the seventy-five valid Linnæan bird genera, fourteen are fortunately monotypic, and the type of some thirty-four others is by common consent (in nearly all cases by elimination) the first species. This leaves about one third of the total number with the currently accepted type some other than the first, ranging from the second to the thirty-fourth. To take the first species in these cases would create nomenclatural chaos. For example, the type of the genus *Fringilla* would be *Dolichonyx oryzivorus*, the bobolink, a bird of a different family, thus transferring the family name Fringillidæ from the finches to a wholly different group, rendering a new name necessary for the finch family; the type of the genus *Psittacus* would be *Ara macao*, a large long-tailed American species instead of the familiar gray parrot of Africa, and involving also the transference of the family name as well; the type of *Anas* would be *Cygnus cygnus*, a swan instead of a duck; the type of *Scolopax* would be an ibis instead of a snipe; and so on through the list. The same confusion would result in the case of mammals, fishes and reptiles, and doubtless in other classes. As, however, Linnæan genera are tabooed in this connection, non-Linnæan genera will be considered later on in this communication.

In Mr. Stone's second paper on this subject[1] he states that in my reply[2] to his former article[3] I relied 'mainly upon general state-

[1] 'The First Species Rule versus Elimination,' Science, N. S., Vol. XXV., No. 630, pp. 147–151, January 25, 1907.

[2] "The 'Elimination' and 'First Species' Methods of Fixing the Types of Genera," Science, N. S., Vol. XXIV., No. 624, pp. 773–779, December 14, 1906.

[3] "The Relative Merits of the 'Elimination' and 'First Species' Method in Fixing Types of Genera —with Special Reference to Ornithology," Science, N. S., Vol. XXIV., No. 618, pp. 560–565, November 2, 1906.

ments' and did not prove any of his 'facts or figures to be inaccurate.' Such facts and figures are of a kind one is not apt to carry around in one's vest pocket, or to have pigeon-holed ready for immediate use. Mr. Stone had the advantage of six months or more for preparation, and presented what seemed to be —and to many really was—a convincing array of statistics. From my general knowledge of the subject I felt confident that both his statistics and conclusions were misleading. I was so strongly convinced of this that I determined at once to make a thorough examination of the case, primarily for my own satisfaction as to the real truth of the matter. After three months or more of pretty continuous application to the subject, I am glad of the occasion Mr. Stone's second article affords to make public the results.

But first a few words in reference to some of the 'points' he has endeavored to make in his rejoinder.

1. In regard to his attempt to explain away his original statement that "Elimination has never been practised in Europe and does not seem to be understood there," it seems a pretty small loophole of escape, in the face of Dr. Bather's reply[4] to this phase of his paper, to say (see foot-note to p. 148) that he meant 'simply that they did not *interpret* the method in the way Americans have done.' He cites the case of *Passerina* and *Sarcorhamphus* as the 'sort of name shifting' he 'claimed to be not understood abroad'; and says further, that where a first reviser had failed to fix the type on the first species "subsequent authors have frequently ignored them and have selected the first species as the type." This is unfortunately true of Mr. Stone and his first species rule associates, but is not true as a general statement of how things have been done in the past, either in Europe or in America. It would be easy to fill columns of SCIENCE with evidence in disproof of such an assumption.

2. It would take up too much space to reply in detail to the many points wherein he seems to have misunderstood or placed a forced construction upon my statements; yet one or two points may be referred to as an illustration of the hopelessness of attempting to diffuse light where light is not desired. If he is unable to see that I have already shown that the first species method is not always so simple in application as he has claimed, and is able to exclaim with sincerity: "Surely to ascertain the first species mentioned by an author in describing a new genus we have only to look at his original description!" and with the intention of implying that this is all there is to do under any circumstances in determining types by the first species rule, reiteration of evidence already given, and the presentation of other like evidence, seem a useless waste of effort. Fixing types implies the determination of the validity of genera, as this is the whole purpose of the work. It is one thing to look up a genus and see what is its first species, and another thing to determine whether this first species has not already been the first species of some other genus. This was the import of my remarks and illustrations, and they were open to no other construction.

3. Mr. Stone says, on p. 149, that "if the types of two or more genera happen to be the same by elimination the later genera become pure synonyms of the earliest"; which is quite true, but it has no bearing on the point at issue, which is that a later genus must become a synonym of any earlier one that has the same first species, no matter how different may be their constituents as a whole, while by elimination a heterotypic genus can be restricted so that the name, instead of being reduced to synonymy, may be conserved for some part of its original constituency. As an illustration we may take the genera *Limosa* Brisson, 1760, and *Actitis* Illiger, 1811. As originally constituted, *Limosa* contained 8 species, representing 3 modern genera; *Actitis* contained 4 species, representing 4 modern genera. Of these 12 species, only two were common to both genera. The first species was the same in both, namely, *Scolopax limosa* Linn., which by tautonomy is the type of *Limosa,* and by first species rule is also type of *Actitis.* By elimination the type of *Actitis* is *Tringa hypoleucos* Linn., the fourth and

[4] 'Elimination in Fixing Genotypes,' by Dr. F. A. Bather, SCIENCE, N. S., Vol. XXIV., No. 625, pp. 809, 810, December 21, 1906.

last species. *Actitis* is currently recognized as a valid genus, with *Tringa hypoleucos* as type, but Mr. Stone, without hesitation, would relegate the name *Actitis* to synonymy and bring in some other name for the genus commonly known as *Actitis.*

4. Mr. Stone says his "chief objection to the method (*i. e.,* elimination) is that it will give different results in the hands of different workers owing to the almost infinite variety of ways in which it may be applied." In the opening sentence of his second paper, Mr. Stone says that the 'extravagant statements of the probable revolution that would be thus occasioned by the adoption of the first species rule in our nomenclature' are what led to his preparing a 'statement of the matter based on fact and not on theory.' If anything more 'extravagant' than his repeated assertions about the 'diversity of results' from elimination and 'the almost infinite variety of ways' in which it is conducted have found their way into this controversy I have yet to be apprized of them. He proceeds to illustrate this infinity of ways by citing two methods which he assumes to be in current use, one of them with two subdivisions, making in all, we will say, four ways of conducting elimination. There is always a common sense way of doing things and other ways. He says:

(*a*) Some remove only the species which has been made the type of a subsequent genus at the date at which the genus was established.

(*b*) Others remove along with the type any other strictly congeneric species, and here again there are two practises according as we interpret congeneric to mean congeneric from the standpoint of the author of the genus, or congeneric from the standpoint of the eliminator.

I am glad that Mr. Stone has put these several 'methods' on record, for it throws great light upon his possible points of view of elimination, and also goes far toward explaining how his 'facts and figures' were compiled. I may here say, at the outset, that I first became aware that there was any such method as his method '*a*' only some six months ago through correspondence with Mr. Stone, or that any one could take 'congeneric' in this connection from any other standpoint than that of the eliminator! To me both of these propositions are unthinkable, for I do not see how any results—at least, any rational results—can be obtained if "we interpret congeneric to mean congeneric from the standpoint of the author of the genus." The suggestion is on its face an absurdity, as it would permit of no elimination whatever; and we must credit the author of a genus with putting an assemblage of species into a single genus which he knew were only in part congeneric and in part really belonged somewhere else. Of course, an author often states that certain species are referred to a given genus provisionally, or are given as doubtfully belonging to it. In all such cases the rules of our standard codes prohibit the taking of any such doubtfully referred species as the type of a genus.

5. In criticizing my treatment of the genus *Vultur* and the genera into which it became subsequently divided Mr. Stone says: "I fail to see why we have to ascertain the types of the involved genera when we eliminate *Vultur.*" In determining the type of *Vultur,* or of any other heterotypic genus, each of its specific components must be traced to its final generic resting-place. It is thus necessary to determine first the types of all the genera to which species of *Vultur* were successively removed. As the involved genera were also good illustrations of the working of the two methods of determining types, each was taken up in historic sequence, bringing out the fact that the status of neither *Sarcorhamphus* nor *Gypagus* could be determined by looking at the description of the genus to see what was the first species; in other words, that a knowledge of the literature was necessary to get correct results in nomenclature even under the first species rule.

Mr. Stone, in his criticisms, has properly enough taken advantage of a pure blunder on my part in the elimination of *Sarcorhamphus* —an incomprehensible slip which, through haste in preparing the paper for an occasion other than its publication in Science, I overlooked and failed to observe in revising the proof. This warrants his statement that I have really, in this case, "interpreted 'congeneric' to mean congeneric from the standpoint of the original author, and not from that of the eliminator." Nothing, however, was further from my thought or intention, for I do not admit the possibility of such an interpretation of the term 'congeneric' in connection with its use in elimination. It was an 'unconscious' slip, which most of us have now and then to regret. In reality *Sarcorhamphus,* by the method of elimination is a synonym of *Vultur,* as it is by the first species rule. If I had put the case in my usual manner of formulating elimination cases, it could not have happened. It leaves, however, the cases of *Vultur* and all of the other involved genera without change. They may be more clearly restated as follows:

Genus *Vultur*, 1758

6 noncongeneric species, representing 6 modern genera and two modern families, as follows:

1. *gryphus,* type of *Gryphus* Duméril, 1854.
2. *harpyia,* type of *Harpyia* Illiger, 1816.
3. *papa,* type of *Gypagus* Vieillot, 1816.
4. *aura,* type of *Cathartes* Illiger, 1811.
5. *barbatus,* type of *Gypaëtus* Storr, 1784.
6. *percnopterus,* type of *Neophron* Savigny, 1808.

Type, by elimination, *Vultur gryphus* Linn., the last species to become the type of a later genus.

Genus *Sarcorhamphus*, 1806

3 noncongeneric species:

1. *gryphus,* type of *Gryphus* Duméril, 1854.
2. *papa,* type of *Gypagus* Vieillot, 1816.
3. *auricularis,* type of *Otogyps* Gray, 1841.

Type, by elimination, *Vultur gryphus* Linn. *Sarcorhamphus* is thus a synonym of *Vultur.*

Genus *Cathartes*, 1811

2 noncongeneric species:

1. *papa,* type of *Gypagus,* 1816.
2. *aura.*

Type, by elimination, *Vultur aura* Linn.

Genus *Gypagus*, 1816.

2 noncongeneric species:

1. *papa.*
2. *gryphus,* type of *Gryphus* Duméril, 1854.

Type, by elimination, *Vultur papa* Linn.

6. Mr. Stone emphasizes the difficulties of elimination by calling attention to two genera I have overlooked, namely, "*Rhinogryphus,* 1874, and *Torgos,* 1828, which, respectively, antedate *Œnops* and *Otogyps,*" but which, he adds, fortunately do not alter the results of my eliminations. Space for a few words must be taken to place these 'sins of omission' in their true light. As to *Torgos,* he fails to give the author or place of publication. *Torgos* is not in 'Scudder's Nomenclator Zoologicus' (1882–1884), nor in Waterhouse's 'Index Generum Avium' (1889), nor in Richmond's "List of Generic Terms proposed for Birds during the years 1890 to 1900, inclusive, to which are added Names omitted by Waterhouse in his 'Index Generum Avium,'"[5] nor does it appear to have been before cited since its original publication. It is one of the recent discoveries of overlooked names that have rewarded the commendable zeal of some persistent name-hunter who has not yet imparted to the public the latest results of his labors.[6] As to *Rhinogryphus* and *Œnops,* they were both published in the same year, and for the incidental use I made of *Œnops* it did not occur to me to find out which has priority, as neither is at present in current use.

At this point (p. 150), Mr. Stone devotes a paragraph to what *might* have happened '*if*' the dates of certain genera had been earlier than they really were. He raises the hypothetical possibility that "the discovery of two overlooked genera would not only replace two current genera by reason of priority, but would *by elimination* alter the types of three other genera. With the types fixed by the first species rule the only effect of the resurrection of the old names would be their sub-

[5] *Proc. U. S. Nat. Mus.,* XXIV., pp. 663–729, May 2, 1902.

[6] Since writing the above I have discovered by accident the place and manner of publication of *Torgos,* which it seems worth while to make public. It occurs in *Isis* von Oken, Bd. XXI, Heft 11, p. 1143, Nov., 1828, in a paper by Kaup entitled, 'Ueber Hyaena, Uromastix, Corythaeolus, Acontias, von Kaup.' Under the 'Gattung *Hyæna* Cuv.' is the remark: "Diese Gattung repräsentiert die Gattung *Torgos* (*Vultur auricularis*) mihi." So here is *Torgos,* a monotypic genus, with *Vultur auricularis* Daudin as type, in a paper devoted mainly to reptiles, in a journal with a nonalphabetic 'Inhalt' and no index.

stitution for the two current names having the same types." In reply to this it is only necessary to recall that in the case of *Vultur* two overlooked names did not in the least affect my elimination of types. Furthermore, Mr. Stone knows, and I and some others know, that since the publication of the last supplement to the A. O. U. Check-List in 1904, it has been found that more than thirty of the current generic names of North American birds will have to be replaced by others solely on the ground of priority, or will be carried back to other authors and to earlier dates, without affecting the type of any of the genera involved. We can imagine almost anything. But such hypothetical speculations are hardly to be looked for from one who especially deprecates 'extravagant statements,' and relies so emphatically upon 'facts and figures.'

7. In regard to the 'action of revisers,' it must be noted that there are all sorts of revisers, who in times past have revised in all sorts of ways, even to designating as types of genera species not originally contained in them, and even transferring names to groups wholly different from those for which they were originally proposed. This was pretty commonly practised prior to about 1850; yet where genera were restricted and a type properly designated, that is, in conformity to the requirements of modern codes of nomenclature, it is of advantage to accept them, and often a distinct aid in settling complicated cases, like the large genera of early authors. If a reviser selects his type in contravention of generally accepted rules his work is not of course entitled to recognition.

8. In this connection, Mr. Stone refers to the fixing of 'the types of the genera *Cathartes, Sarcorhamphus* and *Gypagus* by Mr. Ridgway in 1874, and independently by Dr. Bowdler Sharpe in the same year,' and adds that it is interesting to note (foot-note, p. 150) that both authors "in each instance selected the *first species* as the type and one would be inclined to suspect that they were following, consciously or unconsciously, the first species rule." But Mr. Stone fails to give us the subsequent history of this piece of work. Ten years later Mr. Ridgway and Dr. Stejneger, to whose excellent work in fixing the types of the North American genera of birds in the A. O. U. Check-List we are so greatly indebted, reversed the work of Mr. Ridgway in 1874, making *aura* the type of *Cathartes* and *papa* the type of *Gypagus*, as they have since stood in Mr. Ridgway's 'Manual of North American Birds,' as well as in the Check-List. Dr. Sharpe in 1902, in his invaluable 'Hand-List of Birds,' reversed his position of 1874, making *aura* the type of *Cathartes* and citing both *Rhinogryphus* and *Œnops as synonyms* of *Cathartes*, giving also generic recognition to *Gypagus* with *papa* as its type. Thus my recent independent determination of the types and the use of these genera is in harmony with current usage by the best authorities on both sides of the Atlantic. If Ridgway and Sharpe determined the types of these genera by the first species rule in 1874, they have done otherwise since. Evidently when Mr. Stone cited this case he really had seen 'too many Vultures' to clearly discern the present nomenclatural conditions of the group—conditions which were evidently not reached by the strict application of the first species rule.

9. Mr. Stone quotes an eminent zoologist as saying that "elimination is absolutely dead and ought not to be revived in any code or thought of in any connection." It is a suggestive 'coincidence' that another zoologist, especially eminent in invertebrate zoology and a recognized authority in several classes of animals, has expressed to me the same sentiments in practically the same language about the first species rule!

10. Great emphasis is placed by Mr. Stone upon the fact that the first species has so often become the type, even where the type has been determined by elimination. I have stated that this has often resulted by 'coincidence' rather than from a conscious reservation of the first species in the process of subdividing polytypic genera. There is abundant evidence that such is the case, but space can not here be taken to cite examples in detail.

In the case of Brisson's genera the type, by tautonomy, is nearly always the first species. In the case of Stephens's numerous bird genera, a species is generally figured to illustrate the genus, and this species nearly always heads his list. Naturally, in subdividing these heterogeneous groups, the illustrated species is taken as the type. In other instances it frequently happens that where an author proposes a genus to which he refers several species, all the species except the first were already the types, or congeneric with the types, of other genera. Sometimes the diagnosis shows that the author based his genus primarily on the first species, and subsequent revisers, in dismembering the group, have had the good sense to restrict the original genus to this species. In the non-monotypic Linnæan genera the currently recognized type in 44 per cent. of these genera is some other than the first species. These facts suffice to show that the type may be quite often the first species by 'coincidence,' or without the conscious application of the first species rule.

11. In place of 'general statements' a few facts and figures may now be presented respecting the comparative number of name changes rendered necessary through the strict application, respectively, of the 'elimination' and the 'first species' rules in bringing the nomenclature of the genera of the Check-List of North American Birds to a proper standard of accuracy. Mr. Stone stated in his first paper[7] that the number of changes would be practically the same under each, namely, fifteen by elimination and sixteen by the first species rule. As said above, I have spent a large part of the last three months in determining the types of the genera and subgenera of the Check-List by both elimination and the first species rule. The Check-List was taken as it was left by the publication of the last 'Supplement' in July, 1904. The changes later found necessary by the Check-List Committee, being as yet not officially published, are not considered. Also, in order to show just how much truth there is in the allegations that no two eliminators ever reach the same results, owing to the 'almost infinite variety of ways' in which elimination may be applied, I have eliminated from the same basis as the original A. O. U. Committee in preparing the first edition of the Check-List, and their successors in preparing the second edition and its subsequent supplements; that is, I have taken the genera at the dates and from the same sources as they took them, even in the few cases where later investigation has shown that they originated earlier and with a different constituency. These later discoveries are considered, however, in making up the statements for comparison with Mr. Stone's statistics, with a view to treating both phases of the subject with perfect fairness.

(*a*) First as to the difference in results reached by different eliminators. I disclaim any knowledge of just *how* Mr. Ridgway and Dr. Stejneger reached their surprisingly accurate results. I followed my own method, strictly and consistently, and did not check up my results with the Check-List till my work was finished. The total number of errors of elimination in the Check-List sufficiently serious to affect generic nomenclature is 3, which result in changing the names of 2 genera and 1 subgenus. One additional change, affecting two genera, is due to the application of the principle of tautonomy, a rule not formulated till many years after the publication of the Check-List, and this change of names is thus not chargeable to the A. O. U. Check-List Committee as an error of elimination. The type in five other genera is transferred from one species to another strictly congeneric with it, in three of the five cases through the application of the rule of tautonomy, but in none of these five cases is a generic name affected. There are thus, all told, five errors of elimination, only three of which affect the names of species.

As bearing on the question of alleged diversity of results through elimination it may be noted that my results not only agree closely with the Check-List, but also in every case with Mr. Ridgway's recent eliminations in 'Birds of North and Middle America' (Vols. I.–III., 1901–1904), and also almost invariably with those of the British Museum 'Catalogue

[7] Science, N. S., Vol. XXIV., p. 562.

of Birds,' so far as the genera in these several works are strictly comparable.

(*b*) As to the changes necessary to correct properly the generic nomenclature of the Check-List. Here it is necessary to take note of recent discoveries of overlooked names, and of names transferred to earlier dates, as this was doubtless Mr. Stone's basis. This is seemingly quite a different question from that of actual errors in the Check-List eliminations. Although there are some thirty of these discoveries, it is surprising to find that not any of them affect the types of genera as determined by elimination.

The changes necessary on the basis of elimination are as follows:

Podiceps (subgenus) becomes nameless.
Cyclorrhynchus becomes *Phaleris.*
Phaleris (subgenus) becomes nameless.
Ceophlœus becomes *Phlœotomus.*

(*c*) As to the results of the strict application of the first species rule. The changes that would surely follow such action are as follows:

Colymbus becomes *Podiceps.*
Podiceps (subgenus) becomes nameless.
Phaleris (subgenus) becomes nameless.
Cyclorrhynchus becomes *Phaleris.*
Dysporus (subgenus) becomes nameless.
Aix becomes nameless.[8]
Erionetta (subgenus) becomes nameless.
Melanitta (subgenus) becomes nameless.
Actitis becomes *Tringoides.*
Bonasa becomes nameless.
Tympanuchus becomes *Bonasa.*
Cathartes becomes *Rhinogryphus.*
Gypagus becomes *Cathartes.*
Conurus becomes *Conuropsis.*
Aphelocoma becomes *Cyanurus.*
Acanthis becomes *Ægiothus.*
Spinus becomes nameless.
Passerina becomes *Plectrophenax.*
Cyanospiza becomes *Passerina.*
Poœcetes becomes *Zonotrichia.*
Zonotrichia becomes nameless.

Summary.—According to Mr. Stone, the Check-List contains 124 composite genera for which no type was designated by the founder, and of which the type has been fixed by 'elimination.'

[8] That is, if *A. sponsa* and *A. galericulata* are considered as noncongeneric, as is done by various late authorities.

The strict application of elimination involves 3 changes of names, of which 2 are generic, affecting the names of 2 species, and 1 is subgeneric, and hence does not affect the names of species. Mr. Stone's estimate was 12 genera and 3 subgenera—an estimate over 500 per cent. greater than the reality.

The strict application of the 'first species rule' involves 16 changes of generic names not otherwise necessary, which affect 33 species and 18 subspecies, and 5 subgeneric names, making 21 changes in all. Mr. Stone's estimate was 16 changes, an under-estimate of nearly 33 per cent.

The ratio of the required changes by elimination is as 1 to 41 by first species rule as 1 to 8, or 5 times as many by the first species rule as by elimination. A number of other genera that would be relegated to synonymy by the first species rule are saved only by the rule of tautonomy.

Seven other lamentable changes in the names of numerously represented genera of American birds come under the spirit of the first species rule and partly under its scope, although urged on the basis of priority, in disregard of a hitherto almost universally recognized principle of nomenclature—the designation of types by the founder of the genus. It has been customary in selecting types to consult the intent of the author, and to accept his types even if only inferentially designated. In 1827, Swainson published two papers on birds, one a list of a collection of birds from Mexico, the other describing many new genera; this more general paper was sent to the *Zoological Journal* for publication long before the other was sent to the *Philosophical Magazine,* which latter, however, was unfortunately published a few months before the other. In the *Zoological Journal* paper were described five new genera represented in the Mexican collection reported upon in the *Philosophical Magazine.* In referring species to these unpublished genera he made a cross-reference to *Zoological Journal,* No. 10, where they were not only described but had their types explicitly designated; and these types had been accepted by all subsequent authors, down to 1904, or for over 75 years, when it was proposed to take the first species associated with the generic name as the type, instead of the type designated by the author a few months later in the same year. In each case the generic name is transferred to a wholly different group, and different names, some of them new, have to be substituted for the groups long known under the displaced names. These names are:

Ammodramus, changed to *Coturniculus*.
Coturniculus, renamed *Ammospiza*.
Euetheia, changed to *Tiaris*.
Tiaris, renamed *Charitospiza*.
Helminthophila, changed to *Vermivora*.
Dendrornis, changed to *Xiphorhynchus*.
Xiphorhynchus, renamed *Xiphornis*.

These changes affect 18 species and 10 subspecies of North American birds, and about 30 species and subspecies of Mexican and South American birds.

The revised A. O. U. Code (as yet unpublished) has a rule to the effect that an author may designate the type of a new genus in any part of the work or paper in which the genus was originally proposed, and (by inference) not elsewhere. But it has happened many times in the past that an author has designated the type of his own genus in some subsequent work, and such designation has been respected as valid. It hence seems desirable to add to the new A. O. U. rule the following provision, namely: *The type of a genus designated by its author in a publication subsequent to the one in which the genus was originally proposed may be taken as its type provided that the species thus designated as type was one of the original species and had not in the meantime been made the type of some other genus.* This would prevent the ruthless overturn of such long established names as those mentioned above.

12. In conclusion, a word on the subject of methods of elimination. I fail to see in elimination but a single principle, the rule of priority. As Dr. Stiles has well said:[9] "If this principle is just when applied to generic names, why is it not equally just when applied to the generic types?" I also fail to see how there can be more than one way of applying the rule, or anything difficult or abstruse in it, beyond a proper knowledge of the literature of the subject. Experts evidently *do* reach the same results; those who try to apply the principle without thoughtful consideration of how to do it naturally meet with trouble. As said before, it is unfortunate that there has been so rarely a definite statement of the process, which should have long since been set forth in the codes of nomenclature for the guidance of the inexperienced. The statement of the method given in an earlier number of SCIENCE (Vol. XXIV., p. 777) covers the whole matter. Where trouble arises it is not from any obscurities of the method but comes from the taxonomic side, the doubts that arise in relation to the validity or value of groups that have been set aside as genera or subgenera. But this would arise equally under any method of determining types.

The convenience of the first species rule is its only asset; every other consideration, as emphasized by Dr. Stiles (*l. c.*), scores against it. It ignores all types hitherto established under any other method, in the case of genera whose types are not determinable by one of the three universally accepted rules. As Dr. Stiles has well said, when a "type is once designated, by any method whatever, so long as the species selected was an original species, valid from the original author's point of view, and unreservedly classified in his genus, why reopen the question?" As a matter of fact, the A. O. U. Committee in preparing the Check-List established the types of such genera of North American birds as had not had types previously properly designated, and why now reopen the cases except for cause? The proposal of a new rule, obviously disastrous to the stability of nomenclature, is certainly not a sufficient cause.

Dr. Stiles makes reference to the rule laid down by Linnæus himself for the determina-

[9] "The 'First Species Rule' vs. the 'Law of Priority' in Determining the Types of Genera," SCIENCE, N. S., Vol. XXV., No. 630, pp. 145–147, January 25, 1907.

tion of the types of his own genera. It is my opinion that Linnæus's rules were followed to a larger extent by his disciples and immediate followers than we are wont to recognize. But the types of Linnæan genera in ornithology are not now a source of trouble. With the exception of two or three, of which *Vultur* is one, they have long been settled in a way to meet general approval. I do not apprehend that Dr. Stiles meant to suggest that any of these cases should be reopened if it is found that they were not settled in accordance with Linnæus's own rule. Indeed, his later remarks (quoted above) seem to preclude such a suggestion.

In stating the results of my determination of types by the two methods, as given above, it is impracticable to show the steps by which they were reached; I shall, however, publish soon elsewhere not only the basis of these results, but a complete list of all the North American genera and subgenera, with their types by both methods, where the results differ; and also showing each step in the process of elimination for all the genera to which elimination is applicable.

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CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

CUMULUS CLOUDS OVER THE SAN FRANCISCO FIRE

Mention was recently made in these notes of an observation recorded in *Nature* of cumulus clouds which formed over the fire succeeding the San Francisco earthquake of last year. Attention was at the time called to the fact that this was the first, and only, mention of such clouds which seems to have found its way into print. The publication of that comment in Science brought to the compiler of the notes a letter from Professor George D. Louderback, of the University of California, with reference to the clouds observed by him on the same occasion. As the matter is of some general interest, Professor Louderback's letter is here inserted, with the permission of the writer.

From your review of Mr. Van Norden's description of a cumulus cloud over San Francisco at the time of the great fire, and your noting that it is the only reference to that phenomenon you had seen, I have concluded that you may be interested in a corroborative observation of mine on the same occasion. I was in Nevada at the time of the earthquake, and as my parents and other near relatives lived in San Francisco I took the first train for that city, but on arriving at the Oakland pier the morning of the nineteenth I found that no one was permitted to cross by the ferries in that direction. I spent several hours on the water front looking for a launch that would take me over, and then had a slow trip across the bay and arrived in San Francisco about five o'clock in the afternoon.

The form of the rising column of smoke impressed me very strongly, and I have made a number of efforts since to find some one who might have taken an expressive picture of it, but so far, without success. The dark smoke rising from a large area of the city rather quickly gathered itself together and rose to a great height as a tall column with a low conical base. At the top it spread out in a practically horizontal layer and drifted slowly to the northwest, in which direction its limit was beyond the range of vision. In fact, the first visible indication that I had of the fire was this drifting smoke cloud beyond the coast mountains seen from the train at Benecia (north of San Francisco), and looking west along the gap of Karquinez Straits.

This horizontal cloud extended a very short distance to the south of the main column—probably not greater than the thickness of the column. Rising above its upper surface and directly over the vertical shaft of smoke was the cumulus cloud, its upper surface forming four or five beautifully regular and pure white domes. Not only was it differentiated from the rest of the visible floating material by its form and position, but distinctly by its color and luster, and I decided that the cumulus cloud was of pure water particles, uncontaminated with the smoke particles that gave their character to the horizontal stratum. I watched it, at intervals, for several hours and noticed but little change. It reminded me very strongly of the photographs I have seen of volcanoes in eruption, and especially suggested the stone pine of Vesuvius. Even the form of the volcanic mountain was presented to the eye in the conical spreading base of the smoke column.

As we approached the city the lowering sun was veiled by the cylinder of smoke, and later by the lower layers, and produced the most striking and weird absorption effects. It became a deep fiery red, sometimes mixed with various shades of brown and added, if possible, to the tension and horror of the scene.

On reaching the city, it was found to be pervaded with a sultry heat even several blocks from the fire. There was no breeze blowing—not a breath of air except the indraught and eddy currents produced by the conflagration. This is an unusual condition for San Francisco and lasted, I am told, through the first two days of the fire. I know that a light breeze from the west or south of west sprang up late in the night of the nineteenth and was just enough to throw the balance in favor of the fire-fighters who stopped the progress of the flames in the 'western addition' and the 'mission' districts.

R. DeC. Ward

THE SHALER MEMORIAL FUND

At the last meeting of the corporation of Harvard University the treasurer presented the following communication specifying the terms governing the Shaler Memorial Fund, the receipt of a part of which was reported at the meeting of January 14, 1907:

More than 760 alumni of Harvard University unite in giving to the president and fellows of Harvard College the sum of thirty thousand five hundred dollars ($30,500) to establish a Shaler Memorial Fund in commemoration of the long services of Professor Nathaniel Southgate Shaler and of the great affection in which he was held by his many students and friends.

The subscribers to this fund have left the designation of its use to the undersigned committee; and the committee, after consideration of various projects, concludes that the memorial object of the fund will be best attained —first, by setting aside a sum with which the corporation shall procure a memorial tablet to be put in the geological section of the university museum, or some other suitable place as may be designated by the corporation; and second, by using the income of the balance of the fund for the benefit of the division of geology, in support of original research and in the publication of the results of research, under the following conditions:

The researches here contemplated are to be undertaken by persons nominated by the committee of the division of geology and appointed by the corporation, whether officers or students of Harvard University or not. The subject and the locality or field of research are to be approved by the division committee, preference being given to studies of an advanced and original character. The sums of money allotted from the income for research are to be determined by the division committee, with the approval of the corporation. The money appropriated for such work from the income of the fund shall be in addition to the salary that would be otherwise paid to the person or persons undertaking it; and any work or journey thus supported in whole or in part shall be carried on under the name 'Shaler Memorial Research' or 'Shaler Memorial Expedition.'

The publications here contemplated are to include the results of original research carried on with the income of the fund, or independently of such aid; but the results must in all cases receive the approval of the division committee as to subject and presentation—though not necessarily as to the conclusions stated—before they are accepted for publication.

All publications thus approved, whether appearing in independent volumes or in some established journal, shall bear the general title, 'Shaler Memorial Series.' The allotment of money for publication shall be determined in the same way as for research.

Beneficiaries under the fund, either as to research or publication, may be invited by the division committee to give one or more public lectures in Cambridge on the results of their studies, under the general title 'Shaler Memorial Lectures,' but no additional payment is to be made for these lectures.

The income of the fund may be allowed to accumulate in case an investigation, expedition, or publication of considerable magnitude is contemplated by the division committee; but it is not desired that such accumulation shall continue beyond a reasonable period of time.

In addition to any future subscriptions that may be added to the fund, such part of the income as shall constitute one per cent. of the principal may be annually added to the principal; but action in this regard is left to the discretion of the corporation.

It is wished that the fund shall be administered in accordance with the conditions indicated above, so long as the objects there stated shall be regarded as desirable by the committee of the division of geology; but if the time should come when such objects are no longer held by them to be desirable, the income may be applied to such other objects as the corporation may determine; providing only that it shall be administered as a memorial of Nathaniel Southgate Shaler.

Robert Winsor,
W. M. Davis,
Edward W. Atkinson.

Whereupon it was voted that the Shaler Memorial Fund be gratefully accepted upon the terms and for the uses stated in the foregoing communication, and that the president and fellows hereby record their satisfaction in the possession of such an enduring and fruitful memorial of Professor Shaler.

THE INLAND WATERWAYS COMMISSION

President Roosevelt has appointed an Inland Waterways Commission whose work will not only be of great importance for public welfare, but is also of considerable scientific interest. The objects of the president in appointing the commission are clearly stated in his letter to those whom he has asked to act upon it. It is as follows:

Numerous commercial organizations of the Mississippi Valley have presented petitions asking that I appoint a commission to prepare and report a comprehensive plan for the improvement and control of the river systems of the United States. I have decided to comply with these requests by appointing an Inland Waterways Commission, and I have asked the following gentlemen to act upon it. I shall be much gratified if you will consent to serve.

Hon. Theo. E. Burton, chairman,
Senator Francis G. Newlands,
Senator William Warner,
Hon. John H. Bankhead,
General Alexander Mackenzie,
Dr. W J McGee,
Mr. F. H. Newell,
Mr. Gifford Pinchot,
Hon. Herbert Knox Smith.

In creating this commission I am influenced by broad considerations of national policy. The control of our navigable waterways lies with the federal government, and carries with it corresponding responsibilities and obligations. The energy of our people has hitherto been largely directed toward industrial development connected with field and forest and with coal and iron, and some of these sources of material and power are already largely depleted; while our inland waterways as a whole have thus far received scant attention. It is becoming clear that our streams should be considered and conserved as great natural resources. Works designed to control our waterways have thus far usually been undertaken for a single purpose, such as the improvement of navigation, the development of power, the irrigation of arid lands, the protection of lowlands from floods, or to supply water for domestic and manufacturing purposes. While the rights of the people to these and similar uses of water must be respected, the time has come for merging local projects and uses of the inland waters in a comprehensive plan designed for the benefit of the entire country. Such a plan should consider and include all the uses to which streams may be put, and should bring together and coordinate the points of view of all users of water. The task involved in the full and orderly development and control of the river systems of the United States is a great one, yet it is certainly not too great for us to approach. The results which it seems to promise are even greater.

It is common knowledge that the railroads of the United States are no longer able to move crops and manufactures rapidly enough to secure the prompt transaction of the business of the nation, and there is small prospect of immediate relief. Representative railroad men point out that the products of the northern interior states have doubled in ten years, while the railroad facilities have increased but one eighth, and it is becoming obvious that no development of the railroads possible in the near future will suffice to keep transportation abreast of production. There appears to be but one remedy—the development of a complementary system of transportation by water. The present congestion affects chiefly the people of the Mississippi Valley, and they demand relief. When the congestion of which they complain is relieved, the whole nation will share the good results.

While rivers are natural resources of the first rank, they are liable to become destructive agencies as well, endangering life and property, and some of our most notable engineering enterprises have grown out of efforts to control them. It was computed by Generals Humphreys and Abbott half a century ago that the Mississippi alone sweeps into its lower reaches and the Gulf 400,000,000 tons of floating sediment each year (about twice the amount of material to be excavated in opening the Panama Canal), besides an enormous but unmeasured amount of earth-salts and soil-matter carried in solution. This vast load not only causes its channels to clog and flood the lowlands of the lower river, but renders the flow capricious and difficult to control. Furthermore, the greater part of the sediment and soil-matter is the most fertile material of the fields and pastures drained by the smaller and larger tributaries. Any plan for utilizing our inland waterways should consider floods and their control by forests and other means; the protection of bottomlands from injury by overflows and uplands from loss by soil-wash; the physics of sediment-charged waters and the physical or other ways of purifying them; the construction of dams and locks, not only to facilitate navigation but to control the character and movement of the waters; and should look to the full use and control of our running waters and the complete artificialization of our waterways for the benefit of our people as a whole.

It is not possible properly to frame so large a plan as this for the control of our rivers without taking account of the orderly development of other natural resources. Therefore, I ask that the Inland Waterways Commission shall consider the relations of the streams to the use of all the great permanent natural resources and their conservation for the making and maintenance of prosperous homes.

Any plan for utilizing our inland waterways, to be feasible, should recognize the means for executing it already in existence, both in the federal departments of War, Interior, Agriculture and Commerce and Labor, and in the states and their subdivisions; and it must not involve unduly burdensome expenditures from the national treasury. The cost will necessarily be large in proportion to the magnitude of the benefits to be conferred, but it will be small in comparison with the $17,000,000,000 of capital now invested in steam railways in the United States—an amount that would have seemed enormous and incredible half a century ago. Yet the investment has been a constant source of profit to the people and without it our industrial progress would have been impossible.

The questions which will come before the Inland Waterways Commission must necessarily relate to every part of the United States and affect every interest within its borders. Its plans should be considered in the light of the widest knowledge of the country and its people, and from the most diverse points of view. Accordingly, when its work is sufficiently advanced, I shall add to the commission certain consulting members, with whom I shall ask that its recommendations shall be fully discussea before they are sumitted to me. The reports of the commission should include both a general statement of the problem and recommendations as to the manner and means of attacking it.

SCIENTIFIC NOTES AND NEWS

THE bodies of Berthelot and his wife were entombed in state in the Panthéon on March 25 in the presence of President Fallières, the cabinet ministers, the diplomatic corps, members of the French Academy, judges, deputies, senators and deputations from learned societies. M. Briand, minister of education made an address. Every school in France was closed as a sign of mourning.

THE portrait-group of Drs. Halsted, Kelly, Osler and Welch, of the Medical School of the Johns Hopkins University, painted by Mr. John S. Sargent, R.A., was unveiled on the evening of January 19, 1907, in McCoy Hall. The painting was hung at the south end of the hall, where the wall had been appropriately draped. The portrait-group was presented to the university by Miss Garrett and accepted on behalf of the trustees by President Remsen. The Sargent portrait of Miss Garrett was hung in the panel to the left. Dr. Welch gave an account of some of the experiences of the sitters, and Mr. Royal Cortissoz, of the *New York Tribune,* spoke of Sargent as an artist.

AT the New York meeting of the American Association for the Advancement of Science, a silver loving cup was presented to Professor W. F. Ganong by Professor G. F. Atkinson on behalf of the former members of the Society of Plant Morphology and Physiology, as a token of appreciation of Professor Ganong's long and efficient services as executive officer of that society.

AT the fourth International Mathematical Congress to be held at Rome from April 6 to 11, 1908, lectures have been arranged by Professors G. Darboux, A. R. Forsyth, D. Hilbert, F. Klein, H. A. Lorentz, G. Mittag Leffler, S. Newcomb, E. Picard and H. Poincaré.

Professor George T. Ladd, who recently retired from the active duties of the chair of philosophy at Yale University, has gone from Japan to Korea, at the invitation of Marquis Ito, in the interest of the educational development of the country.

At the close of the year Dr. George Frederick Wright, professor of the harmony of science and revelation at Oberlin, will become professor emeritus, retiring under the provision of the Carnegie Foundation.

The Prussian ministry of education has appointed Professor Felix Adler as Theodore Roosevelt professor in the University of Berlin for the year 1908–09, upon the nomination of the trustees of Columbia University, where he holds the chair of political and social ethics.

Dr. C. S. Sherrington, professor of physiology at Liverpool University, has been elected a foreign corresponding member of the Belgian Royal Academy of Medicine.

Dr. P. Chalmers Mitchell, secretary of the London Zoological Society, and Mrs. Mitchell arrived in New York on March 30.

Mr. W. T. Horne has been appointed head of the department of plant pathology in the Central Agricultural Station of Cuba.

Mr. W. H. Power, C.B., F.R.S., medical inspector of the Local Government Board, has been appointed chairman of the Royal Commission on Tuberculosis, in succession to the late Sir Michael Foster.

Dr. W. A. Kellerman, of the Ohio State University, and students, Condit and Imaly, who accompanied him, have just returned from Guatemala where the winter was spent in botanical exploration. He reports excellent success in getting interesting, rare and new plants, especially of parasitic fungi. Moreover, he has planned a regular 'Peripatetic School of Tropical Botany' for the successive winters beginning at the next holidays. This has been submitted to the State University authorities for approval. A very limited number of students would be accepted.

Professor R. Meldola, F.R.S., gave the presidential address before the Chemical Society, London, on March 22, his subject being 'The position and prospects of chemical research in Great Britain.' The anniversary dinner was held in the evening, when addresses were made by Lord Rayleigh, Sir William Ramsay, Professor Ray Lankester and Lord Kelvin.

The Institution of Civil Engineers held its annual dinner in London on March 13, with Sir Alexander B. W. Kennedy in the chair. Addresses were made by Lord Kelvin, Sir Arthur Rücker and Lord Tweedmouth.

At the annual meeting of the Michigan Academy of Sciences, held at Ann Arbor during the past week, Professor William H. Hobbs delivered by invitation the public address, his subject being 'Earthquakes viewed in a New Light.'

Professor Alfred C. Lawson, of the University of California, chairman of the California earthquake commission, gave an illustrated lecture on 'The California Earthquake' before the Geographical Society of Philadelphia on April 3.

Professor Gilman A. Drew, of the University of Maine, lectured before the Portland Society of Natural History on March 18, on 'Animal Adaptation.'

On April 2, Professor John W. Harshberger, of the University of Pennsylvania, delivered a free public lecture on 'The Scientific Application of Ecology in the Wet and Dry Cultivation of Plants,' illustrated by lantern slides and specimens, in the lecture hall of the Academy of Natural Sciences.

The Friday evening meetings of the Royal Institution will be resumed on April 12, at 9 p.m., when Professor A. H. Church will give a discourse on 'Conservation of Historic Buildings and Frescoes.' Succeeding discourses will probably be given by Professor C. E. Sherrington, James Swinburne, Esq., Sir James Crichton-Browne, Signor Come, Giacomo Boni, Professor G. Chrystal (assisted by E. W. Wedderburn, Esq.), Professor F. A. Fleming, A. H. Savage Landor, Esq., Professor Sir James Dewar and others. To these meetings members and their friends only are admitted.

Professor Ernst von Bergmann, the eminent German surgeon, died on March 25, after an operation for appendicitis. The seventieth birthday of Professor von Bergmann was widely celebrated on December 16 of last year.

The *Washington Evening Star* states that at a meeting of the Washington Society of Engineers at the Hubbard Memorial Hall on March 26 it was announced that a plan was under way for the erection in Washington of a permanent home for the various scientific societies of the national capital. A letter was read from the Washington Academy of Sciences, which, through its building committee, invited the cooperation of the Washington Society of Engineers and like associations. The society by a unanimous vote decided to become a party to the movement, and announced that it would pay a yearly rental of $800 for a portion of the building. It was stated that an endowment fund of $100,000 is necessary for the maintenance of the building, which sum, it is expected, will be subscribed by the various societies interested.

The Goldsmiths Company has given $50,000 to be used at the Rothamsted Experiment Station for investigations on soils.

The library of Oberlin College has recently received a valuable addition through the bequest of the late Professor Albert Allen Wright of his zoological and geological books.

The Loubat prizes, awarded every five years by Columbia University, will be given for all works which have appeared between January 1, 1903, and January 1, 1908, which treat of the history, geography or numismatics of North America prior to 1776. The value of the first prize is not less than $1,000, and that of the second prize not less than $400, and the work of all persons, whether citizens of the United States or of any other country, will be considered.

The Harvard Officers' Fund Association held its annual meeting on March 15. The objects of the association are indicated by the following extracts from the articles adopted in 1894: "This association shall be known as the Harvard Officers' Fund Association. Its members shall be limited to those persons whose names are entered as officers of government or instruction in the annual catalogue of the university at the time their first subscription is made. The minimum amount of the annual subscription of each member shall be $5. The funds of the association, so far as they may be derived from annual subscriptions or from interest thereon, shall be used by the trustees at their discretion in providing relief, ordinarily of a temporary nature, for the families of any deceased officers or for any officers distressed by illness or by other calamity."

The Geographical Journal states that Dr. Rudolf Pöch, who, as assistant physician to the Austrian Plague Expedition in 1897 and 1902, made a name for himself by his malaria researches in West Africa, has in 1904–6, with the aid of the Imperial Academy of Sciences in Vienna, prosecuted anthropological journeys in New Guinea, and has also with like purpose visited New South Wales, the Solomon Islands and Bismarck archipelago. In these two years he has traveled along three quarters of the coast of the island of New Guinea. At five spots he stopped for some length of time, and thence wandered into regions of the interior, still in part wholly unknown. The material brought home with him includes 300 measurements of living persons, 15 skeletons, 80 skulls, many anatomical preparations, 1,500 photographs, more than 3,000 feet of cinematograph films (taken by bioscopic camera), representing dances and scenes of village life. Included in the collection are also 90 plates for the phonographic archives of the academy, with a view to the study of the language, songs and music of the natives, and 2,000 ethnological objects. Itineraries of the hitherto unknown regions were kept and altitudes noted in them.

We learn from *Nature* that Lieutenant Boyd Alexander, who, with his brother Captain Claud Alexander, Captain G. B. Gosling, Mr. P. A. Talbot (surveyor), and a Portuguese collector, left England in the spring of 1904 on an exploring expedition across Africa, has returned to London. Captain Claud Alexander died at Naifoni in November, 1904, and Captain Gosling in the Ubangi-Welle region of June, 1906. Much valuable work has been accomplished. A careful triangulation has been carried out from Ibi, in Nigeria, to Lake Chad, and the lake itself traversed in various directions. Part of the course of the Shari was explored; from thence the Ubangi was reached, and the expedition made its way northward to the little-known region where many of the Bahr-el-Ghazal tributaries rise, and down the Yei to the Nile. The expedition has been particularly successful in collecting specimens in natural history, including skulls, bones and skins of the okapi.

The visiting committee of the division of geology of Harvard University has provided the geological department with the funds necessary to erect a seismograph in the University Museum. A Bash-Omori seismograph with two 100-kilogram conical pendulums, one swung in the meridian and the other east and west, will shortly be placed on a suitable foundation in the basement of the geological section. This type of instrument records earth vibrations on smoked paper carried on revolving drums operated by clock-work. One of the same general type, which has been set up in the State Museum at Albany, N. Y., for more than a year, on a clay foundation like that underlying the Harvard Station, gave complete records of the San Francisco, Valparaiso and the great Indian earthquakes. The Harvard Station will pay particular attention to New England earthquakes and to the geological examination of the recent fault-lines along which it is suspected many historically recorded small shocks have arisen.

UNIVERSITY AND EDUCATIONAL NEWS

At a meeting of the General Education Board, held in New York City on March 26, the sum of $625,000 was conditionally appropriated as follows:

Bowdoin College, Brunswick, Me., $50,000 toward $250,000.

Colorado College, Colorado Springs, Col., $50,000 toward $500,000.

Millsaps College, Jackson, Miss., $25,000 toward $100,000.

Yale University, New Haven, Conn., $300,000 toward $2,000,000.

Princeton University, Princeton, N. J., $200,000 toward $2,000,000.

It is reported that the suggestion to send 500 or 1,000 American teachers next year to study the educational system of Great Britain has met with such a response that it is practically certain now the pilgrimage will be made. It is understood that Mr. Alfred Moseley will make the same arrangements for the trip to England and return as he did for the British teachers—a rate of $25 for the round trip. Dr. Nicholas Murray Butler, president of Columbia University, and City Superintendent Maxwell, of the New York city schools, are the American committee.

Barnett Smith, Ph.D. (Pennsylvania), has been appointed assistant professor in the geological department of Syracuse University.

Miss Ida Whiteside, B.A. (Vassar, 1904), has been appointed assistant in astronomy in Wellesley College.

In the faculty of medicine of McGill University, Drs. F. G. Finley, H. A. Lafleur, and C. F. Martin have been made professors of medicine and clinical medicine. These promotions follow the death of Dr. James Stewart, who was for many years head of the department.

Lord Curzon was elected chancellor of Oxford University on March 14. The votes were: Lord Curzon, 1,101; Lord Rosebery, 440.

The council of the University of Liverpool has elected Mr. John Edmond Salvin-Moore to the chair of experimental and pathological cytology, recently established by the Liverpool Cancer Research Committee.

Mr. C. G. Hewitt, has been appointed to the newly-established lectureship of economic zoology in the University of Manchester, and Mr. H. Heap has been appointed assistant lecturer in sanitary chemistry.

The King of Spain has created a chair of automobilism at the School of Arts and Sciences at Madrid.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, APRIL 12, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA

I

THE eighth annual meeting was held December 27 to 29, 1906, at Columbia University, New York. About sixty-five members attended and thirty-two papers were presented.

President Pickering, on taking the chair, discussed three lines of work which he believed the society should pursue. First, by cooperation to carry out some great routine investigation too extensive to be undertaken by a single observatory. The best example of this was the accurate determination of the positions of the northern stars, by European and American observatories, under the direction of the Astronomische Gesellschaft. Second, to bring together socially astronomers from all parts of the country, especially the older and younger men. The latter may think the work of the older men out of date, but they may find the experience of the older men and their personal acquaintance with the eminent men of still earlier date of great assistance. The older men have much to learn regarding new methods, and the extensive appliances at their command may often be employed to much greater advantage if they keep themselves personally in touch with the most recent developments of astronomical research. Third, the presentation of papers. While hitherto this has been the principal function of this and other societies it is not necessarily the most valuable. General discussions are more interesting and instructive than long technical papers. It may, therefore, be wise to follow the example of some of the engineering societies, and print abstracts of papers for distribution some days before the meeting. A brief statement is made by the author of each paper, and the greater portion of the time is devoted to discussion. The ideal conditions for meetings of the society would seem to be—a large hotel where all would eat and sleep under the same roof, and where the meetings could be held in the same building.

On the afternoon of December 28, a general discussion took place regarding neglected fields of work in astronomy, in which a large number of members took part, and the views expressed were varied and interesting. The president, in opening the discussion, cited a number of examples of fields of work, which seemed to him important but neglected. For example, in the astronomy of position, the formation of a standard catalogue of stars uniformly distributed, having similar spectra, and of nearly the same magnitude. Many troublesome sources of error, like those due to magnitude and color, would thus be eliminated. The variation in latitude should be studied at a series of southern stations like those now in operation in the northern hemisphere. The systematic search for double stars of the ninth magnitude and brighter, undertaken at the Lick Observatory, should be extended to the south pole. Photometric measures of faint stars, of comparison stars for faint variables, of the components of clusters, and of nebulæ are much needed. It is not known whether the spectra of nine tenths of the nebulæ are gaseous or continuous. A wide field is opened in the study of the spectra of bright variables when faint, and of faint variables when bright, of the distribution of faint spectra and of the components of clusters.

The election resulted as follows:

President—Edward C. Pickering.
First Vice-president—George E. Hale.
Second Vice-president—William W. Campbell.
Secretary—George C. Comstock.
Treasurer—C. L. Doolittle.
Councilors for 1907–9—Ormond Stone and W. S. Eichelberger.

The council designated Harold Jacoby to act as editor for 1907–8.

We give below a list of papers presented at the society's sessions, together with brief abstracts furnished by the authors. Some of these have been slightly condensed by the editor.

PAPERS PRESENTED

Distribution of Double Stars in the Zone $+56°$ to $+90°$: R. G. Aitken.

The survey of the sky undertaken at the Lick Observatory to secure data for a statistical study of the number and distribution of double stars is well advanced, but the only large zone completely examined is the one from $+56°$ declination to the north pole. This region was divided into eight smaller zones, four of which were examined by Professor Hussey and four by the present writer. By counts of the stars on the charts, it appears that 12,299 stars 9.0 or brighter were examined in the region $+60°$ to $+90°$. Of these, 294 were known double stars and 259 more were found to be double, a total of 553 pairs, all under $5''$ except seven bright stars, giving a ratio of one double star to every $22\frac{1}{4}$ stars to 9.0 magnitude.

Tabulated by hours of right ascension and by zones $4°$ wide, it is found that the distribution curve of these pairs closely resembles that of the stars to 9.0 magnitude in the same area, but that the double stars are *relatively more numerous in the richer sky areas.*

When only the pairs under 2″, or those under 1″, or the very close pairs, or the brighter pairs, are considered, the same relation is found; as may be seen from the following table which groups the results by quadrants:

Table

Zone + 60° to + 90°

Double Stars	0 h. to 6 h. No. Ratio	6 h. to 12 h. No. Ratio	12 h. to 18 h. No. Ratio	18 h. to 24 h. No. Ratio
Under 5″	173 1:19.4	87 1:28.6	86 1:30.2	207 1:18.7
Under 2	113 1:30	56 1:44	66 1:39	132 1:29
Under 1	76 1:44	37 1:67	40 1:65	80 1:48
Under $\frac{1}{3}$	15 1:224	9 1:276	11 1:236	24 1:161
7.5 Mag. or brighter	49 1:68	23 1:108	18 1:144	63 1:61
Zone + 56° to + 60°				
Under 5″	98 1:14.4	35 1:20.4	30 1:22.4	81 1:18.0

The zones north of 60° were examined with the 12-inch telescope, but for fully one half of the zone + 56° to + 60° the 36-inch was used. In this zone 4,257 stars 9.0 magnitude or brighter were examined and 130 new double stars were added to the 114 previously known pairs, giving a ratio of one double star to 17.4 stars to 9.0 magnitude. The table shows that the relations established in the area north of 60° hold true here also, though the ratio in the fourth quadrant is affected by the fact that in this quadrant the work was all done with the 12-inch telescope. It is, therefore, safe to state that in this entire sky area the double stars are relatively as well as absolutely most numerous in the region richest in stars to 9.0 magnitude, that is, in the region of the Milky Way. The study of the local irregularities in the distribution, of which the charts give evidence, is reserved for a later paper.

The Variable RS Persei: Ida Whiteside.

Three minima of this star observed this fall indicated a period of a little over thirty-one days, through about two thirds of which the star remained constant or very nearly so. This period also agreed very well with some scattered observations published in the *Astronomische Nachrichten,* and with others made at the Vassar College observatory. At the time predicted for the fourth minimum the star failed to respond, going down very little if at all. It is probably irregular, but more observations are needed to confirm this statement. The range is only about a magnitude, from 8.2 to 9.2.

A Systematic Error in Distance Measures of Close Double Stars: W. H. Pickering.

In 1900 a series of measures was made of the wide double star μ Draconis. Its components are nearly equal, and their distance is 2″.3. The observations were executed with a 5-inch telescope whose aperture could be reduced by a series of diaphragms to 0.5 inch. A magnification of 300 was usually employed. It was found when the aperture was so small that the disks of the two components were in contact, or nearly so, that the distance between their centers was materially reduced. With an aperture of 0.6-inch the two centers coincided. Had they remained at their true distance apart, their combined image would have been appreciably elongated, instead of which it was perfectly circular.

It occurred to the writer to determine if the same effect was produced in the case of close double stars where large apertures were employed. A selection of suitable cases was made from Lewis's recent publication of measures of the Struve stars. It was found that the apertures employed by the various observers could be divided into three classes, those of about 8 inches, those of 15 inches and those of about 30 inches.

When the separation was so slight that the star disks would have appeared in contact, or nearly so, with the smaller telescopes, it was found that the measures made with them were uniformly about ten per cent. too small. When the images were well separated the large and small telescopes gave identical results.

The early measures made on μ Draconis with the very small apertures indicate that the deviation is a real rather than a subjective one, and that the star images actually approach one another owing to some diffraction phenomenon. It would appear, therefore, that future catalogues of close double stars should always give the reader an opportunity of knowing the aperture employed with every observation. A more detailed account of this investigation will shortly appear in the *Harvard Annals*, Vol. 61.

The Tenth Satellite of Saturn: W. H. PICKERING.

The elements of the orbit of the tenth satellite of Saturn, recently published (*Harvard Annals*, Vol. 53), were based on negatives taken at Arequipa in 1904. An examination of another set of negatives taken in 1900 has just been completed. The satellite was found upon them also, but some of the elements of its orbit differ materially from those found in 1904. This is particularly true of the eccentricity and the inclination. The orbit of 1904 was extremely eccentric, and it was noted in the publication above mentioned that in one part the satellite would pass extremely near to Titan. It was further suggested that the peculiarities of the orbit might be due to such a recent encounter.

The orbit of 1900 was found to be nearly circular, at times passing near Hyperion, however. It was not possible to tell from the negatives just when or how the elements had been transformed from those of one orbit to those of the other, if such a transformation had indeed taken place, but it seemed at least as probable as that two satellites should exist with practically the same period. The orbit is still under investigation, and later results will be published in the *Harvard Annals*, Vol. 61.

Photographic Color Photometry of Short Period Variable Stars: J. A. PARKHURST and F. C. JORDAN.

The broad problem of astronomical color photometry finds an interesting application in the determination of color changes in short period variables of the δ *Cephei* type. Various observers, by indirect methods, have found that these variables show stronger color at minimum than at maximum. As the spectroscope shows these stars to be binaries, a proof of color change coincident with light-variation would be of the highest importance. A direct determination of such changes is made possible by a suitable combination of color-filter, orthochromatic plate and reflecting telescope. An exposure on an ordinary plate (Seed 27) was followed immediately by one with a filter and Cramer 'Trichromatic' plate, which give visual magnitudes (as described by R. J. Wallace in the *Astrophysical Journal* for November, 1906). The two principal advantages of the method are: (1) the simultaneous determination of visual and photographic magnitudes; (2) elimination of personal equation with its physiological complications, from the visual magnitudes of the colored stars, and the substitution of the peculiarities of the instrumental outfit, which can be definitely stated and exactly reproduced.

Briefly stated, the results confirm the color-changes in the short period variables under observation, the photographic range being greater than the visual; in other words, the color-factor (photographic *minus* visual magnitude) is greater at minimum than at maximum. Three stars will illustrate the results obtained.

X Cygni

	Visual	Photographic	Color Factor
	M	M	M
Max.	6.28	7.25	0.97
Min.	7.15	8.75	1.60
Range	0.87	1.50	0.63
T Vulpeculæ			
Max.	5.50	6.05	0.55
Min.	6.10	6.90	0.80
Range	0.60	0.85	0.25
S Sagittæ			
Max.	5.33	5.85	0.52
Min.	5.95	6.93	0.98
Range	0.62	1.08	0.46

We have then the following results:

1. Binary systems, showing orbital revolution.

2. Colored stars, pointing to atmospheric absorption.

3. A regular variation in that color (*i. e.*, that absorption).

Standard Photographic Magnitudes: Edward C. Pickering. (By title.)

On the Probable Distance of Orion: Henry Norris Russell.

Almost all the bright stars in Orion are similar in spectrum and proper motion, and are probably at roughly equal distances from us. Their very small proper motions indicate that their parallax is too small to be directly measured, and Gill's observations of β Orionis confirm this view.

But many of these stars are double, and some appear to be physical systems, showing more or less motion—always very slow. It is possible to derive a relation between the parallax and mass of such systems and the observed distance and rate of motion, which, though not exact in any one case, give good mean values when applied to several stars. Tests of this relation on forty well-known binaries show that its probable error for a single case is about 20 per cent. Applying it to nineteen pairs which appear to belong to the Orion group, and to be physical systems, we find for their mean parallax $\pi = 0''.011/\sqrt[3]{m}$, where m is their average mass, and π is subject to considerable uncertainty owing to errors of observation.

There are many spectroscopic binaries in Orion, but orbits have been published for only two. These indicate that the average mass of these stars is about ten times that of the sun (subject to revision when more data are available). This leads to a mean parallax of $0''.005$ or a distance of 600 light years.

For reasons already mentioned, this should be regarded only as an indication of the order of magnitude of the true distance. Errors of observation, and also errors of judgment in including as physical systems pairs which may ultimately turn out to be optical, would both tend to make the calculated parallax too great.

The Number and Distribution of Stellar Clusters and Nebulæ: S. I. Bailey.

The visible universe beyond our solar system is made up of two classes of objects, stars and nebulæ. All star-clusters may be divided into two classes, irregular and globular; the Pleiades serve to represent the former, the cluster in Hercules, the latter. The term nebula may be applied to any nebulous object, whatever its real nature, which can not be resolved into stars. All nebulæ may be divided conveniently into two classes, gaseous and white, the former giving a spectrum of bright lines, the latter an apparently continuous spectrum. The best known example of the former is the great nebula in Orion, of the latter, the great spiral in Andromeda.

Somewhat less than 700 resolvable star

clusters are known in the whole sky. Less than 600 of these are irregular; nearly all are in or near the Milky Way, of which they form an intimate part, as has long been known. More than 100 resolvable globular clusters are known, the distribution of which bears no relation whatever to the Milky Way, although a rich group of such clusters is found in the Milky Way. This appears to be an accident, however, since they do not seek the lines of greatest luminosity. This peculiar distribution of the globular clusters does not seem to have been pointed out before. Discovery of irregular clusters has been practically exhausted by small telescopes, since powerful photographic instruments reveal few new ones. It is probable, however, that the number of known globular clusters will be considerably increased among the so-called nebulous stars.

About 12,000 nebulæ are now known. The whole number in the sky, however, is several times, perhaps many times, greater. The white nebulæ are distributed in cloud-like groups over the sky, avoiding, in general, the Milky Way. The gaseous nebulæ occur probably over the whole sky, perhaps in greater numbers in the Milky Way where the most striking objects of this class are found. The gaseous nebulæ are probably few in number compared with the white nebulæ.

The majority of all nebulæ are spiral, but there are large numbers of nebulous stars, many of which may be faint globular clusters. The relation between spiral nebulæ and globular clusters may prove to be more intimate than is now apparent.

The Nebulous Regions of the Milky Way: E. E. Barnard.

This paper deals with some peculiarities of the nebulosities in the Milky Way. Though in some cases there seems to be every evidence that the stars and nebulosities are freely mixed together, still peculiarities of distribution, etc., show that with few exceptions (such as ρ *Ophiuchi*) the nebulosity does not indicate any tendency to condense about the stars. This is especially striking in the case of the great North America Nebula (so named by Dr. Max Wolf), where to all appearances the stars seem to exist in a vast bed of nebulous matter with no condensation about them individually. In this particular case the identity of the details of the nebula and the groupings of the stars seem to be conclusive proof that the nebula and the stars, apparently in it, really occupy the same part of space. Even in the case of that remarkable combination of nebulosity and stars, *M 8*, the stars seem freely mixed with the nebulosity, but in no case to be centers of condensation. Contrary to this condition, the great nebula of ρ *Ophiuchi* has condensation about the principal stars in that region, including σ *Scorpii.*

A photograph shows the singular nebulosities about γ *Cygni,* where a profusion of curved sheets and wreaths of nebulous matter cover a large part of the sky about the star and extend as far south as χ *Cygni.* A photograph of the nebula about ν *Scorpii* seems to show quite distinctly an obscuration of the light of the stars in that region. Where this remarkable object veils the sky, the stars seem to have decreased in magnitude. It gives the impression that the brighter stars in this region are dimmed and the fainter ones blotted out, as if produced by a thin obscuring veil of nebulosity this side of the small stars. This peculiarity is very strikingly brought out by the photograph.

In *A. N.* No. 3,111, Bd. 130, the writer has given an account, with a sketch, of a group of small nebulous stars

$$(1860 \quad \alpha = 18^h 2^m \pm \quad \delta = 23°52' \pm),$$

which lies about $1\frac{1}{2}°$ following *M 8.* When these nebulous stars were examined with the 12-inch of the Lick Observatory in 1892, no connecting nebulosity could be seen. It was noticed, however, in a photograph made with the Willard lens that a nebulous wisp ran toward the group from *M 8*. The photographs made by the writer with the ten-inch Bruce telescope of the Yerkes Observatory at Mt. Wilson in the summer of 1905 show that in this group the nebulous stars are simply condensations in a large irregular mass of nebulosity which extends to and connects with *M 8*.

Apex of the Solar Motion: George C. Comstock.

It is well known that different astronomers have reached discordant results in seeking to determine the direction of the sun's motion through space. It is the purpose of this paper to present graphically (lantern projection) these several results classified with respect to the average brightness of the stars employed in the determination. When thus classified all determinations indicate a solar motion toward the north edge of the Milky Way, and the discordances in galactic latitude are not great. In galactic longitude, however, there is a well-marked dependence of the position of the apex upon the mean magnitude of the stars with reference to which it is determined. Stars of the tenth magnitude place this apex some 40° farther east than do stars of the third and fourth magnitudes, while intermediate magnitudes furnish intermediate positions. This variation is attributed to a small drift of the nearer stars in the plane of the galaxy and toward the constellation Cassiopeia. This is shared by most of the sun's stellar neighbors and affects the bright stars in larger measure than the faint ones because a larger percentage of the bright stars are included in this drift.

A Comparison Study of Bright Clusters and Nebulæ: S. I. Bailey.

Field Experience with Transit Micrometers: John F. Hayford.

Spectrographic Observations of Stars: Edwin B. Frost.

Nine spectroscopic binaries were recently discovered with the Bruce spectrograph of the Yerkes Observatory, viz., *RZ Cassiopeiæ, X Cygni,* 13 *Ceti,* ω *Leonis,* 85 *Pegasi,* τ^5 *Eridani,* τ^8 *Eridani,* τ *Orionis,* ξ^1 *Canis Majoris.* (To be published in the *Astrophysical Journal.*)

The Central Eclipse of 1912, April 13: David Todd and R. H. Baker.

Although the land-totality of this eclipse will be visible for a few seconds only, it is desirable to prepare for observation. The central line extends from Oporto, Portugal, to Oviedo, Spain, the line of subsequent annularity passing close to Paris. These points are given by a definitive calculation of the track, made possible by the courtesy of Professor W. S. Harshman, U. S. Navy, Director of the Nautical Almanac, who forwarded in advance the necessary solar and lunar data which will be embodied in the 'American Ephemeris' of 1912. The change from total phase to annular takes place in the Bay of Biscay, according to the accepted diameters of sun and moon; and it will be well to repeat the computation with slightly varying values. The application of photographic apparatus, operated automatically, may make it possible to obtain a useful and permanent record of this brief totality.

Relation between Stellar Spectral Types and the Intensities of Certain Lines in the Spectra: Sebastian Albrecht.

In connection with the measurement of spectrograms obtained at the Mills Observatory in Chili by Professor Wright, an investigation of the individual spectrum lines was begun, with a view to determining whether there is a shift of any lines which is progressive from spectral type to type. Several lines were found to undergo such a progressive change, as is indicated by the radial velocities obtained from them. An examination of Rowland's tables shows that in most cases, lines apparently single are in reality blends of two or more close components. The nature of the variations is such as to indicate varying intensities of the same components rather than the presence or absence of different components in the different types. The investigation was limited to types *F* to *Mb* inclusive on the Harvard classification. In this classification the sun is of type *G*.

A comparison with Adams's list of sun-spot lines gives strong indications that the physical conditions in the stars as we pass from the *F* to the *Mb* type vary in the same direction as from the sun to the sun-spots.

It was thought possible that for variable stars of large light changes, traces of velocity variations for some of the lines might be found, corresponding to small changes in spectral type as the stars varied from maxima to minima—and *vice versa.* In the case of *o Ceti* actual changes in the character of the spectrum are well established, though up to the present no appreciable changes in the wave-lengths of any of its spectrum lines have been observed—leaving out of account the large displacements of the bright hydrogen lines. A comparison of the available measures of η *Aquilæ,* a variable star of the fourth class with a range of only 0.8 of a magnitude in light variations, showed evidences of variations in the positions of some lines from light maximum to minimum similar to the variations that were found from type to type. A further study of this variable star is desirable to establish definitely the exact character and amount of these variations.

These variations in the wave-lengths of some of the lines depending on spectral type, will make necessary the exercise of great care in the selection of lines in radial velocity determinations, and a proper allowance for the type.

A Device of eliminating Guiding Error from Photographic Determinations of Stellar Parallax: Frank Schlesinger.

Experience has shown that a fruitful source of error in photographic work is what has been aptly termed the 'guiding error.' This arises from the fact that when the guiding is imperfect, the image of each star wanders from its mean position and these excursions are registered upon the photographic film to a different extent for stars of different brightness. This error is particularly troublesome in parallax work where the star under examination is usually much brighter than the comparison stars. After considerable experimenting the writer adopted the following simple device that seems to overcome the difficulty in a very satisfactory way. A rotating disc was mounted a little below the center of the photographic plate and about ten millimeters in front of it; that is, on the side toward the objective. The disc was made in two halves moving on the same axis, so that the opening left between their edges could be adjusted to any desired angle. The image of the bright star having been brought to the center of the plate and the disc set into motion, a part of the star's light will be occulted in each rotation of the disc. The amount of light that reaches the plate depends only upon the angular opening between the two halves of the slit and neither upon the rate at which they are revolving nor the distance of the image from the center of rotation. The images obtained in this way were free from any trace of diffraction caused by the edges of the disc-opening and were otherwise indistinguishable from images of fainter stars taken without the occulter. In parallax work the opening of the disc should be so adjusted that the apparent magnitude of the star under examination is about equal to the mean of the magnitudes of the comparison stars. The effect of guiding error will, therefore, almost entirely disappear. Actual measures upon plates taken with this device fully confirm this last statement.

Photographic Observations of Giacobini's Comet: E. E. BARNARD.

These photographs show the almost sudden development of the tail between December 25 and 29, 1905, from a scant trace to a splendidly developed tail. A comparison with a photograph, made by Mr. Duncan at the Lowell Observatory on December 26, limited the time of development to less than three days. The tail underwent remarkable changes from day to day, but through interference from cloudy weather it was not possible with the one set of photographs to distinguish identical features on different dates, even though they existed, to determine the velocity of the tail-producing particles.

On the Distortions of Photographic Films: FRANK SCHLESINGER.

The question as to the amount of distortion that a photographic film suffers during development is not yet settled. The method employed in the present paper is free from some assumptions that have hitherto been made and gives the average amount of the distortions independent of other sources of error. A plate was exposed to the sky and developed and dried in the usual way. It was then measured independently by two observers. Let us call these measurements A' and B'. The plate was soaked in water and then dried. It was now inserted in the measuring engine in, as nearly as possible (within 0.01 mm.), the same position it had occupied and measured a second time by each observer. Instrumental errors were thus eliminated. Call these second measures A'' and B''. Then if e represents the *mean value* of the distortion (in a sense analogous to mean error) and if e_A and e_B are the mean errors of bisection for the two observers, then by appropriate subtractions we obtain

$$[(A'-A'')^2]=[e^2]-2[e_A^2],$$
$$[(B'-B'')^2]=[e^2]-2[e_B^2].$$

If we subtract each of the differences $(B'-B'')$ from the corresponding $(A'-A'')$ the result is free from the effect of distortion and we have

$$[\{(A'-A'')-(B'-B'')\}^2]=2[e_A^2]-2[e_B].$$

These three equations enable us to evaluate e as well as e_A and e_B. Three plates, each with about 60 star images, were treated in this way and gave $\pm$.0008 mm., $\pm$.0006 mm. and $\pm$.0003 mm., respectively, for the mean value of the distortion of the film. The experiment was varied and improved by spattering an *undeveloped* film with ink and measuring the small and perfectly round dots that were thus obtained, before and after the plate had been cleared in hypo, and thoroughly washed and dried. Five plates treated in this way gave, respectively, $\pm$.0006 mm., $\pm$.0006 mm., $\pm$.0014 mm., $\pm$.0015 mm. and $\pm$.0015 mm. These experiments indicate that the distortions of the film are considerably less than inevitable errors like those of bisection, guiding error, etc. If this conclusion should be confirmed by other observers it must have an important bearing on methods for measuring and reducing plates, since it would follow that elaborate precautions, such as have sometimes been employed for guarding against these distortions, are unnecessary.

Distortions of Photographic Films on Glass: Sebastian Albrecht.

In the winter of 1904 an investigation of the distortions of photographic films on glass plates was undertaken by the writer at the suggestion of Director Campbell and Dr. Perrine. A preliminary investigation showed that if ordinary good care is taken in the treatment of the plate during development, fixing, hardening, washing and drying, there will be no general distortions, *i. e.,* no distortions extending over a large area of the film. Accordingly, the work was continued by a method which would more readily determine the character of local distortions. Photographs of spectra with numerous lines of different widths are very well suited for this purpose. These spectrograms showed that there were local distortions. These distortions were confined in each case to one or a few adjacent spectrum lines, and very rarely covered an area as much as 0.25 mm. square. The displacements of individual points in the distorted area occasionally amounted to 0.02 mm., which on the Crossley star photographs is equivalent to about one second of arc, and on the spectrograms taken with the one-prism spectrograph of the Lick Observatory to a radial velocity of about 80 km. per second (for the H_γ region). Such large displacements as these are, however, extremely rare.

The distortions seemed to be of two different kinds: one caused by an actual movement of portions of the film, and the other by a displacement of the image apparently due to local differences in the sensitiveness of the film. Some of the characteristic shapes of the distorted lines due to the first cause were: a sine-curve, a question mark, an irregular crinkle, an abrupt bend, a gradual bend which was sometimes shared by two and occasionally by three lines, while the lines immediately on either side of the distorted ones were perfectly straight.

The structure and chemical composition of the film are materially altered in the process of development and fixing, so that the strains produced in the subsequent drying need not be exactly the same everywhere in the film as they were before development. In fact, slight local readjustments might be expected, and it is remarkable that the resulting movements in the film should be so few in number, and be confined to such minute areas.

A Riefler Clock and a Self-registering Right Ascension Micrometer: W. S. Eichelberger.

In September, 1903, there was installed at the U. S. Naval Observatory one of Riefler's sidereal clocks, No. 70, with a nickel-steel pendulum and mounted in an air-tight glass case. At the same time the following scheme of observing was undertaken with the 9-inch transit circle; one astronomer would observe fundamental and miscellaneous stars for about two hours before sunrise, would observe the day planets and the sun, and would finish his tour of duty with another two hours' star observing just after sunset; a second observer would then commence and do star work for about six hours; and then a third man would observe a list of stars for about two hours before sunrise. Thus there were frequently three clock corrections determined during the same night by three different observers. Each night from September, 1903, to August, 1904, on which two or three men observed, has been utilized for determining the relative personal equations of the various observers. This interval was divided into five parts and an independent determination of the relative personal equation was obtained for each of these shorter intervals with the following results:

	$E-L$	$E-R$	$E-B$	$E-M$
1903	s	s	s	s
Sept. 5 to Oct. 5	+0.210	+0.051		
Oct. 12 to Nov. 13	+0.195	+0.048	+0.188	
1904				
Jan. 30 to Mar. 29		+0.056	+0.139	+0.062
Apr. 3 to May 13		+0.052	+0.119	+0.055
May 24 to Aug. 15			+0.112	+0.019
Weighted Mean	+0.205	+0.052	+0.133	+0.047

The average probable error of one of these individual determinations is $0^s.009$.

All the men except *M* had observed with a transit circle some years before the observations here discussed were begun, yet with one pair of experienced observers there is quite a decided change in the relative personal equation within a few months.

For the same periods and the same pairs of observers the following differences between the largest and smallest value for a single night were obtained.

	$E-L$	$E-R$	$E-B$	$E-M$
1903	s	s	s	s
Sept. 5 to Oct. 5	0.072	0.053		
Oct. 12 to Nov. 13	0.083	0.050	0.069	
1904				
Jan. 30 to Mar. 29		0.067	0.128	0.137
Apr. 3 to May 13		0.090	0.094	0.040
May 24 to Aug. 15			0.140	0.075

The general mean of all these numbers is $0^s.085$.

In attempting to adopt a mean clock correction for the entire night and to test the irregularity of the running of the clock from day to day, this number is certainly large enough to be annoying. If it is due to irregularities in the running of the clock during the night, a new clock is needed. If it is inherent in the old key method of observing transits, something should be done to improve the method.

To test the clock for periodic daily irregularity another Riefler clock, No. 82, was compared with it every morning and afternoon and frequently twice during the night from January 19 to February 2, 1906, and a second time from February 23 to March 9. Between these two periods Riefler No. 70 was dismounted, cleaned and set up again. The probability that any periodic irregularity that might exist in these two clocks would be in the same phase in both series of comparisons is not very large.

The relative daily rate of the two clocks was in the first period

$$0^s.1426 + 0^s.00052\,t$$

and in the second period

$$0^s.0298 + 0^s.00032\,t.$$

The residuals arising from the comparison of the observed and computed differences between the two clocks were then grouped with respect to the tenth of the day at which the clock comparison was made with the following results:

January 19 to February 2, 1906

Decimal of a Day	No. of Comp.	Mean Residual	
		s	s
.0	—	—	
.1	13	+0.004	+0.005
.2	2	+0.008	
.3	1	+0.011	
.4	7	0.000	—0.001
.5	2	—0.014	
.6	5	+0.001	
.7	—	—	
.8	1	+0.008	—0.005
.9	11	—0.006	

February 23 to March 9

Decimal of a Day	No. of Comp.	Mean Residual	
		s	s
.0	—	—	
.1	15	0.000	0.000
.2	—	—	
.3	6	+0.002	
.4	7	0.000	0.000
.5	—	—	
.6	4	—0.005	
.7	—	—	
.8	—	—	
.9	14	0.000	0.000

Of the 88 separate residuals three quarters are less than $0^s.01$ each, a quantity that I had supposed less than the uncertainty with which a clock comparison was read from the chronograph sheet.

At the end of October, 1906, the 6-inch transit circle was equipped with a self-registering right ascension micrometer of the Repsold type made by Warner and Swasey.

Previous to beginning the following series of observations, one of the observers had practised with the new micrometer on two nights and the other two on one night each. Two men worked on each of the eight nights, each observing ten time stars. Each man on half of the nights that he worked observed the first list of ten stars, and on the remaining nights the second list. The same twenty stars were used throughout the entire series.

A least-square solution gives for relative personal equation

$$E-F=+0^s.001 \pm 0^s.012$$

and

$$E-L=-0^s.001 \pm 0^s.015.$$

The relative personal equation $E-L$ by the key method of observing was $+0^s.20$.

The clock rates during these six weeks were also computed by least squares, first giving a weight unity to each rate and, second, a weight equal to the interval between the observations from which it was determined.

The resulting rates are

$$-0^s.0436+0.000924\,(T-\text{Nov. } 25.0)$$

and

$$-0^s.0445+0.000997\,(T-\text{Nov. } 25.0),$$

giving as residuals:

$O-C$	Wt.	$O-C$	Wt.
s		s	
−0.011	1	−0.009	8
+0.015	1	+0.017	5
+0.010	1	+0.012	1
−0.023	1	−0.022	2
+0.011	1	+0.012	4
−0.001	1	0.000	13
−0.003	1	−0.003	7
±0.011		±0.007	

A similar discussion of rates obtained by the key method of observing transits gave for thirteen weeks in the spring of 1904 a mean residual of $0^s.015$.

We thus see that by the use of the self-registering right ascension micrometer in this series of observations the mean residual of the daily clock corrections is decreased from 25 to 50 per cent. and relative personal equation has disappeared.

Harold Jacoby,
For the Council

(To be concluded)

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SECTION I—SOCIAL AND ECONOMIC SCIENCE

Fifteen different papers and addresses were presented at the New York session. The attendance varied from thirty-five to over a hundred persons. One joint session with Section H was held. In the absence of Professor Irving Fisher, whose address has already appeared in Science, at the opening session, the incoming vice-president for the section, Charles A. Conant, presided, beginning the proceedings with an introductory discussion on banking reform, the substance of which was as follows:

Unless some sound legislation is in due time enacted, our prosperity will be arrested, our rapidly absorbed currency will prove entirely inadequate for business needs, and we must be put at a great disadvantage at home and in our competition with foreign nations in the open markets of the world.

The proposed banknote reform, it should be understood, is not a measure intended to aid the banks, but to remove restrictions upon the use of credit in this form where it is found to be useful. If the government will remove the outgrown restrictions and require proper security, there can be no danger to any economic interest involved.

Commerce determines the amount of bullion currency, not the government. And this is just what we ask in regard to note issues. The idea that a government can or ought to determine the amount of money in circulation is not held in any civilized country. The government ought only to determine the rules that insure that a currency shall be what it purports to be, and that it shall have stability and flexibility.

The regular papers were presented in the following order:

The Evolution of Property: Logan G. McPherson, New York City.

The problems of to-day in the United States are largely problems concerning property. The corporate régime has extended; and attacks upon corporations have extended.

The change from the artisan and the tradesman doing business in a small way to the great organization operating in an extending field, unquestionably marks one of the great transitions in the mechanism whereby property is held and transferred, one of those cataclysms, it being understood that every cataclysm is the slowly accumulated result of forces long at work, one of those cataclysms that has marked the passage from one property régime to another. Three such régimes are clearly discernible in the history of the human race. These are the tribal or gentile régime, the feudal régime and the joint stock or corporate régime of property, representing in a general way the ancient, medieval and modern forms of property evolution.

Under the system of the gens it was every family for itself. Under the system of Cleisthenes it became, subject of course to the modifications of the political government, every man for himself. The régime of the joint stock company has come with that progress of invention through which the efforts of all men are coordinated for the benefit of all men. This régime tends more and more to reach that status whereunder any person who through frugality, thrift, foresight obtains a measure of resource above his living need may participate in the ownership of one or more of the great corporations that produce and distribute the things of material need.

That it is possible for a great organization to produce more cheaply and sell more cheaply, that the ability of administrative officers can be directed into channels for which it is best fitted, that it presents—if properly steered—a formidable resistance to the dangers of both the waves of prosperity and the slough of adversity have time and again been demonstrated in theory.

That in practise all of these advantages have not been attained at all times by all corporations, that in practise all of these advantages may not have been attained at any one time by all corporations, is known to all men. It may even be that there are corporations which have not in practise attained any of these advantages at any time. But it is incontestable that every one of these advantages has been attained at one time or another by one corporation or another; and the ultimate salvation of any corporation lies in its attaining and maintaining in its practise every one of these advantages all the time.

The Concentration of Wealth: Henry Laurens Call, Washington, D. C.

Fifty years ago there were not to exceed fifty millionaires in the whole of the United

States; and their combined fortunes—including the half-millionaires as well—did not exceed a probable $100,000,000, or one per cent. of the then aggregate wealth of the nation. Sixteen years ago the combined fortunes of this class were estimated at $36,500,000,000, or fifty-six per cent. of our national wealth. To-day a bare one per cent. of our population owns practically ninety per cent. of the entire wealth of the nation.

As a result of this wealth concentration, industrial society is practically divided into the two classes, of the enormously rich, and the miserably poor; our 18,000,000 wage earners receive an average of but $400 per year; nine tenths of our business men are, notoriously, failures; our clergy receive an average annual salary of about $500; the average for the educators of the land is even lower; and the income of other professional men in proportion; while of our 6,000,000 farmers, one third are tenants, and the homes of one third of the remaining two thirds are mortgaged, and a debt burden is almost universal.

These conditions are not normal nor the result of national law or causation, but are instead the result of a monopoly of land and mineral resources, of money, of transportation, and other public utilities, as also of industry. This monopoly has, moreover, been brought about by means of the corporation, industrial, financial and public service. It is thus the work of human law alone; the product of vicious institutions. The corporation, as constituted, is in fact a monstrosity in our industrial system.

But if the conditions outlined are the result alone of unjust and vicious institutions, then to law must we look for their correction. The corporation should yet be made cooperative, social, instead of as now the instrument of private greed.

Methods of ascertaining the Cost of Transportation: JOHN B. DAISH, Washington, D. C.

From the adjudicated court cases it seems reasonably certain that the method used by the Supreme Court in Smyth *v.* Ames is inaccurate, for the relation which operating expenses bear to gross receipts does not and can never show the cost of transporting the commodities. The method used by the Circuit Court of the United States for the District of South Dakota could not stand the tests applied by the Supreme Court of the United States, which tests, it will be recalled, were different from the method formerly used by that tribunal. The method used by the Kentucky Railroad Commission is clearly more elaborate if not more correct, than the Supreme Court cases.

To determine with mathematical accuracy the cost of traffic, a definite method has not as yet been devised. The difficulty is twofold. First, on the one line of road, with a single equipment, two kinds of traffic are carried. How is the capital to be separated for the purpose of producing revenue upon these two kinds of traffic? Again, the method used by the Kentucky Commission considers the whole schedule of rates, rates in gross, and secures its results in figures per ton per mile. In short, in the same focus it takes a broad survey of all kinds of commodities in the two kinds of traffic and microscopically looks at the smallest possible unit of measurement. The shipment of a box of books is augmented to per ton per mile, a shipment of a car-load or train-load of vegetables is reduced to the same unit. While this unit may be used in mathematics, it can be confidently asserted that it rarely, if ever, enters the head of a traffic manager making rates; ordinarily he does not consider the cost of traffic, for he does not know it.

The difficulties in this matter seem to be the unwarranted assumptions, the legal fictions, as it were, within which there might be such a variation as to cause rates (single or as a schedule) to be unreasonably low on the one hand or extortionate on the other.

The assumptions heretofore made may or may not be true. Until proved correct, we can not hope to ascertain to a mathematical certainty the cost of transportation to the carrier by any of the methods considered or one hereafter to be devised.

Competition versus Combination of Railroads as affecting the Basis of Rate-making: H. G. BROWN, Yale University.

The fact that railroads are operated under the law of increasing returns lies at the basis of the systems of rate making. Increased traffic is carried at proportionately less cost and yields proportionately greater profit. It therefore becomes worth while for a railroad company to get this increased traffic even if somewhat lower rates must be conceded. But lower rates will not be conceded unless there is a compensating increase of business. Reductions will be made where traffic is responsive and only where it is responsive.

Discrimination between different cities is largely due to railroad competition. When two railroads connect a couple of cities by different routes, each has a monopoly of the traffic between intermediate points on its own line. Hence there is discrimination in favor of the through traffic, which is most responsive. The reductions take place on the through traffic not because it is absolutely responsive, not, that is, because by such reductions traffic as a whole can be increased, but only because it is relatively responsive, because traffic is so gained by one road at the expense of another. If in the absence of competition reductions are made in favor of traffic between any two places, it will only be when such traffic is absolutely responsive, when the total traffic of the district served is thus increased.

The discriminations among different shippers, which have been so common with competing railroad companies, are similarly explainable. The concessions to large corporations forced by the threat to transfer patronage to another railroad are concessions made where considerable traffic is in question, where, therefore, traffic is responsive and where it is relatively responsive. It is not so much that these discriminating reductions increase traffic as a whole as that they increase the traffic of one company at the expense of another.

The tendencies of competition and of combination to affect diversely the basis of rate charges, whether among different cities, among different corporations, or among different commodities, have been reduced to the generalization that rate reductions will take place *where* traffic is sufficiently responsive to such reductions and *only* where it is sufficiently responsive. With competing roads it is chiefly relative responsiveness and with non-competing roads it is absolute responsiveness only that determines a lowering of rate.

Psychic Factors in the Fluctuation of Prices: RICHARD T. COLBURN, New York City.

The first decade of the twentieth century has many surprises for America and for Europe, political, social and financial; and in none more remarkable than the expansion of commerce and credits. These are not accurately reflected by the visible balance of trade figures, but by an invisible ledger balance of credit. This is born of confidence—a state of mind—our credit is good, perhaps too good. We are the good customer of tradition, suggestive of the spendthrift.

The 'economic man' of the text-books is unlike the actual man (and woman) of society, because he is often swayed by a group of impulses, emotions and psychic phases which have little reference to his gain or loss and range into the irrational or pathological. Many illustrations might be given from history or from current events. The inflation of stock-exchange dealings, real-estate booms; speculative 'buying for a rise' in commodities, holiday gifts, bargain-counter rushes; wedding, funeral and religious expenditures. The imitation of the crowd seems a large factor, and in spite of the spread of knowledge and useful inventions, the range of rise and fall in prices is more extreme and the psychic factors equally important.

Economic and Social Conditions of Porto Rico: John L. Hogan, New York.

The main need of this people is now initiative and leadership in lines of productive enterprises with which the Porto Ricans are themselves more or less acquainted and which are based on utilizing their natural resources and their climatic advantages, as in the growing of tropical fruits, canning vegetable products, etc. Their talent as lace-makers is a fine art that is being developed by at least one company which is introducing the needlework of Porto Rican women and children to the markets of the United States. The government has succeeded only moderately in promoting prosperity; the main reliance is now and always on far-sighted private enterprise.

The Charter and Work of the Board of Education of St. Louis: Calvin M. Woodward, St. Louis.

The discussion of the subject included details of the charter, the reasons for its adoption, the organization and work of the board, its non-partisan character and how it is maintained. Some obvious fruits of the general policy adopted by the board were described. The reports of the board give much information on these topics.

Ethical Training for Efficient Citizenship: Felix Adler, New York.

An outline of the qualities which have been regarded as essential for good citizenship, from the days of Plato (see his 'Republic') and Aristotle (see his 'Politics') to the present time.

Training for Physical Efficiency as an Aim of Education: G. O. Brewster, New York.

Present methods of physical training aim to improve the body in both normal and abnormal conditions. Exercises known as hygienic and corrective are employed to this end. The highest aim of education is the development of reason, and reason must have facts as a basis. In present methods of physical education I have been unable to discover reason, inasmuch as there are no facts or definite knowledge of the chief subject under consideration employed. The chief consideration is the individual, and a definite knowledge of himself is necessary to a reasonable understanding of his work in physical education. The smattering of physiology and hygiene, which the pupil receives during the formative period of life, aims to conceal rather than reveal the facts of life, from fecundation to birth and from birth to death.

Our aim is supposedly to leave better men and women in our places than we are, and to accomplish this result we must give them the benefit of our mistakes through our lack of knowledge or whatever cause regardless of pride and personal feeling.

In my experience in school teaching and the practise of medicine in this city, I came to feel that ignorance of the structure and functions of the body was accountable for the majority of humanity's ills.

Therefore I believe it necessary to the physical training of our youth, *not* that they should receive more exercise or mentally exhaustive and complicated drills for the exercise of muscles and functions of which they know nothing, but that instead they should receive simple, recreative exercises graded according to their physiological knowledge of themselves, and so applied that they can know what occurs in their physiological economy when they exercise either body or mind—to the end that physical efficiency be increased as abuses decrease and the standard of life proportionately raised.

Standards of Social Efficiency in School Policy: John Franklin Crowell, New York.

There are three requisites by which school work has to be judged to determine its level in the scale of social worth. They are the economic, the political and the spiritual standards.

The economic standard of efficiency requires that the school training shall first of all bend its efforts to guarantee to the individual the capacity to make a decent living and maintain a respectable livelihood for himself and those rightfully dependent upon him.

The political standard requires that the individual have that degree of intelligence and self-expression to participate in constructive discussion on questions involving the common welfare.

The spiritual standard requires that there be developed in each pupil the love of things moral, intellectual and spiritual so that he may appropriate for himself the best that our civilization affords.

Some Results of Research in Child-employing Industries: Owen R. Lovejoy, Assistant Secretary National Child Labor Committee.

The census for 1900 shows 1,750,178 children between ten and fifteen years engaged in gainful occupations. The majority are in agricultural labor, but the numbers increase most rapidly in the industrial trades. While the chief evils are in the great industrial centers, we have found poverty, ignorance and neglect of a large number of children in country communities. There is evidence that, in some branches of agriculture the hours of labor and housing conditions are sufficiently alarming to warrant the careful regulation of child labor in agriculture.

Research in manufacturing and mechanical pursuits, mercantile establishments and street trades, indicates that as soon as childhood shows its capacity for any specific form of labor, industry at once seizes it, and posits an economic necessity for so doing; that the physical effects of child labor are a menace to society; that the effect upon family income is detrimental; and that child labor tends to industrial deterioration.

While many phases of our research must be carried on for several years before positive results are announced, we believe we are justified in urging the following demands in opposition to child labor: (1) The regulation of all industries in which children are employed, (2) the entire elimination of the child under fourteen as an industrial factor, (3) the restriction of employment for children between fourteen and sixteen, prohibiting the defective and the illiterate and carefully regulating the hours and employment of others, (4) the agreement of age standards in child labor laws and compulsory education laws and the fitting of our educational curriculum to the needs of our population, by providing such training of the hand as will appeal to the parents as promotive of the future industrial efficiency of their children.

News Service and Political Science: Franklin Ford, General News Office, New York City.

The gathering and sale of news for a profit is a modern development. It was hardly sold at all before the time of Charles II. in England. Of one thing you may rest assured, that when some man in London first conceived the possibility of getting his living by the gathering and sale of news, that day unjust privilege had, clearly and definitely, started on the road to its last ditch. And if you wish to dramatize this assertion you have only to think of a pack of reporters at the heels of derelict life-insurance officials.

The fast press and the talking wire have come in, but the manner of classifying and handling news—the mode of interpretation—has not changed essentially. True organization in the field of news has been waiting on the introduction of a new point of view, of a new way of ideas. Scientific method has entered nearly all the great branches of manufacturing, but a new coordination has yet to obtain in news handling.

To organize news we must classify it, which implies a science of news. But, as already indicated, since the news movement relates to all organs or functions in the social body, the desired science must in the nature of things be the science of Politics. Call it sociology, if you will; there is no difference.

The pending advance in news organization to the level of the highest integrity introduces the oncoming new legislature, or the fact-finding and law-declaring organ. News is the universal element of social control.

From the business standpoint, when a new fact is disclosed in the news movement it carries three possible sales or profits: (1) Its general application to be distributed through the daily paper, (2) its class application to be sold through the trade paper, and (3) its special application to individuals which they can buy through the bureau of information.

The Rate of Sickness as determined by the Companies doing Health Insurance in the United States, particularly, the Experience of the Travelers Insurance Company of Hartford: Hiram J. Messenger, Hartford, Conn.

The first attempts to write health insurance in the United States were made about sixty years ago and all efforts following these attempts up to about 1897 were practically failures on account of the lack of knowledge of the business and lack of solid financial backing. About 1897 a stock accident company commenced doing health insurance, first by issuing a special health policy limited to fifteen diseases which was later increased to twenty-one and then to over thirty. The general tendency was towards a more comprehensive contract and after a few years most of the companies were issuing a health policy which covered practically all diseases. The experience of the Travelers Insurance Company on their special health policy shows .29 of a week of sickness for each year of exposure for ages 18–25, and gradually decreases to .18 for ages 40–49, and then increases to .30 for ages 55–60. On the general health policy the number of weeks sickness per year of exposure is about .65 for ages 18–50 and then rapidly increases to .95 for ages 55–60. Several of the accident companies have combined their experience on their health policy, covering practically all diseases, with the result giving a rate of sickness about 60 per cent. of the Travelers' experience.

The Manchester Unity's experience of England shows .81 of a week of sickness for ages 16–19 and from there on gradually increases to 1.44 for ages 40–44 and 3.76

for ages 55–59, representing risks that have been under observation for a longer period.

Special investigations in connection with this experience show in general that there is considerably more sickness among the outdoor industrial classes than among the indoor industrial classes and slightly more sickness among the indoor industrial classes than among the clerical and professional classes. The amount of sickness in the north and west is about the same and in each case is less than in the south—while the amount of sickness in large cities is less than in the small towns.

The following subjects were read by title:

Two Neglected Subjects of Social Economy, including interment and cremation considered from sanitary and sentimental standpoints: by MRS. L. OSBORNE TALBOTT, Washington, D. C.

Relation of Death Rates and Birth Rates, applying well known natural laws to the explanation of sociologic phenomena, and concluding that the far-reaching sociologic effects of the reduction of the birth rate are reduction of death rate, survival of the most intelligent, loss of physical robustness, and growth of higher civilization and democracy: CHARLES E. WOODRUFF, U. S. A., Plattsburg, N. Y.

The Brain and Education: THOMAS M. BALLIET, New York University. The main thesis being that while our knowledge of the brain is too scanty to build a theory of education on it, neurology can throw many side-lights on educational problems.

J. FRANKLIN CROWELL,
Secretary

SCIENTIFIC BOOKS

Chemical Abstracts. Published by the American Chemical Society. Edited by WILLIAM A. NOYES and C. A. WATERS, with the cooperation of a large staff of assistant editors and abstractors. Published semi-monthly. Easton, Pa., Chemical Publishing Co. Vol. I., No. 1, January 1, 1907. Price, $6.00 per annum.

Brief abstracts are given of all chemical investigations of any importance published in any part of the world. These abstracts are classified under the following sectional headings, each section being in charge of an expert in that particular branch of chemistry: Apparatus (Walker); General and Physical Chemistry (Lewis); Radioactivity (McCoy); Electrochemistry (Whitney); Photography (Friedburg); Inorganic Chemistry (Smith); Analytical Chemistry (Dennis); Mineralogical and Geological Chemistry (Hillebrand); Metallurgy (Richards); Organic Chemistry (Bogert); Biological Chemistry (Mendel); Foods (Bigelow); Nutrition (Langworthy); Water, Sewage and Sanitation (Kinnicutt); Soils and Fertilizers (Veitch); Fermented and Distilled Liquors (Wahl); Pharmaceutical Chemistry (Stevens); Acids, Alkalies and Salts (Briggs); Glass and Pottery (Barton and Bleininger); Cements and Mortars (Drew); Fuel, Gas and Coke (Pennock); Petroleum, Asphalt and Wood Products (Sadtler); Cellulose and Paper (Little); Explosives (Munroe); Dyes, Bleaching and Textile Fabrics (Olney); Pigments, Resins, Varnishes and India Rubber (Sabin); Fats, Fatty Oils and Soap (Richardson); Sugar, Starch and Gums (Browne); Leather; Patents (U. S., British, German and French) (Seaman).

The appearance of this journal marks an epoch in the progress of chemistry in America, and is an achievement of which the American Chemical Society may well feel proud. The establishment of such a publication has been made possible by the untiring efforts of the editor, Dr. Noyes, and the loyal support of more than 130 of the leading chemists of the country who are acting as assistant editors and abstractors either gratuitously or at a merely nominal remuneration. The society is to be congratulated also on the fact that it has had the courage to undertake this publication single-handed and without entering into 'entangling alliances' with any other organization. This simplifies greatly the business management of the venture, insures more rapid appearance of the abstracts, and should appeal strongly to the patriotism of all American chemists. It was feared at the outset that the project might not be a financial success, and that the necessary increase in the membership dues from five dollars to eight dollars would be followed by many resignations. This fear was apparently groundless. But few resignations have been received as the result of this increase in dues. On the contrary, the membership of the society is now increasing more rapidly than ever before.

The value to the profession of such a journal can scarcely be overestimated. It will not only bring before every American chemist a concise summary, in his own native tongue, of all important chemical investigations, thus enabling him to keep abreast of the progress of the science, stimulating and encouraging research, but, furthermore, as it covers all branches of the subject, it should prove a most potent factor in drawing together in bonds of closer cooperation all chemists in this country.

Its only rival at the present time is the *Chemisches Zentralblatt* of the German Chemical Society, which has had a monopoly of this field for many decades. It is much more expensive than *Chemical Abstracts,* and in some respects (notably on the technical side) is not so complete. On the other hand, the German publication is superior to ours in certain points—it appears weekly, instead of twice a month; diagrams and illustrations are more freely used; and the abstracts do not have to be condensed quite as much, not only because they have more space available, but also because of a much more extensive use of abbreviations.

In *Chemical Abstracts* the American Chemical Society has produced a most excellent journal, and all American chemists should rally to its support, to make it, as it should be, the best of its kind in the world. Those who are not already members can best assist by joining at once and urging others to do likewise.[1] MARSTON TAYLOR BOGERT

[1] The secretary of the society is Dr. W. A. Noyes, Bureau of Standards, Washington, D. C.

SOCIETIES AND ACADEMIES

THE AMERICAN PHYSICAL SOCIETY

A REGULAR meeting of the Physical Society was held in Fayerweather Hall, Columbia University, on March 2, 1907. In the absence of the president, Professor W. C. Sabine was made temporary chairman.

An address was delivered before the society by Professor O. Lummer, of the University of Breslau, on 'The Temperature of the Sun and Recent Solar Theories.'

The following papers were then presented:

E. F. NORTHRUP: 'On the Forces on the Interior of a Conductor Carrying Current.'

C. W. WAIDNER and G. K. BURGESS: 'The Radiation from, and the Melting Points of, Palladium and Platinum.'

F. M. PEDERSON: 'The Viscosity of Certain Isomeric Ether Compounds.'

J. G. COFFIN: 'The Effect of Frequency upon the Capacity of Absolute Condensers.'

C. C. TROWBRIDGE: 'The Physical Nature of Meteor Trains.'

C. C. TROWBRIDGE: 'On Atmospheric Drifts above Fifty Miles from the Surface of the Earth.'

C. C. PERRY: 'On the Current carried by Canal Rays in a Discharge Tube.'

CARL BARUS: 'The Equations of the Fog Chamber.

W. G. CADY: 'Note on the Hissing Metallic Arc.'

F. L. TUFTS: 'The Relation between Luminosity and Electrical Conductivity of Flames.'

ERNEST MERRITT,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 425th meeting was held on February 9, 1907, with President Stejneger in the chair.

Dr. A. D. Hopkins read a paper, illustrated with lantern slides, on 'Some Results of Anatomical Investigations of the Thoracic Segment of Insects.' The substance of this paper will appear in a bulletin of the Bureau of Entomology.

Mr. T. H. Kearney spoke on 'The Date Palm in the Northern Sahara,' illustrating his subject with a large number of lantern slides. He described the oases of southern Tunis and especially the group known as the Djerid, where numerous fine varieties are grown. Methods of irrigating, cultivating, pollinating and harvesting were discussed. Attention was called to the great number of varieties existing in this region and to the characters by which they are distinguished. A description was given of the Oued Souf oases in eastern Algeria and of the peculiar method of growing date palms there in 'sunken gardens' among high sand dunes. The efforts of the Department of Agriculture to establish date culture in the southwestern United States were also discussed.

The 426th meeting was held on February 23, 1907, with President Stejneger in the chair.

Mr. C. V. Piper presented a paper extensively illustrated by lantern slides, on 'Some Features in the Distribution of Life in the Columbia Basin.' The Columbia Basin comprises practically all of Washington and Oregon east of the Cascade Mountains and the greater part of Idaho. This region is entirely covered with the Columbia lava of an average thickness of about 1,000 meters. The central portion of the basin is the lowest, ranging from 150 to 300 meters altitude, from which it gradually rises in all directions to the surrounding mountains. The Cascade Mountains on the west, the Bitter-Root and Rocky Mountains on the east as well as the Blue Mountains, are inhabited mainly by boreal plants and animals pushed southward during the glacial period. The lowest part of the basin is occupied by the upper Sonoran plants and animals and the intermediate region by arid transition forms.

In studying the inhabitants of this region statistically, it is found that, if plants of continental range be excluded amounting to about 30 per cent. of the whole, that the following proportions appear. Of upper Sonoran plants, 7 per cent. are of California origin, 64 per cent. of Great Basin origin and 29 per cent. are endemic. Of the arid transition plants 33 per cent. are of California origin, 26 per cent. of great basin and 31 per cent. endemic. It will be noticed, on comparing these figures, that Californian arid transition species have much freer access to the region than upper Sonoran species. Indeed, at the present time the arid transition area is still continuous from California into the Columbia Basin. This with the prevailing southwest winds is probably the reason why these plants dominate over those of Great Basin origin in the Columbia Basin, though the latter apparently have much easier access. In the Columbia Basin the upper Sonoran life area is practically coextensive with the area where the rainfall is 12 inches or less and the characteristic plant is the common sage brush.

A second point of interest in the distribution of plants in this region, is the relation which exists between the plants of the Blue Mountains, those of the northernmost part of the Cascade Mountains and those of the Siskiyou Mountains. These regions are composed largely of granitic mountains and in each of them there is a considerable number of peculiar species. It was suggested that the explanation of this was to be found in the fact that the greater portion of the Cascade Mountain region is volcanic in character and comparatively recent in origin. It is possible, therefore, that the plant species peculiar to the granitic regions either prefer granitic soils or else through volcanic conditions have disappeared from the greater portion of the Cascade Mountains. While neither of these explanations is entirely satisfactory the fact of the peculiar relations of their floras is very clear.

Another striking feature mentioned by the speaker was in regard to the islands in the northern part of Puget Sound. These islands, including the southwest extremity of the Vancouver Island, lie in the lee of the Olympic Mountains and have the lowest rainfall of any of the country west of the Cascade Mountains. The rainfall on these islands varies from 19 to 31 inches, practically paralleling the conditions of the arid transition area east of the Cascade Mountains. The Cascade Mountain form at the present time is a perfect barrier for arid transition and upper Sonoran plants. Nevertheless, in some way a considerable number of such species have found their way to this region of low rainfall in the northern part of Puget Sound. Among these are *Opuntia missouriensis, Juniperus scopulorum, Platyspermum scapigerum, Zygadenus paniculatus, Lupinus microcarpus* and many others. The speaker stated that he was unable to give any satisfactory explanation as to how these arid land plants could have crossed the barrier of the Cascade Mountains.

In discussing this point, Mr. M. B. Waite suggested that seeds of these plants might have been carried by birds or possibly Indians. The speaker admitted that both of these explanations were possible, although he did not regard either of them as likely. Mr. Vernon Bailey called attention to the distribution of many mammals in the Columbia Basin which virtually coincide with that of the plants.

Dr. Leonard Stejneger presented the next paper on 'The Celtic Horse in Norway.' The Celtic horse (*Equus caballus celticus*) was briefly described by Professor Ewart in 1902 as a small pony from the Outer Hebrides, northwestern Ireland, Shetland, Færœs and Iceland, and more fully characterized in 1904 in a paper on the multiple origin of the horses in Scotland, in which he compares it with the 'Norse horse' (*E. caballus typicus*), the essential differences consisting in different proportions of forehead and snout and in the absence of callosities on the hind legs of the Celtic pony. The latter character at one time was supposed to be of generic value since these callosities are absent in the asses and zebras.

The speaker, during 1905, was able to verify his suspicions that the fjord-horse of western Norway is identical with the Celtic pony by examining a number of pure-bred west Norwegian horses which had no hind callosities and which in other respects also agreed with Ewart's description. The apparent discrepancy between the views of the speaker and that of Professor Ewart relative to the identity of the Norse horse was explained by the fact that there are two native races in Norway, the fjord-horse which is identical with the Celtic, and the valley-horse, or eastern Norwegian horse, which is the one to which Ewart has reference.

The important conclusion to be drawn from the identity of the Scotch and the west Norway pony is that it probably came to the latter country from Scotland simultaneously with and possibly domesticated by the West Norwegian brachycephalic population. This invasion may have occurred in late pleistocene or early post-glacial times, at the same period as a complex assemblage of plants and animals, known as the 'Atlantic' biota crossed from Scotland to west Norway over a continuous land bridge, a question more fully discussed in a paper now going through the press.

In the discussion following Dr. Stejneger, replying to a question, stated that no skeleton or part of skeleton of the Celtic horse is in any museum in this country, and that even in European museums, except that of the Landwirthschaftliche Hochschule in Berlin, but little effort has been made to collect series of the domesticated animals.

Dr. Gill suggested that anatomical characters derived from the laryngeal apparatus might furnish better ground for generic distinctions among the Equidæ than the hind callosities.

M. C. Marsh,
Recording Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 630th meeting was held on the evening of March 2, 1907, President Hayford in the chair. Mr. W. P. White read a paper on 'Melting-point Determinations,' stating that melting points are usually determined at higher temperatures by heating the material steadily and noting the stationary temperature which occurs during melting. With perfectly pure substances, this method presents no difficulties or complications. With practically all substances, however, sufficient impurity is present to make the melting extend through a temperature interval. This effect is greater at high temperatures. The proper curve is then distorted from two causes: (1) The supply of heat from the furnace usually varies. This trouble may be corrected by regulating so as to keep the difference of temperature between the furnace and charge constant. The furnace is then held nearly stationary during the melting. (2) The interior of the charge lags behind the outside during melting; this effect becomes negligible if small charges are used. An ordinary platinum thermoelement can be put naked in a charge of sodium chloride, melted silicate, etc., without perceptible error, and thus excels the electric thermometer for work with small charges, and even with fairly large ones. The best results have been obtained with about 1 c.c. of material. Error from conduction of heat along the thermoelement may exist, but is very small with bare elements directly immersed.

Mr. E. B. Rosa presented a paper on 'Preliminary Studies in a New Determination of the Ohm,' in which it was stated that various methods of measuring resistance in absolute units have been employed, and the results obtained by different investigators expressed in terms of the length of the column of mercury having a resistance of one ohm, range from 106.21 cm. to 106.34 cm., omitting some of the least accurate determinations. The value taken for the international ohm, 106.30 cm., is uncertain by several units in the last decimal place. If one were to attempt to fix the last figure, or in other words get a value that should not be in error by more than 1 part in 10,000, it would be necessary to use only the best methods and to calculate the mutual or self-inductances involved in the determinations at least to one in 25,000.

The best form of self-inductance to be computed from its dimensions is a single layer winding on an accurately ground cylinder. The formulæ heretofore used for this case are correct only for a current sheet, and the speaker has recently derived the formulæ for correcting this expression so as to make it possible to compute accurately the self-inductance of a winding of insulated round wire.

For the method of mutual inductance, the most favorable case is perhaps two equal coils of large radii, and square cross section, placed at a measured distance apart, and the speaker had recently corrected the formulæ of Weinstein and Stefan, so that the value of the mutual inductance can be calculated with high precision, even when the cross section of the winding is relatively large. He had also worked out a new formula for the method of Lorenz and investigated the magnitude of possible errors due to the thickness of the disc, imperfections of centering and errors in the dimensions. The uncertainty arising from variation in the speed of the disc will be very small, using the method of holding the speed steady which has been in use at the U. S. Bureau of Standards for some time.

A brief account was also given of the effect of moisture on the resistance of coils of wire covered with shellac, which causes the resistance to increase with the humidity, and hence to be greater in summer than in winter. This is prevented by sealing the coils air-tight or by covering them with paraffine.

R. L. FARIS,
Secretary

THE TORREY BOTANICAL CLUB

THE club met on March 12, 1907, at the American Museum of Natural History at 8:15 P.M., with President Rusby presiding. Ten persons were present.

The following scientific program was presented:

Remarks on Regeneration: Miss ELSIE M. KUPFER.

The various meanings which have been assigned to the word regeneration were first discussed. It was brought out that, while some writers would limit the term to the restoration of embryonic tissue in root and shoot, others would include within the scope of the process merely the development of buds present before injury. It seemed best to take the middle ground and consider as a regeneration an organ formed anew after injury or loss.

The different plant organs were used as cuttings and their behavior examined when buds were absent. On the roots which formed shoots it was found that these were not confined to the upper (basal) surface, but could appear from the apical as well, or from the middle of the root. The roots of less than half of the species used formed shoots, while all produced roots not always as true regenerations, but as outgrowths from the uninjured *cambium*. Bundleless stems proved able to root with ease, but were unable to replace the buds which had been cut out. Such parts continued growing for fifteen months without undergoing any tissue change, while a part on which a single bud was left established secondary vascular strands between the bud and the new roots. The pseudobulb of an orchid proved able to regenerate roots and a shoot from the base, and in a conifer the apparent 'restoration' of a single root on the seedling and in an older stem-part was described. Of eighty-two species of leaves used in experimentation only two new ones were found which produced a shoot, though the large majority formed roots. Modified leaves of various types, phyllodes and bulb-scales, were also found to be able to root. Regeneration was likewise reported in the inflorescence of *Dudleya californica* and *Ruellia rosea*, in the fruits of *Phaseolus vulgaris* and *P. lunatus*, and finally in the 'head' of the alga *Penicillus capitatus*.

An extended discussion followed.

Owing to the lateness of the hour, Dr. Rusby did not present his paper on 'Field Observations of the Past Year,' but exhibited a few interesting plants collected at Oscoda, Mich.

Dr. Southwick exhibited several interesting specimens of the seeds of *Ricinus*.

C. Stuart Gager,
Secretary

DISCUSSION AND CORRESPONDENCE

THE ANTHROPOLOGICAL EXHIBITS AT THE AMERICAN MUSEUM OF NATURAL HISTORY

To the Editor of Science: The March number of the *American Museum Journal* contains a brief description of 'A New Eskimo Exhibit.' In the first paragraph of this article is found the following: "the American Museum stands preeminent among all institutions along the lines of ethnological research amid Arctic peoples. The completeness of the material and data thus assembled has enabled the Museum to install a series of groups and cases which illustrate vividly the home and village life of the Central Eskimo, together with their utensils, implements and weapons and the methods of using them." Those who are familiar with the historical development of the department of anthropology of this institution realize the significance of this article. It seems to point to a change in policy which is so far-reaching and of such importance to all American anthropologists as to deserve consideration. I propose to consider, therefore, the question whether the old point of view is so entirely wrong and the new point of view is so entirely right as to warrant this change which blots out of existence the results of many years' work.

The activity of the department of anthropology in the American Museum of Natural History became very great ten or twelve years ago and continued with increasing strength until about two years ago, at which time there seems to have been a change in the administration. During the ten years above referred to, we find a systematic attempt on the part of those in charge to carry on investigations over an ever-increasing large area as fast as means would permit. As a result of this intelligently directed series of field operations there grew up in the American Museum one of the greatest departments of anthropology to be found in any museum in the world. The plan of exhibition was on the broadest and most liberal scale. One could for the first time in an American museum study in detail the essential and salient features of the culture of a very large number of tribes, especially those of North America and northeastern Asia; and it seemed it was only a question of time and the continuance of the same policy when all cultures, exclusive of that of Europe, would be found adequately represented. It seems doubtful if any institution ever acquired in the same period of time collections of such magnitude or ever accumulated material with such intelligence or exhibited it in an equally sound manner. Here one could really study the culture of tribes, one could study conditions as they exist; one felt that one was not looking at the illustrations of some elementary text-book, but that he had in front of him the data from which the history of the material culture of mankind might be written. One felt instantly in the halls of the department the spirit of investigation and it was everywhere apparent that this was prompted by the desire to advance science and not by the desire to find material which would fit into or harmonize with some ideal scheme of exhibition. One instinctively felt in the presence of these exhibits that one was in close contact with actual conditions and that one was studying people at close hand, for everywhere was present the evidence of intelligent direction. It was evident that the objects on exhibition were neither placed there with the idea of their beauty nor was their arrangement such as to present primarily a beautiful picture, but rather one felt that as one passed from the exhibit of one tribe to that of another that the dominating features of each culture were so presented that they were apparent, and of course this was due to the fact that the work of collecting and exhibiting had been performed in an intelligent manner. The collections revealed so far as possible the influence of environment both geographical and historical as the culture of one tribe upon that of another. This great series of exhibits properly excited the admiration of anthropologists both at home and abroad, and the wonderful growth of the department in such a short time deserved the admiration of all who were engaged in the study of anthropology.

It seems, however, that the point of view in the installation of this great mass of material was wrong, for on visiting the museum to-day one finds the condition so different from that which prevailed two or three years ago that one necessarily infers that the old point of view is no longer held. It seems, furthermore, that the present condition may be regarded as a visible manifestation of this complete change in policy. The first evidence of a change in the point of view became manifest when the great gallery containing the archeological collection from South America was thrown open to the public. The character of the scheme of installation of this collection was so singular that one felt that possibly an experiment was being tried and that the arrangement of the material might be only temporary and consequently a judgment of the merits of the scheme did not at the time seem justifiable. Since this gallery was thrown open to the public, however, the suspicion has become a conviction that the former ideals have been abandoned and new ones substituted. Thus it now appears that a great part of the ethnological collections are to be removed from exhibition and placed in storage, where, it is said, they will be available for students, and that in their stead will be placed on exhibition a series of type or standard or unit exhibits illustrating certain phases and areas of culture. Two such exhibits are fairly complete and are open to public inspection, namely, the Eskimo and the plains. We have then, on the one hand, the fact that the former scheme of installation has been abandoned, that by far the larger part of the material which was formerly exhibited in the halls devoted to ethnology has been or is soon to be removed and placed in storage, and on the other hand, we have in place of the old series of actual exhibits, certain systematic exhibits of which the Peruvian, the Eskimo and the plains, which are at present installed, may be regarded as typical.

First a word concerning the storage of material. Unquestionably in every institution occasions arise when it becomes necessary to withdraw from public exhibition for a longer or shorter period of time large collections. However necessary this may be, I am convinced that collections, especially those of ethnology, which are forced into retirement always suffer. There is not only the inevitable deterioration which always follows when ethnological specimens are packed away and which is always to be considered, but there is especially that loss of personal interest in such collections which can never be completely restored. The argument that such collections are always available for study is on the whole specious. As a matter of fact, at any rate in anthropology, these collections are rarely demanded for study. The reason for this is, of course, that one does not know what exists in the storage rooms; nor can a catalogue of storage material ever be of such a nature as to make such collections of any great value. When one visits a public institution like the American Museum, which from its size, wealth and position may be supposed to occupy a commanding position in American science, it is not for the purpose of finding what they have in storage but to see what they have on exhibition and to take advantage of the information which may be thus obtained. The student who desires to examine this material, having ascertained that it exists in the museum, might be reconciled to the idea of storage of the bulk of the collections if they were in glass cases, easily accessible, but it is quite impossible to reconcile the idea of study collections with the character of the storage cases which at the present time are to be found in this institution, for these storage cases are of the flimsiest material, from which it would seem impossible to exclude insects and dust and which apparently might be very easily destroyed by fire. The reasons why stored collections in ethnology lose their vital interest and deteriorate from every point of view and especially fail in the purpose for which they were made are so obvious that it does not seem necessary to dwell longer on the subject.

Of the three collections above referred to, which represent the new ideals of installation in this museum, namely, the Peruvian, Eskimo and the plains, it may be noted first that while they all have certain elements in common they are not consistent one with another, for it seems that in the two ethnological exhibits it was the intention that no duplicate specimens should be shown, whereas in the Peruvian exhibit there is endless duplication. If the Peruvian exhibit is to conform to the other two it should be reduced to one tenth its present size. It is possible, however, that this is contemplated. As this exhibit even in other respects differs from the two ethnological exhibits it may be considered independently of them. Its essential defect is the fact that nowhere in the hall is emphasized the salient features of the culture of the Peruvians, namely, that they were a sessile, agricultural people, living in permanent habitations, possessing domestic animals. On the other hand, we are introduced to such categories as 'objects in stone,' 'objects in wood,' 'objects in bone,' etc., and this fortuitous principle of classification, of course, makes it exceedingly difficult for the student to obtain any idea of the true character of Peruvian culture. It would seem that in installing this collection an ideal scheme was held in mind, that the exhibition cases were conceived of as containing compartments, and each compartment received in advance its label, and that then the attempt was made to find specimens to fit the compartments. Where this was not possible the compartment was left vacant. The absurdity of this ideal scheme of installation, if carried to a logical conclusion, can be easily imagined. It only remains to add that the scientific interest of a great and valuable collection has been almost entirely lost.

An equally absurd phase of this kind of installation is seen in the rearrangement of one section of the Chinese hall, where bronzes have been assembled in certain cases and arranged chronologically, centuries and epochs not represented in the collection being indicated by empty shelves. That these handsome dark bronzes have been placed on the darkest side of the hall is, of course, a minor detail.

Upon examining the Eskimo and plains exhibit it seems that a similar ideal scheme was conceived of and that the great collections which existed in the museum from these two regions were searched to find specimens to fill in the pockets of this scheme. The two collections differ in many ways in detail and must be considered separately. The Eskimo exhibit as it stands conceives the Eskimo as a unit and makes such differences in culture as exist, for example, between that of the Greenlander and that of the Alaskan of very secondary importance. The very fact of any difference existing between the Eskimo of the east and of the west is practically lost sight of in the exhibit, and thus also is lost the opportunity to illustrate the influence of the contact of one culture upon another. Of course, if the culture of the Eskimo is a unit, it is quite unessential that one should know what tribe or tribes lie to the south of them or how they have been influenced by these tribes. It may be pointed out next that apart from this defect the collection gives an impression of the Eskimo which is false and misleading. Prominently displayed in the center of the exhibit is an Eskimo woman fishing in the ice. Owing to its position it might be taken as representing a typical phase of Eskimo culture. Thus a false impression is conveyed, as the Eskimo are not essentially fishers but hunters of sea mammals. Taking the fisherwoman on her own merits, however, the details of the group are misleading, to say nothing of the fact that men rather than women engage in such pursuits. Of nearly equal importance, owing to its position, is a house scene, bad in detail and misleading. The scene represents a woman by a lamp, the source of light and heat in an Eskimo house, completely clad in winter furs. Near her is a baby sprawling on the floor, also clad in furs. Both figures, of course, should be practically nude. The house is constructed after the manner of a temporary habitation, which is never lined. This house is lined. If the woman and child are properly costumed then the house must be regarded as a temporary structure, in which case its lining is entirely inappropriate. The assemblage of objects about the woman is also misleading and has been made without regard for actual conditions. Thus lying on the floor near the woman, who is cooking, is a man's knife, which is entirely inappropriate, and near by is a little toy kettle, which is, of course, entirely out of place.

And so it would be possible to continue in detail an examination of this entire exhibit. Two or three other points, however, seem worthy of notice. In another case are two figures, one of a woman sewing, the other a man cutting a skin. According to the label the woman is dressed in clothing from Cumberland Sound, while her hair is dressed after the Labrador fashion. She is supposed to be talking to the man who is dressed in a costume from the west coast of Hudson Bay—a truly intertribal gathering. According to the label, the man is cutting a thong from seal skin, the label stating that it is taken from fresh seal skin. In the group, however, the man has a nicely tanned piece of common skin, which is never used for lines. The posture of the man is not only wrong, but absolutely impossible.

The case devoted to the decorative art of the Eskimo is perhaps the weakest and most trivial of the whole exhibit. The motive which determined the selection of the objects is not at all clear. Many of the specimens are such as are made for trade and possess little or no ethnological interest. Among the highly decorated objects of the Eskimo are the bone and ivory pipes of the Alaskan Eskimo. None of these are to be found in the case. There are, to be sure, several pipes in the case, but none are typical of the Eskimo, but rather of the Algonkin tribes of the south. To introduce Peary's pack sledges in an exhibition of this sort does not add to its ethnological value. To characterize this exhibit as a whole, I think the word trivial does it justice. If instead of the great collection formerly on exhibit in which one could study in detail the Eskimo from the eastern to the westernmost division we are to have a single exhibit of this nature it should be accurate at least, so far as the character of such an exhibit will allow.

The plains exhibit is perhaps on the whole less fortunate than that of the Eskimo. While the defects here are equally grave they are not so apparent. The most serious objection to this exhibit is probably the fact that the objects used in making the exhibit have been drawn from two or three tribes, especially the Blackfoot, Sioux and Arapaho, which, while they may be regarded as typical of a certain region of the plains, represent only one phase of the culture which actually exists upon the plains. These tribes as they existed in recent times are non-agricultural, buffalo-hunting, tipi-dwellers. No objection could have been made to an exhibit representing this phase of the life of the plains, but to single out the predominating features of such tribes as these is entirely to misrepresent the plains conditions. To consider a single fact, the habitations of the plains are shown by a full-sized tipi and several tiny models. Thus the inference may be drawn that no other kind of habitation prevailed in this region, whereas many other kinds of habitations were found, such, for example, as the earth lodges of the Mandan, Arikara, Omaha, Pawnee and Tonkawa, the grass lodges of the Wichita and Caddo, not to mention other types of dwellings of other tribes. Of the existence of many kinds of wind breaks, shelters, arbors, etc., no one would suspect. The life of the plains is represented as that of hunters, especially of the buffalo. One looks in vain for facts representing the sessile agricultural populations on the plains. This was once the occupation of all the tribes just enumerated and it was undoubtedly that of the other tribes until within a comparatively recent period. Turning to the exhibit itself, it seriously fails as a whole to represent adequately the wealth of the house life on the plains. To judge of this failure by a single example, the miniature tipi, thrown open so as to permit a view of its interior, may be taken. The model seems entirely inadequate and appears rather as an interesting toy. Its interior arrangement does not conform to that of the Indian tipi; the beds are represented as on the ground; the leanbacks are not in pairs, and it is equally at fault in other details. The little model consisting of two men, presumably hunters or warriors, for their bows, arrows and shields are near-by, is even more misleading. The group is either intended to represent men preparing a hasty meal while on the hunt, in which case they would not, ordinarily, boil their meat by hot stones in skin vessels, or it represents a process of cooking, in which case it would not be done by men, but by women. The section devoted to the subject of transportation presents some curious features. In the section of the human burden-bearer there is exhibited a pair of moccasins, which, by the way, are a specialized type worn only in winter by certain tribes, several cradle-boards typical of no one tribe or group of tribes, while at the top of the section is a poor specimen of a packing strap badly shown.

This plains exhibit, even more than that of the Eskimo, is inconsistent. Whereas the first section of this idealized exhibit treats the plains as a unit, in the latter sections tribal elements appear, thus introducing an amount of detail which, however valuable it may be for scientific purposes, seems out of keeping with the idea of the exhibit. I refer especially to the exhibit illustrating the warrior orders of the Arapaho and Blackfeet. Of course, it would have been possible to have maintained a logical and consistent point of view throughout this exhibit, in which case these specialized exhibits would not appear, but in their stead would be found a general treatment of the religious and social life. No such attempt, however, has been made.

Throughout this exhibit, as in the Eskimo exhibit, is everywhere manifest the result of the conscious effort to make the cases look pretty, to have the object on one's right balance in size with that on one's left, quite regardless of whether it illustrates anything or not. After all, the chief defect of this exhibit of the plains is that, in attempting to illustrate an idealized culture, it not only fails to recognize the actual conditions which exist in that culture, it not only makes it impossible to study these conditions and the reasons therefor, but it assumes that all peoples within this area had a common culture. To illustrate this by a single observation—when a buckskin shirt is shown as the costume of a man the casual visitor would assume that all the men wore buckskin shirts, presumably of this pattern, whereas many Indians on the plains wore no shirts at all. The labels here, as in the Eskimo exhibit, are prolific in number, but generally of a trivial kind, conveying information of a kindergarten nature.

Such exhibits as those above characterized might with some degree of propriety be found in the lower grades of the public school, but they certainly do not seem worthy of an institution which claims to be foremost among American museums. If exhibits of this nature are advisable, it would seem that there is no reason for the concentration under one roof of large collections. If the purpose of the general public is to be served by such exhibits it would seem desirable that the great bulk of the collections which are now being stored should be distributed among some ten or twenty of the high schools of the city, for thus the ideal of this scheme might the more easily and cheaply be realized. Or, again, this type of installation might very well be adopted in a small institution with extremely limited resources, or it might even be adopted for a single one of the great halls of the American Museum; indeed, it is conceivable that one of the halls, such as the north hall of the main floor, might very appropriately, in this institution, be given up to an exhibit, in the briefest and most concise manner possible, which would attempt to represent the great general areas of culture which exist among the different peoples of the earth. Such a hall would indeed be of great value in a great institution like the American Museum and would amply repay the labor of preparing such an exhibit, for the material for such a hall could easily be selected from the duplicate specimens without making any considerable drain on the exhibit halls proper, which should be devoted to the ethnographic exhibit. But to adopt this as the type and standard of installation for the entire department seems, when one considers the greatness of the collections and the size of the building of this institution, utterly incongruous. In view of the commanding position which this institution holds in America, its example is bound to have a very great influence on all of our public institutions, and one has the right to expect from it work of the highest scientific value, and to expect that through its exhibition halls it shall appeal primarily to the intelligent scientific world.

George A. Dorsey

THE FORMATION OF LAKE SUPERIOR COPPER

To the Editor of Science: The report of the meeting of the Geological Society of Washington and of the paper from the Geophysical Laboratory of the Carnegie Institution on the artificial production of silver and copper in your number of March 8, 1907, leads me to think I should, in justice, publish extracts from a letter received by me from Dr. G. Fernekes, of the Houghton College of Mines, left undated, but received some weeks ago. In a paper on the theory of copper deposition (annual for 1903, p. 249, etc.) emphasis was laid on the probable importance of chloride solutions in copper formations. Then when I read of Stokes's work in *Economic Geology* (Vol. I., p. 644) I suggested to Dr. Fernekes that he extend it to what seemed to me probable conditions of copper formation. He has been engaged in experiments, including an extensive series of tests of mine waters and minerals, along this line, not yet finished, concerning which I will not pretend to report in full, but just quote one extract to show the kind of results he is getting. An apparatus like that of Stokes's was used. Of course the experiment is not precisely the same as the experiment made in the Geophysical Laboratory, and there is no direct question of priority involved, but they are closely parallel and entirely independent. It is up to me to say this as a 'mutual friend.'

Alfred C. Lane

I have therefore again tried the action of $FeCl_2$ on $CuCl_2$. When these two salts react on each other in an almost neutral solution, free acid is given off according to the following reaction:

$$2FeCl_2 + CuCl_2 \rightleftarrows Cu + 2FeCl_3$$

$$FeCl_3 + 2H_2O \rightleftarrows Fe(OH)Cl + 2HCl$$

If we constantly neutralize this acid by some alkali such as $Ca(OH)_2$ we can change the above reversible reaction into one which will proceed but in one direction, namely, from left to right as above. On trying this I was pleased to see that every trace of copper was precipitated from the solution and of course calcium chloride was formed as a by-product. I immediately upon this tried calcium silicate as a neutralizing agent, and was delighted in seeing all the copper precipitated. Natural wollastonite was the calcium silicate employed. The by-product in this case was, of course, besides calcium chloride, silica (quartz). The whole thing is now cleared up. That is, three factors were active in bringing about the deposition: copper chloride (or copper silicate and HCl); calcium and sodium silicates, as neutralizing agents; and then minerals with ferrous iron in them.

* * * As to the aluminum: the same happened to it as to the iron. After all the copper was precipitated and the solution was neutral it was thrown out as an extremely basic salt. A trace of chlorine is detectable in most of the minerals around here. How beautifully we will check up with Pumpelly's observations.—The mineral now gone; and the greenstone, etc. * * *

* * * Will send you once more corrected sheet of analyses and further notes as to tests of Cu and Ni thereon.

Yours,

(Signed) G. Fernekes

RADIUM IN BIOLOGICAL RESEARCH

A Radioactive Microscopic Slide.—In the course of experiments on the effects of the rays of radium on plants it became desirable to observe directly the reaction of the living protoplast to these rays. For this purpose the principle of Lieber's radium-coating was applied in the preparation of a radioactive microscopic slide. A solution of any desired concentration of radium bromide of known activity is made in a suitable solvent, and applied to the surface of the slide near the center. When the solvent evaporates a film of the salt is left on the slide. The film is protected by a coating of a specially prepared substance. Living cells may now be mounted as on an ordinary slide, and their response, if any, to the stimulus of the rays observed. The coating has the advantage, not only of being sufficiently transparent to light, but easily transparent to the β and γ rays, and in less degree to the α rays also.

Slides of various styles and modifications have been prepared on the above principle by Mr. Hugo Lieber, of H. Lieber & Co., New York City, and their efficacy is now being tested in the laboratories of the New York Botanical Garden. Thus far only plant cells have been studied in this way, but the device could doubtless be used in studying the cleavage of eggs, and other activities of animal cells.

To Discharge Electrified Paraffin Ribbons.—Every user of the microtome has experienced the annoyance arising from the electrification of the paraffin ribbons. The trouble may be easily avoided by any device that will conduct away the charge of electricity as rapidly as it accumulates. If the air were a perfect conductor, the trouble would not arise, but its conductivity is greatly increased through ionization. This ionization may be conveniently accomplished by supporting, near the place where the microtome-knife cuts the sections, a celluloid rod, covered on one end with Lieber's radium-coating. These rods have been used with great satisfaction by the writer to avoid the difficulty mentioned.

C. Stuart Gager
New York Botanical Garden,
New York City

SPECIAL ARTICLES

ELEMENTARY SPECIES AND HYBRIDS OF BURSA[1]

The rearing of over 20,000 pedigreed specimens of *Bursa Bursa-pastoris* (L.) Britton, has demonstrated the presence of at least four elementary species, all of which breed true when self-fertilized or crossed within the limits of the same elementary form. From over thirty hybrid families the fact is derived that these several elementary forms hybridize in strictly Mendelian fashion, each form which went into the cross coming out again in the perfectly pure extracted dominant or recessive form of the parents.

The existence of elementary forms in nature within the recognized limits of the species, differing from one another as do the elementary species of *Bursa,* in the possession of definite characters that behave as hereditary units, presents a condition that is not unique, but one which has an important bearing upon some of the questions that have been recently discussed. Several atypic plants have appeared in the cultures, which have bred true to their atypic characters, when the assumption that they were due to chance crosses would have required that they split into the atypic and typic forms in the ratio 3:1. These occurred in families of which the pollination was not guarded and their status as mutants is in consequence not considered sufficiently secure to be presented in detail at present as proofs of mutation. The fact that throughout these cultures the differentiating characters behaved as units in the Mendelian sense appears to me indubitable evidence that the several elementary species have arisen through mutation and hybridization.

On the basis of allelomorphic differences between different species these elementary species of *Bursa* represent the closest possible relationship between forms belonging to distinct types, since they are seen in most cases to differ from each other by a single distinguishing unit or by two units at most. The fact that *Bursa Bursa-pastoris* is everywhere recognized as a variable species, and the presence of several distinct forms in each of the localities from which material was derived for these studies, show that these elementary species generally grow in actual contact with each other. There is neither geographical isolation nor complete physiological isolation, yet these nearly related elementary forms

[1] Extract from paper read before the joint meeting of Section F and Section G, A. A. A. S., New York City, December —, 1906.

maintain themselves absolutely distinct. This should convince any one who may still entertain any doubt regarding it that in the presence of Mendelian hybridization, no form of physical isolation is necessary for the maintenance of closely related forms. Moreover, these observations on *Bursa* show that Mendelian behavior is a strictly normal natural process and in no manner dependent upon the artificial conditions supplied by garden practise.

When a new form arises, differing from the parent in one or several unit characters, these new characters may be either dominant or recessive to the corresponding character of the parent. Less rarely they are neither dominant nor recessive. The chances of survival under these several possible conditions seem to need discussion, since, in several recent conversations, I have found the notion to prevail that recessiveness is a handicap, and allusions based upon the same idea have found their way into print. This view is quite erroneous; not only has the dominant form no advantage in the competition which the newly arisen elementary species must encounter, but it can be shown that under certain conditions the reverse is true.

If the dominant and recessive forms are equally adapted to the particular environment in which they live, there is absolutely no advantage in favor of either. The second generation of a Mendelian monohybrid contains the same number of pure recessives as of pure dominants, and the heterozygotes continue to produce in each succeeding generation just as many recessives as extracted dominants. The chances that extracted dominants will self-fertilize or that they will cross with other extracted dominants are exactly the same as the chances that recessives will self-fertilize or cross with other recessives. In like manner extracted dominants and recessives will cross with heterozygotes with equal frequency, and the quantitative results in these two cases will be exactly parallel, in one instance giving fifty per cent. of pure dominants, in the other case fifty per cent. of pure recessives. In this equal fashion the struggle will continue indefinitely so long as the premise holds, that the two forms are equally well suited to the conditions under which they must grow.

The situation is different when natural selection favors one or the other of the competing forms. A single extreme case will suffice to demonstrate: Let us suppose that the new form is dominant over its parent, but so poorly adapted to the particular habitat in which it originated that it can not successfully compete with the parent form. All the hybrid offspring resulting from crosses between the mutant and its parent will have the unadapted new form, and when the selection becomes extreme, not only will all the pure-bred specimens of the new form be destroyed, but all the hybrids as well, and in this way every vestige of the new form will be entirely lost. Assuming, on the other hand, that the mutant is recessive to its parent but that in other respects the conditions are the same as before, the extreme selection that is assumed to destroy all the recessive individuals, leaves the heterozygotes living because they have the successful form possessed by the parent species. These successful heterozygotes give rise to a progeny in the next generation including the recessive form, and also a considerable percentage of heterozygotes that may carry the form on to still another generation, and in this way the recessive mutant may be preserved indefinitely under the protection of the dominant characteristics of its more successful parent. Such prolongation of the life of a recessive may serve to tide it over times of special stress, or may continue its existence until the various distributing agents have carried it beyond the limits of the habitat in which it is a failure into others in which it may become a success.

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NEW PROCESSES OF TAKING IMPRESSIONS OF NATURAL MOLDS OF FOSSILS

ONE of the perplexing problems which confront the invertebrate paleontologist is that of finding some substance with which an accurate and permanent cast or impression of natural molds of fossils can be made. In attempting to solve this problem quite a number of substances have been tried by the writer, such as plaster, gutta percha, wax, modeling clay, etc., and when the relief is not great and other conditions are favorable, some acceptable impressions have been made. In many cases, however, the shape of the mold is such that the opening, through which the cast must be drawn, is too small to allow it to pass, if the above substances are used, or the sculpturing, on the plates of a crinoid, for instance, is so delicate that in drawing a rigid cast out of a rigid mold, the finer markings are destroyed.

The properties which a substance must possess to give a reproduction of the form of a shell over which a natural mold was formed are: (*a*) ability to become liquid or plastic to such a degree that it can be forced into every crevice of the mold; (*b*) little or no shrinkage in cooling or drying; (*c*) elasticity, to insure its resuming its original form after the distortion necessary in drawing the cast out of the mold and (*d*) durability. Glue possesses most of these properties and gives satisfactory casts for some purposes, but they shrink after a day or two. A substance found very satisfactory when the molds are large and the ornamentation not too delicate is the so-called 'roller composition,' such as is used in making the ink rollers on printing presses. This composition, which can be purchased at any printers' supply house, is used as follows: Melt the composition in a double glue pot, to avoid burning, as a comparatively high temperature is needed. Heat the natural molds as hot as they can be handled and thoroughly oil with lard oil just before the composition is put in. Keep the mold hot for five or ten minutes after the composition is put in and stir the composition to allow any air bubbles to rise to the top, otherwise the mold may not entirely fill. After the composition is quite cold it can be removed from the mold and will last for a long time without shrinking. The writer has some casts that were made in 1901 from this substance and they are still in good condition.

When the molds are small and irregular with delicate sculpturing, the writer was unable to get rid of the bubbles or to obtain sharp impressions with the composition. Unvulcanized rubber, such as is used for making rubber stamps, was tried and by vulcanizing it in the molds, under pressure, very satisfactory casts have been made on which the surface markings are perfectly preserved. The process is as follows: Dust the inside of the mold and anything that is to come in contact with the rubber with talcum powder to prevent sticking. Cut the rubber in small pieces and, after cleaning in benzine, pack it tightly into the mold, until the mold is a little more than full; then put the mold in a screw clamp to press the rubber while it is vulcanizing and insure complete contact throughout the mold. The vulcanizing is accomplished by placing the mold in a drying oven heated to 135° or 140° Centigrade. The time required varies from half an hour to an hour or more according to the size of the mold.

Arthur W. Slocom
Field Museum of Natural History

PIERRE EUGÈNE MARCELLIN BERTHELOT[1]

France, for the third time in the space of a few months, mourns the loss of an illustrious savant. Again the nations of the world extend their sympathy. The heroes acclaimed were not martial victors over mankind, whose honors were bought in the price of blood, but men of lofty ideals who conquered nature, brought truth to light, instituted new industries and improved old ones, thus bettering man's physical, and through enlightened thought elevating his moral condition. Truly 'science guides humanity.'

Curie, Moissan, Berthelot! How varied the achievements of each, though each chose chemistry as a field of labor. How differently each worked out his task and how successfully.

Berthelot was born in Paris, October 25, 1827. His father was a physician, and the young man inherited not only a taste for a scientific career, but was schooled most effectually for it. His education at the Lycée Henri IV. developed the taste for historical research

[1] Read at the meeting of the New York Section of the American Chemical Society, April 5, 1907.

which won him his first prize and which later directed his attention to the early history of alchemy, the foundations of our science. His great erudition, his mastery of the Greek language and his love of exactitude in securing fundamental facts have given us nine volumes covering the several topics of these researches. They appeared from 1885–1893 and represent his maturer years when the activities and acquisitions of middle life were subjected to the criticism of a calm judgment. This phase of Berthelot's character is also seen in his frequent minor articles dealing with questions of education, morals and philosophy. He possessed a marvelous memory. He lived in a period when the sciences were rapidly developed. He obtained an extraordinary grasp of their relationship. He lived in an environment which was stimulating. He quickly understood what was fundamental in each, and so at eighty he was one of that type of men, now growing rare because of the intense specialization of our day, known as 'the encyclopedists.'

His first scientific memoir was presented to the Académie des Sciences the twenty-seventh of May, 1850. It described the liquefaction of gases by the pressure secured by the dilatation of mercury. He found that pressure alone would not reduce gases to the liquid state. From that date there was no cessation in his labors; he attended, as its perpetual secretary, a meeting of the Académie within an hour of his death, March 18, 1907.

He became assistant to Balard at the Collége de France and obtained his doctorate in 1854 with a sensational thesis on the synthesis of natural fats from glycerin and the fatty acids. A continuation of these researches, especially on the polyatomic alcohols led, in 1863, to the founding of the chair of organic chemistry at the Collége de France, that he might have the conditions for carrying out his personal ideas. He thus entered on a field of work which made him famous. *Analysis* had until this period been the chemist's aim. *Synthesis* now claimed his attention, and before the end of the nineteenth century wonders were indeed wrought, revolutionizing both philosophy and the arts.

By causing an electric arc to play between carbon electrodes in an atmosphere of hydrogen Berthelot secured the direct union of carbon and hydrogen with the production of acetylene. He then converted this by the action of heat into benzene, and from these passed to other syntheses. He also experimented with the silent discharge turning oxygen to ozone. With the induction current he combined acetylene and nitrogen to hydrocyanic acid. He obtained formic acid starting from carbon monoxide. By the use of sealed tubes in which chemicals were subjected to high temperature and pressure through considerable time he influenced them to combine, and also gave us a new general method in chemical manipulation. Six important works, in all nine volumes, attest his genius as applied to this department of his labors. His soul was in his work. When one contemplates how his experiments steadily progressed, effecting the grouping of the elements to form hydrocarbons, alcohols, acids, ethers, sugars, fats, thus simulating natural processes and building up compounds which up to his day were conceived as being solely the result of vital force, we little wonder that he became permeated with the idea that ultimately man would manufacture his own sustenance. In his address to the second International Congress of Applied Chemistry he says: "No one can deny that the day is perhaps near when the progress of chemistry will realize the manufacture of foods; in that day the cultivation of wheat and the raising of cattle will be exposed to the same destiny which has overtaken the culture of madder in our day." What perplexing situations will then arise with reference to the pure food law!

Berthelot took an active part in the great movement of the middle of the nineteenth century when the correlation of the sciences was discussed and the conservation of energy was established as the basis principle in physics. It was, therefore, natural that he should attempt to measure the energy developed by chemical reactions in definite terms. He labored indefatigably for thirty-five years in founding thermochemistry. The facts and principles are collected in two volumes published in 1897. They had been preceded by a work entitled 'Essai de Méchanique Chimique,' also in two volumes, 1879, followed by a volume, 'Traité pratique de Calorimétrie chimique,' 1893. His two volumes 'Sur la force des matières explosives,' 1883, was intimately connected with these other laborious researches in thermochemistry, and led to the discovery by others of smokeless powder. His work on the detonation of endothermic substances, such as cyanogen and acetylene, was followed by a research on explosive waves by which he elucidated many seeming contradictory facts. During the stirring times of 1870 Berthelot was made president of the scientific committee on defense; he afterwards became consulting member of the committee on powder and saltpetre, and president of the commission on explosives. In connection with these duties he devised many original methods of research.

His thesis that chemical phenomena are identical in animate and inanimate nature is thus expounded in 1855:

> we may, I say, claim to form anew all the substances which have been developed since the origin of things, to form them under like conditions in virtue of the same laws, by the same forces which nature brings into play in their formation.

And as a necessary sequel of his life's work we find him attacking the serious problems of the theory of agriculture and of biological chemistry. His 'Chimie Agricole,' in four volumes, and his 'Chimie Animale,' in two volumes, were both published in 1899.

The beautiful experiment farm at Meudon was the scene of his labors. One climbs the 'tour Berthelot' of over eighty feet in height and about one is the charming scenery of this suburb overlooking Paris. Here the master undertook his experiments on the influence of electricity on the growth of plants—generating this force or deriving it from the atmosphere. Here it was that the fixation of nitrogen was studied, a problem that has engaged the sturdiest minds, and here it was that he found that microbic life was the means of transferring atmospheric nitrogen to the living plant cell. The import of this phenomenon he tersely stated in saying: 'The soil is something alive!' To us the products of whose broad acres furnish enough for ourselves and to spare this discovery is of incalculable value. And yet this man, who instructs us in economic farming, does not hesitate to indicate how we may manufacture our own food and thus make ourselves independent of climatic influences.

The experimental investigation on plant life led to that of the animal organism. The principles of the production of heat in living beings was a topic quite germane to the investigations on thermochemistry.

While Berthelot found his greatest pleasure in experimentation in science he was fully alert to the intimate relations his investigations bore to the advancement of the liberal arts. He made his position in this regard quite clear to the audience he addressed at the second International Congress of Applied Chemistry:

> In chemistry, as in all studies useful to man, theory and practise are related to each other by indissoluble bonds.
>
> Senseless the theorist who, shutting himself up in the solitude of his egotistical personal views, affects to disdain the incessant applications of science to civilization, for the wealth and happiness of mankind!
>
> Senseless, no less senseless, the practical man who, satisfied with the knowledge acquired by his ancestors, out of admiration for their conservatism and tradition, opposes all progress, refuses to enlarge or change the processes used in his industry, that it may remain each day in complete accord witn the newest and most advanced theory!
>
> No science probably, more than chemistry, shows the necessity of this constantly renewed harmonious relation between practise and theory.

To-day the traffic of this great city, the incessant tide of travel, the lighting of its streets and homes, is effected by the aid of electricity generated by the burning of coal, and the specifications, under which the coal is bought, require that its calorific value shall be determined by the bomb calorimeter, invention of Berthelot, devised for theoretical purposes.

While a great theorist, he invariably had recourse to the experimental method for establishing his premises on a sure foundation. His temperament was that of an idealist, of a literateur, yet he foreswore in part his allegiance to science to serve his country.

France had honored him by the bestowal of many favors in recognition of his labors. Member of the Institut, 1900, grand officer of the Legion of Honor, 1886, perpetual secretary of the Academy of Sciences, 1889, succeeding Pasteur; but she also made him senator for life, 1881, gave him the portfolio of the Minister of Public Instruction and the Fine Arts, 1886, and made him Minister of Foreign Affairs, 1895, and he served his country with ardor.

The fiftieth anniversary of his first scientific publication was celebrated at the Sorbonne on November 24, 1901. Official delegates of foreign scientific societies voiced their congratulations. The French Academy, in a stirring discourse delivered by Moissan, 'tendered him its homage and thanked him for having given it a little more of truth.' All departments of the government were represented at this unique festival. A beautiful medal, by Chaplain, bore on its face the likeness of Berthelot and the inscription, 'La Synthese Chimique. La Science Guide l'Humanité.' On the reverse side the savant appears seated before his laboratory table, on which is placed now classical apparatus, while above are two figures typifying the inscription 'Pour la Patrie et la Verité,' and the president of the republic, M. Loubet, as he handed him this gift, kissed the dear old man in token of the love and gratitude of the nation and in behalf of his admirers of all other nations.

Berthelot was particularly happy in his surroundings. He was constantly in his laboratories in Paris, Meudon and elsewhere; it was here that his *positive* science claimed him. In late years he resided in the Institut, a palace formerly occupied by Cardinal Mazarin. It was here, surrounded by his family and friends, that he enjoyed his *ideal* science.

He married early in life a beautiful and charming woman by whom he had five children, the four sons surviving. The forty-five years of married life came to a dramatic end. Both husband and wife suffered from heart trouble. Berthelot, anxious about his partner's failing health, was ever watchful. He left her to be present at the semi-monthly meeting of the academy, but returned shortly —only in time, however, to be with the beloved one in her last moments. Shattered by the blow he was led to a couch in his work room. Alas! The strain had been too great and his own heart, weakened by age and the present anguish, ceased beating.

On March 25 this noble man and woman were given public obsequies. The great Pantheon was filled with the representatives of all branches of the government from President Fallières down. The edifice was crowded with distinguished men and women. As the two bodies rested on catafalques M. Briand gave an eloquent discourse. Afterwards the body of Berthelot was placed on another catafalque before the church and the army passed in review, saluting the great dead. In the afternoon the public did him homage, and towards evening he and his dear wife were placed in the crypt, not far from the remains of Victor Hugo.

In his peroration to the second congress Berthelot summed up his views of life; he fulfilled them in his own:

> Our duty is clearly outlined. Let us be doing, that is let us work! Work without cessation; let us try to be useful. Diligence and the love of mankind! This is the true aim of both home and public life.

Charles G. Doremus

THE GEOLOGICAL SURVEY AT JAMESTOWN

Under the general direction of Mr. David T. Day, a comprehensive exhibit of the geologic, topographic, and hydrographic work of the United States Geological Survey will be made at the Jamestown Tercentenary Exposition. The geologic data will be prepared by Mr. J. S. Diller, the topographic by Mr. H. M. Wilson, and the hydrographic by Mr. M. O. Leighton.

A pillar of mounted geologic maps will be one of the most important exhibits. It will include maps from 70 of the Survey's folios, representing areas throughout the United States.

Special maps have been prepared showing the general distribution of important economic mineral products east of the Rocky Mountains. These will include maps showing the distribution of coal and iron, of oil and gas, of cements, phosphates, and glass, of gold, silver, copper, lead, and zinc. The minerals and ores whose distribution is shown on the economic maps will be illustrated by a small collection of specimens in an adjoining case.

The colored geologic map of North America, which was prepared by the Survey in cooperation with the Geological Surveys of Canada and Mexico for the International Geologic Congress, which met in Mexico last fall, will also be exhibited.

A collection of coals and other economic minerals, the distribution of which is shown on the economic maps, will be presented.

On the flat surface of a map it is difficult to show clearly the relief of the country represented, but by means of a model it can be fully expressed. For this reason a number of models have been prepared to illustrate some of the most important relief features and economic resources of the country tributary to the Jamestown Exposition. These include geologic models of the southern Appalachian region, the New River coal field, and the Philadelphia region, a topographic model of the Atlanta-Chattanooga region and a topographic and geologic model of Alaska. A collection of Alaskan minerals will be especially interesting when studied in connection with the model of Alaska.

The educational series of rock specimens prepared by the Survey for teaching geology will be exhibited. Two hundred similar collections, each containing 156 specimens illustrating the various types of rocks, have been distributed to the universities and colleges of the country.

The machine used by petrographers for grinding thin sections of rocks will be exhibited. The method of preparing thin sections and their kaleidoscopic appearance under a polarizing microscope will be illustrated.

A complete set of survey publications, including topographic and geologic maps, annual reports, monographs, professional papers, bulletins, water-supply papers, and mineral resources will be on file.

The method of storing and arranging the large number of maps and folios for convenient use in public and private libraries has been given much attention, and the best cases yet devised for the purpose are exhibited.

RESEARCH FELLOWSHIPS IN ENGINEERING AVAILABLE AT THE UNIVERSITY OF ILLINOIS

The University of Illinois has extended and strengthened the field of its graduate work in engineering by recently establishing ten Research Fellowships in the Engineering Experiment Station. These fellowships have an annual value of $500, and are open to graduates of approved universities and technical schools, both American and foreign. They must be accepted for two consecutive collegiate years, at the expiration of which period, if all requirements have been met, the Master's degree will be granted. Preference will be given to men who have had some experience in practical engineering work outside of college. The appointments will be made upon the recommendation of the Station Staff of the Engineering Experiment Station, and upon the approval of the Faculty of the Graduate School and the President of the University.

The Engineering Experiment Station, it may be explained, is a department connected with the College of Engineering. It was established in 1903 for the purpose of carrying on investigations along various lines of engineering, and for the study of problems of importance to professional engineers and to the manufacturing and industrial interests of the state. The work of the station and the college is closely related, the heads of the several departments of the college of engineering constituting the station staff. The investigations are carried on by the members of the staff directly, sometimes by a fellow as graduate work, sometimes by a member of the instructional force of the college, and frequently by special investigators belonging to the station corps.

The various laboratories of the station and the college offer exceptional facilities for investigational work, being well-equipped with the most modern apparatus. During the past four years about $300,000 has been appropriated by the state legislature for the maintenance and extension of this equipment, and it is believed that the same liberal policy will be continued.

By offering these research fellowships at $500, and throwing them open to graduates of both American and foreign universities, the station hopes to secure a picked body of men imbued with the true spirit of genuine investigators who will do graduate work of high grade. It is expected that valuable results will accrue to the station, and that a body of experts will be developed, some of whom may be attached later to the regular corps of station investigators. A circular giving full information will soon be issued, and can be obtained upon application to the director of the Engineering Experiment Station, Urbana, Illinois.

L. P. Breckenridge,
Director of the Engineering Experiment Station.
University of Illinois,
Urbana, Ill.,
February 23, 1907

THE CARNEGIE INSTITUTE

Mr. Andrew Carnegie on April 6 notified the president and board of trustees of the Pittsburg Carnegie Institute that he was sending them $6,000,000. Of this amount $5,000,000 is United States Steel Corporation 5 per cent. bonds and $1,000,000 in cash. The cash is to be used in erecting more technical school buildings. The bonds are an endowment. One million dollars is to be added to the endowment of $2,000,000 already given the schools, and the other $4,000,000 is to be added to the institute's present $2,000,000 endowment.

In his letter, as published in the daily papers, Mr. Carnegie says:

> The director and teachers of the Technical Schools participate in the pension fund established by me for the advancement of learning, and this should be availed of. Those of the other departments do not. A pension system is, therefore, to be established for them out of the endowment fund; after the death of the recipient the pension to be continued to the widow in all cases where needed.
>
> I desire gratefully to acknowledge my unpayable indebtedness to yourself and the trustees for services which have resulted in such complete triumph. My highest hopes will be realized if the future yields such golden harvest as the past.

As we have already announced, the dedication of the new building of the institute, erected by Mr. Carnegie at the cost of $5,000,000, will take place on the afternoon of April 11, and will be attended by many distinguished guests from Europe and the United States. The program which has been arranged in connection with the ceremonies will last for three days. In addition to a reception and inspection of the buildings on Thursday morning and a concert in the evening, there will be on Friday a reception at the Carnegie Technical Schools, a presentation of addresses, and in the afternoon speeches by distinguished guests and in the evening a dinner by the trustees in honor of Mr. and Mrs. Carnegie and invited guests. On Saturday morning honorary degrees will be conferred on foreign guests by the Western University of Pennsylvania, and in the afternoon there will be an excursion on the river and a visit to the Homestead Steel works of the Carnegie Steel Company.

SCIENTIFIC NOTES AND NEWS

The spring meeting of the council of the American Association for the Advancement of Science will be held in the Assembly Hall of the Cosmos Club, Washington, D. C., on the afternoon of April 17, 1907, at 4:45 p.m.

The annual session of the National Academy of Sciences will be held in Washington, D. C., beginning on Tuesday, April 16, at 11 a.m. The place of meeting will be the National Museum.

The American Philosophical Society will hold its annual meeting at Philadelphia on April 18, 19 and 20.

Dr. George Otis Smith has been appointed director of the U. S. Geological Survey to fill the vacancy caused by the election of Dr. Charles D. Walcott to the secretaryship of the Smithsonian Institution. Dr. Smith received the bachelor of arts degree from Colby College in 1893, and the doctorate of philosophy from the Johns Hopkins University in 1906, in which year he was appointed assistant geologist to the Geological Survey, being made geologist in 1901. He has had charge of the geological work in New England, and is now geologist in charge of petrology.

On March 13, a banquet was tendered to Dr. Charles D. Walcott, by the members of the U. S. Geological Survey, on the occasion of his resignation as director to become secretary of the Smithsonian Institution. Two hundred and forty-four persons were present. Colonel H. C. Rizer presided, and addresses were made by the following men: Mr. Bailey Willis for the geologic branch, Mr. W. M. Beaman for the topographic branch, Mr. M. O. Leighton for the water resources branch, Mr. S. J. Kübel for the division of engraving, and Mr. F. H. Newell for the reclamation service. Dr. Charles B. Dudley spoke of the fuel-testing work of the Geological Survey. A letter from Mr. Arnold Hague was read, as also a telegram from Mr. Henry Gannett. Mr. Gifford Pinchot paid a tribute to Mr. Walcott in relation to the forestry work of the government. The closing address was by the Hon. James R. Garfield, secretary of the interior.

Lord Lister celebrated his eightieth birthday on April 5. Congratulations were received from all parts of the world, and a committee acquainted him with the plan to republish his scientific papers.

M. Jules Tannery, professor of differential and integral calculus at the Sorbonne, has been elected a member of the Paris Academy of Sciences.

Professor J. Burkitt Webb, of the chair of mathematics and mechanics, and Professor William E. Geyer, of the chair of physics, of the Stevens Institute of Technology, will retire from active service at the close of the present academic year.

Professor W. O. Crosby, in charge of the work in economic geology in the Massachusetts Institute of Technology, will retire from active teaching at the end of this year on the Carnegie Foundation for the Advancement of Teaching. As student and teacher, Professor Crosby has been connected with the Institute of Technology continuously for a period of thirty-six years, and the connection is to continue; but the time heretofore given to routine instruction will now be devoted to research.

Officers of the Michigan Academy of Sciences have been elected as follows: *President,* Mark S. W. Jefferson, State Normal School, Ypsilanti; *secretary and treasurer,* E. E. Bogue, Michigan Agricultural College; *assistant secretary,* Walter G. Sackett, Michigan Agricultural College; *librarian,* G. P. Burns, Ann Arbor; *vice-president, botany section,* W. E. Praeger, Kalamazoo; *zoology,* A. G. Ruthven, Ann Arbor; *geology,* Professor E. H. Krauss, Ann Arbor; *sanitary science,* J. G. Cummings, Ann Arbor.

Professor W. M. Davis, of the department of geology and geography and dean of the graduate school, will represent Harvard University at the dedication of the new building of the Carnegie Institute in Pittsburg.

Professor Carl Hess, of Würzburg, will be a guest of the section on ophthalmology of the American Medical Association at the Atlantic City meeting in June. He comes in April to deliver lectures before the universities of Pennsylvania, Chicago and New York.

Dr. Herbert J. Webber, chief of the Division of Plant Breeding Investigations of the Department of Agriculture, assumed last week the duties of the chair of plant biology at Cornell University.

Dr. J. W. Blankinship, recently professor of botany at the Montana Agricultural College, is now connected with the Missouri Botanical Garden, St. Louis, Mo.

Mr. C. W. Gilmore, of the National Museum, under a grant just approved by the secretary of the Smithsonian Institution, will be sent to Alaska to undertake paleontological explorations with a special view to securing specimens of fossil mammals. Continuing his work over two seasons, 1907 and 1908, he will confine his explorations mainly to the Yukon Basin region southeast of Norton Sound, and to the Buckland River region southeast of Kotzebue Sound. Mr. Madren, who was Mr.

Gilmore's predecessor in the Alaskan field, found many remains of mammoth, large bison and horses of a peculiar type.

Professors R. T. Jackson and J. B. Woodworth, of the department of geology and geography of Harvard University, will conduct a paleontological and geological excursion to Yorktown, Va., during the April recess, leaving Boston on Friday night, April 12.

Dr. Schilling, head of the department of tropical hygiene in the Berlin Institute of Infectious Diseases, has been granted by the government eight months leave of absence in order that he may pursue researches on immunization against tsetse fly disease. In the course of his work he will make an expedition to the Congo.

Dr. Joseph Larmor, Lucasian professor of mathematics at Cambridge University and secretary of the Royal Society, presented an address before the Washington Academy of Sciences, on April 2, 1907, on 'Modern Views of the Ultimate Structure of Matter.' It was discussed by Professor F. W. Clarke, of the U. S. Geological Survey, and Professor A. G. Webster, of Clark University.

Mr. Elihu Root, Secretary of State, has accepted the invitation of Yale University to deliver the annual Dodge course of lectures on 'The Responsibilities of Citizenship.' He will speak on May 13, 14, 20 and 21.

Dr. L. A. Bauer gave an address, illustrated by lantern slides, before the Society of Arts of the Massachusetts Institute of Technology at Boston on March 28 on 'Recent Results of Terrestrial Magnetic Observations.'

Lord Avebury will preside at the annual *soirée* of the Selborne Society, which will be held in the halls of the Civil Service Commission on April 26. Illustrated addresses will be given, and there will be a display of microscopes and objects of interest.

M. Edouard Hospitalier, professor of electrotechnic at Paris, has died at the age of fifty-four years.

The death is also announced of Dr. Rudolf Aderhold, director of the Biological Institute for Agriculture and Forestry at Berlin, at the age of forty-two years.

On May 8, there will be civil service examination for the positions of laboratory assistant and assistant physicist and laboratory assistant and assistant chemist in the Bureau of Standards, the salaries varying from $900 to $1,600.

The third regular meeting of the Botanists of the Central States was held at the University of Wisconsin, Madison, Wisconsin, March 28–30. There was a representative attendance of botanists at the meeting, and a number of papers were read. One session was devoted to the demonstration of microscopic preparations. Professor F. C. Newcombe, of the University of Michigan, the retiring president read an address entitled 'A Need in Botanical Science in America.' Professor T. H. Macbride, of the University of Iowa, was elected president for the ensuing year.

The seventy-ninth Congress of German Men of Science and Physicians will be held at Dresden this year from September 15 to 21.

Under the auspices of the University of Illinois, a commission has been organized for the purpose of conducting experiments on the effect on the consumer of the preservatives in common use in food stuffs, especially meats. The work will be done under the direction of Professor H. S. Grindley, of the department of physiological chemistry of the university, and with him on the commission are R. H. Chittenden, professor of physiological chemistry, Yale University; J. J. Abel, professor of pharmacology, Johns Hopkins University, and A. P. Mathews, professor of physiological chemistry, University of Chicago. The expense of the investigations is to be borne in part bv the university and in part by some of the beef-packing houses of Chicago.

Mr. Frank Leverett and other members of the United States Geological Survey are authors of two papers on flowing wells and municipal water supplies of the southern peninsula of Michigan. The results of their studies, so far as they apply to the southern counties, are embodied in Water-Supply and Irrigation Paper No. 182; so far as they apply to the middle and northern counties, in Water-Supply and Irrigation Paper No. 183, both of which are free publications. In the course of glacial investigations made under the direction of Professor T. C. Chamberlin during the last five years, Mr. Leverett collected a large amount of data on water supplies. These investigations resulted in a partial acquaintance with conditions in about 200 separate flowing-well districts and brought out matters of such exceptional importance that arrangements were made to examine each of the flowing-well districts in the state sufficiently to determine its essential characteristics, present state of development, and probable capacity for future development. It was arranged also that attention should be given to the quality of various classes of waters, both surface and underground, as well as water supplies of the cities and villages. By special arrangement with Dr. A. C. Lane, the state geologist, the large amount of material which had accumulated at his office relative to other classes of water supply was embodied in these reports.

UNIVERSITY AND EDUCATIONAL NEWS

By the will of Mr. William C. Eggleston, Yale University has received $100,000 as an additional endowment for the library. By the Ross legacy the university has $75,000 for maintenance in addition to the sum of about $200,000 used for the building. Yale University has also received about $95,000 as residuary legatee, after the death of his widow, of the late William L. McLane.

Mr. Benjamin Thaw, of Pittsburg, has given $50,000 to the fund for the new building of the Western University of Pennsylvania.

We regret to learn that the Macdonald engineering building of McGill University was completely destroyed by fire on April 5, involving a loss of about $750,000, of which $450,000 is covered by insurance.

The College of the City of New York will celebrate the sixtieth anniversary of its foundation on May 7.

The University of Iowa will this year celebrate the sixtieth anniversary of its foundation. Secretary Taft will give the commencement address on June 12.

The University of Berlin will celebrate the centenary of its foundation in the autumn of 1910. As part of the ceremonies a monument will be unveiled in memory of its first rector, Johann Gottlieb Fichte, the philosopher.

The Divinity School of Yale University has proposed changes in its curriculum, its scope and its relations to other departments which have been adopted by the corporation. The new curriculum provides, in addition to the regularly recognized courses of theological study, two other main groups of electives in which the study of Hebrew is not required for the degree of B.D. Sciences and philosophy are to be specialized in one of these groups of study and elementary law and sociology in the other. These subjects will be pursued by the divinity students for the most part in other departments of the university.

Professor B. E. Fernow, lately called to the professorship of forestry at State College, Pennsylvania, which he had temporarily accepted, has been appointed dean of the Faculty of Forestry at the University of Toronto. It is proposed to organize the most complete system of forestry education on this continent, the university undergoing now a reorganization and broadening of its policy generally. It is expected that a large forest reservation will be set aside for use of the school, and university extension work of the broadest kind is contemplated.

At Teachers College, Columbia University, Dr. Maurice A. Bigelow has been promoted from adjunct professor to professor of biology.

The vacancy in the chair of biology in Lake Forest College, caused by the removal of Professor James G. Needham to Cornell University, has been filled by the election of Dr. Cornelius Betten, previously assistant professor in the same department.

Mr. W. Muir Edwards, lecturer in mathematics and civil engineering at McGill University, has been appointed assistant professor of civil engineering.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, APRIL 19, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE SANITARY ENGINEERING PROBLEMS OF WATER SUPPLY AND SEWAGE DISPOSAL IN NEW YORK CITY[1]

THE Section on Public Health of the New York Academy of Medicine is formed at a peculiarly opportune time. Never before has sanitary information of a reliable, authentic character been so much desired by the public, nor so difficult for the public to obtain.

Our great universities have, for the most part, failed to recognize the vast popular and educational value which would attach to the establishment of adequate facilities for teaching sanitary science, hygiene, public health or preventive medicine, as that body of knowledge which relates to the prevention of disease is variously termed, and have left this kind of teaching largely to the newspapers and to the general practitioner of medicine. Unfortunately, physicians do not always appreciate their importance as sanitary teachers.

It is in consequence of this that vast stores of scientific facts which are being constantly collected, and which bear upon the causes and ways of preventing disease,

[1] Address delivered at the opening of a Section on Public Health of the New York Academy of Medicine, January 8, 1907.

are locked up in severely technical journals or brought out, often with an entirely mistaken interpretation, in the public press.

Aside from the collection of new sanitary facts, therefore, the members of this section can perform an extremely valuable service in assimilating the data made available by scientists and other busy workers and help to mold public opinion toward a proper consideration of the endless number of topics which relate to the public health.

The beneficial fruits of these labors will certainly be far-reaching. It has been well said that the eyes of the whole country are upon the metropolis. To a considerable extent what is found to be good here is likely to be thought desirable elsewhere.

At the initiation of this section, it may be well to take a brief glance at some of the larger sanitary engineering problems which now concern New York and consider how, in view of present and future circumstances, these problems should be studied.

We have in New York a singularly good example of a city of the largest class, wherein the highest requirements of sanitation are demanded and are, at the same time, capable of being satisfied. The population is not only great; it is concentrated, and in race, habit and social condition, exceedingly diverse. Practically all of the conditions necessary to maintain life in a wholesome way have to be secured through a most careful observance of sanitary rules and principles. This relates not only to the food, clothing and habitations of the people, but in a peculiar degree to the care of their wastes and the wastes of those who have to do with the city's food and drink. Upon the prompt and adequate disposal of these wastes largely depends the security of the city against disease.

These, in the briefest terms, appear to be the necessities of the present. What the exactions of the future will be, when more refined standards of hygiene are established and the public sense of decency and morality becomes correspondingly elevated, it is impossible to say. It is evident that the subjects which are to concern our future guardians of public health are not to be related solely to the more obvious causes of disease.

Thus far, in the history of sanitation, the great strides of progress have usually resulted from emergencies, most of which have pointed in a striking manner to the fact that the grosser human wastes were not being properly dealt with. Unfortunately this method of progress still prevails to a great extent through the country, as witness the large quantities of filth of all kinds which accumulate in our northern villages and cities through the winter and the epidemics of typhoid fever which occur every year.

Sanitary emergencies, such, for example, as infected water supplies, capable of producing epidemics, now rarely occur in our largest centers of population and are no longer to be expected in the city of New York, which rightfully boasts one of the most efficient health administrations known anywhere.

Sanitation in cities of this class now and in future may be expected to progress along more scientific and conservative lines. The conditions to be avoided must be discovered and corrected as far as possible before they result in nuisance or disease. Large schemes for sanitary improvement must be made and made after careful investigation and preparation while yet there is ample time.

Two large sanitary engineering problems which now confront the city of New York are being studied in this manner, and as they well illustrate what is meant by these remarks, they will be briefly referred to.

The water supply is being enlarged.

Competent authorities have studied the matter exhaustively and decided upon what it is best to do. This problem has now entered upon its second stage, that of construction.

The project is, as you know, to enlarge the supply of all the boroughs of the city by bringing water from the Catskill Mountains. The quantity to be delivered will be from 80,000,000 to 150,000,000 gallons per twenty-four hours at first, and will probably reach 500,000,000 gallons, or more, in time. It is estimated that this, with the present sources of supply, should be enough to meet the needs of the increasing population until 1925. The present supply of Croton, which is consumed in Manhattan and Bronx, is about 292,000,000 gallows per day with a per day increase each year of 14,000,000 gallons, as shown by the records of consumption for the last ten years.

The quality of the new water will be superior to that of the Croton. It will be softer to begin with, and will be filtered through slow sand filter beds, located near White Plains, such as have been extensively used in various parts of Europe and America for many years. It is practically certain that the Croton supply will be filtered in the same manner.

Although a part of the new water will be available for the boroughs of Brooklyn and Staten Island, it is considered highly desirable that Brooklyn, if possible, should avail itself of supplies now stoied in the sands of Long Island east of the present sources of supply.

As pointed out by the mayor, in his message of January 7, 1907, since the New York Board of Water Supply was appointed in June, 1905, remarkable progress has been made by its engineers in the preliminary work necessary to construction. About 40 per cent. of the line of the principal aqueduct of 86 miles from the Catskills to what is known as the Hill View reservoir, located near Yonkers, has been located, as has the site of the dam for the great Ashokan reservoir and the 10,000 acres of the reservoir itself. About 15 per cent. of this aqueduct has been prepared for contract. To accomplish this result, 550 miles of surveys and 12 miles of subsurface borings have been made.

At this rate of progress, it would not be surprising if water from this new source would be available considerably within the eight years allowed by the engineers.

As pointed out by the mayor, the new water supply is to cost over $160,000,000 and it is highly desirable that the taxpayers should understand the benefits to accrue from it.

The need of this work did not arise from any emergency. No epidemic pointed to its necessity. The work is being carried out largely in anticipation of the needs of the future, as pointed out with infallible accuracy by the teachings of sanitary science.

The problem of disposing of the sewage of New York and neighboring municipalities so that it shall not create a nuisance, or in other ways interfere with health, comfort or convenience has been the subject of official study for three years and is likely to continue to be investigated for several years to come. And the question here is not so much to improve present conditions, although this object may be accomplished in the end, as to protect our tidal waters against the vastly increasing pollution of the future.

Hitherto there has been no question as to the efficacy of the method of sewage disposal pursued by New York and its neighboring municipalities. House sewage and street washings have been discharged, without purification of any kind, into the nearest tide waters. Recently, communities remote from the shore have joined together to bring their combined sewage through miles of sewers to the bay. One of these projects is unprecedented in the quantity of sewage to be carried. By a curious coincidence, the contemplated point of discharge is near the statue of Liberty Enlightening the World.

To say the least, it is disquieting to contemplate the discharge of so much potential danger into the waters which flow by our doors; which so many of us cross and recross daily; which is the scene of many of our most imposing national and municipal pageants; where some of us bathe—and many of us get our oysters.

If the wastes are rendered innocuous, they are destroyed in ways which are not understood. Our knowledge of the fate of the sewage of New York may be said to extend no farther than the outfalls of the sewers.

It is unwise to count blindly upon the purifying action of sea water and the tides, for to what extent the flow of the ocean in and about the great rivers and harbors which intersect the metropolitan district transports and renders innocuous the five hundred million gallons of dangerous matters which are discharged into them every day, it is impossible to say.

Perhaps the sewage is flushed out to sea; perhaps it is consumed by minute animals and plants; perhaps some of it is turned into gas, some into liquids, some oxidized or burnt up by the nitrifying bacteria in the water. Perhaps much of it is stored in pockets and sludge banks until freshets in the Hudson flush it out to sea. We do not know what becomes of it.

Obviously, the harbor, as a whole, has a large digestive capacity for sewage, but it would be curious, indeed, if that capacity had no limit. There are few persons who have been actively interested in studying this problem who do not consider that eventually some other method of sewage disposal than the present one will be necessary for a large part of the Metropolitan District. It is only a question of time. How long, nobody knows.

These two questions, the supply of pure water and the disposal of this water after it has been turned into sewage are sanitary problems of the largest kind. The estimated cost of constructing the new water works of New York exceeds the estimated cost of building the Panama Canal. If it becomes necessary to collect and purify all of the sewage of the metropolitan district, it may be a costlier task still.

The highest skill, wisdom and efficiency are none too great to enlist in devising safe and suitable works of such magnitude. The sciences of pathology, chemistry, biology, physics, meteorology and mechanics must contribute generously to the fund of information necessary in order that the plans may be brought to that high point of perfection which engineers characterize as 'necessary and sufficient' in their works.

And there is another consideration which has, so far, received little thought, but which must be taken into account in dealing with the sewage disposal problem. For work to be done at all, it must be done within permissible limits of cost. The charter of New York, which intentionally omits to restrict expenditures for water supply, confines the cost of sewerage and sewage disposal to within the constitutional debt limits of the city.

In thus giving emphasis to two of the problems which New York is attempting to solve, it is not intended to draw attention from other sanitary engineering problems, some of which are of almost equal prominence.

The double problem of cleaning the streets and disposing of the wastes so collected is one of the greatest magnitude. It costs the city over $6,000,000 per year to maintain the department of street cleaning. In no other comparable city in the civilized world is this question in such unsatisfactory shape or so difficult to cope with, under the practical conditions which exist, as in the metropolitan district which we are considering.

The time will come when New York City will insist upon clean streets and find a way to have them. Eventually the public will demand that the refuse from our tables, kitchens and factories shall be disposed of at a minimum of offense and a maximum of economy and despatch. But until this problem is made the subject of competent study and a broad, comprehensive plan of administration and procedure is laid down, we may expect slow improvement in the primitive methods which have always been an offense to the eyes and nose in New York City.

The solution of this problem is probably far beyond the unaided capacity of any person who may be placed at the head of the street-cleaning department, and these remarks, therefore, reflect in no wise upon the ability of any official of the city, past, present or future. If it can be solved at all, and there is a very general impression that it can, the problem can be solved only as the other great sanitary engineering problems of New York have been, and are being, solved. That is, with the help of qualified experts, acting without prejudice, political bias or other ambition than to serve the best interests of the city.

GEORGE A. SOPER

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE SECTION A—MATHEMATICS AND ASTRONOMY

Vice-president—Dr. Edward Kasner, Columbia University, New York City.

Secretary—Professor L. G. Weld, State University of Iowa, Iowa City, Iowa.

Member of the Council—Professor G. B. Halsted, State Normal College, Greeley, Colorado.

Sectional Committee—Dr. Edward Kasner, vice-president, 1907; Dr. W. S. Eichelberger, vice-president, 1906; Professor L. G. Weld, secretary, 1904–1908; Professor Ormond Stone, one year; Professor E. B. Frost, two years; Professor E. O. Lovett, three years; Professor Harris Hancock, four years; Professor A. N. Skinner, five years.

Member of the General Committee—Professor James McMahon, Cornell University, Ithaca, N. Y.

Press Secretary—The secretary of the section.

Professor E. O. Lovett, of Princeton University, was elected vice-president for the year 1908.

The following mathematicians and astronomers were elected to fellowship in the association:

Baker, R. H.,	Maclay, James,
Brown, G. L.,	Manning, H. P.,
Dugan, R. S.,	Olds, G. D.,
Faught, J. B.,	Plimpton, G. A.,
Gates, Fannie C.,	Poor, C. L.,
Glenn, O. E.,	Riggs, N. C.,
Graham, W. J.,	Schultz, L. G.,
Granville, W. A.,	Smith, F. H.,
Hadley, S. M.,	Washburne, A. C.,
Leavitt, Henrietta L.,	Wilson, N. R.,
Lowell, Percival,	Young, Anna S.

The address of the retiring vice-president, Dr. W. S. Eichelberger, entitled 'Clocks, Ancient and Modern,' was presented on the afternoon of Thursday, December 27, in Fayerweather Hall of Columbia University. This address has already been published in SCIENCE for March 22 of the current year.

A joint session of Section A with the American Mathematical Society and the Astronomical and Astrophysical Society of America was held on Friday forenoon, December 28, in Schermerhorn Hall. The chair was occupied by Professor Simon Newcomb, past president of each of the participating societies. This was perhaps the most largely attended and the most generally interesting of any of the meetings in which any of the participating societies had a part. The following program was presented: numbers 1 and 5 being contributed by the Mathematical Society, 2 and 4 by the Astronomical and Astrophysical Society, 3, 6 and 7 by Section A.

1. *The Rational Basis of Mathematical Pedagogy:* Professor S. E. Slocum, University of Cincinnati.
2. *Photographic Observations of the Milky Way:* Professor E. E. Barnard, Yerkes Observatory.
3. *The Stream Function for a Straight Channel with a Circular Island:* Professor James McMahon.
4. *The Tenth Satellite of Saturn:* Professor W. H. Pickering, Harvard University.
5. *On the Law of Gravitation in the Binary Systems:* Dr. F. L. Griffin, Williams College.
6. *Latitude Terms of Long Period:* Professor C. L. Doolittle.
7. *Dynamical Trajectories:* Dr. Edward Kasner.

Abstracts of 1 and 5 of the above papers appear in the *Bulletin of the American Mathematical Society,* Vol. VIII., pp. 265, 266; of 2, 4 and 6, in the report of the eighth annual meeting of the Astronomical and Astrophysical Society of America, in Science for April 12. The others will be further noticed below.

The full list of papers appearing upon the program of Section A, with such abstracts of the same as are available, is as follows:

An Examination of the Results of Seven Years' Observation with the Zenith Telescope of the Flower Observatory for Latitude Terms of Long Period: Professor C. L. Doolittle, University of Pennsylvania, Philadelphia.

A Preliminary Report on a Solar Rotative Period Investigation: Mr. Philip Fox, Yerkes Observatory, Williams Bay, Wis.

The preliminary investigation of the solar rotation period which was made, under Mr. Hale's direction, by measuring calcium flocculi positions on the Kenwood series of spectroheliograms (Science, N. S., XXI., 175), is now being supplemented by a reduction of the measurements of the plates made with the Rumford Spectroheliograph. One hundred of these plates obtained in 1904 give the following results:

ϕ	ξ	Rumford Period	Kenwood Period	$R-K$
° °	°	d	d	d
0 5	14.50	24.82	24.56	+0.26
5 10	14.44	24.93	24.79	0.14
10 15	14.18	25.38	25.02	0.36
15 20	13.92	25.86	25.26	0.60
20 25	13.68	26.32	25.45	+0.87
25 30	13.95	25.80	25.99	—0.19
30 35	13.68	26.31	26.31	0.00
35 40	13.25	27.18		

The periods are thus seen to have been longer in 1904 than in 1893–4–5, the period covered by the Kenwood series. The plates of 1905 and 1906 are about to be measured.

The Retrograde Motion of Phœbe: Mr. A. O. Granger, Philadelphia, Pa. (Read by title.)

The Sect-carrier and the Set-sect: Professor G. B. Halsted, State Normal College, Greeley, Colo.

The school of Plato fixed as the instruments for the solution of geometric problems, the ruler and compasses, the straight line and circle. As in Euclidean geometry the straight line is a circle (of infinite radius) and as Euclid unconsciously made in his very first proposition the 'assumption of the compasses,' that "If a circle have a point within and a point without another circle, it has two points on this other" (Halsted, 'R. Geom.,' VI., 2), the world has had to await the coming of the non-Euclidean geometry to become conscious of the fact that elementary geometry has been carrying a wholly unnecessary 'rider.'

The compasses may be superseded by the simpler 'transferrer of line-segments,' for which the name 'sect-carrier' has been adopted. Thus without the circle or compasses all the problems of elementary geometry are solved in the first edition of Halsted's 'Rational Geometry.' But a remarkable additional simplification has now been achieved, and this paper makes public for the first time the simple demonstration which makes it available for the elements of geometry. This advance is the substitution of the set-sect for the sect-carrier. The transference of only a single sect need be assumed for the solution of all the problems of elementary geometry. Consequently the power to take a centimeter on a given straight line is found to be assumption enough to supersede the circle, the compasses, and even the sect-carrier. Nothing now is needed but a ruler and a set-sect.

On a Fundamental Theorem of Weierstrass by Means of which the Theory of Elliptic Functions may be Established: Professor HARRIS HANCOCK, University of Cincinnati, Cincinnati, O.

The theorem in question is stated by Weierstrass in the 'Theorie der Abelschen Functionen' (*Crelle's Journ.*, bd. 52, § 7; and 'Math. Werke,' bd. I., p. 349).

By means of his theorem it may be shown that the p-function may be expressed as the quotient of two series which are both convergent for all values of the variable; the same is true of the functions

$$\sqrt{p\omega - e_\lambda} \qquad (\lambda = 1, 2, 3).$$

It follows directly from Weierstrass's theorem that the σ-function may be expressed as a convergent series for all values of the variable.

The different series are calculated and it is interesting to compare the results usually obtained from the well-known theorem also due to Weierstrass, that *every one-valued function that has not an essential singularity in the finite portion of the plane, may be expressed through the quotient of two power-series, which are convergent for all values of the variable.*

Weierstrass's theorem is also generalized and applied to differential equations of a higher order.

Dynamical Trajectories: Dr. EDWARD KASNER, Columbia University, New York City.

Professor Kasner discusses two general questions, of interest in connection with celestial mechanics, relating to the geometry of dynamical trajectories. The first is suggested by the problem of binary stars and Bertrand's discussion of the interdependence of Kepler's laws. It is shown that two distinct fields of force can have only a certain multiplicity of trajectories in common. It is then possible to determine a field from a minimum number of trajectories. In particular, the Newtonian law may be deduced without assuming, as Bertrand does, that all the orbits are conics.

The second part of the paper relates to the problem of n bodies, and extends some of the results which hold for a single particle (see *Trans. Amer. Math. Soc.*, 1906, 1907). For example, the locus of the centers of the osculating spheres, under prescribed initial conditions, is a cubic curve; in the case of a single particle, on the other hand, it is a straight line. The results obtained are true for all interacting particles.

The Stream Function for a Straight Channel with a Circular Island: JAMES MCMAHON, Cornell University, Ithaca, N. Y.

This is one of the standing problems in two-dimensional fluid motion. A solution is here obtained by imagining a doublet placed mid-stream in a uniform current so that the line from the source to the adjoining sink points in the direction of the undisturbed current. The appropriate stream function is determined to suit the boundary conditions, by the image-method; and it is shown that one of the stream lines breaks up into the median line of the channel and a symmetrical oval. The strength of the doublet can be so adjusted that this oval does not differ appreciably from a given circle when the latter does not occupy more than half the breadth of the channel.

Preliminary Wave-lengths of Flash Spectra taken in Spain, August 30, 1905: Dr. S. A. MITCHELL, Columbia University, New York City.

The wave-lengths were obtained from photographs taken by the writer while a member of the United States Eclipse Expedition. The spectrograph was a four-inch grating of 14,438 lines per inch ruled on a parabolic surface, which was used without slit. Weather conditions were splendid. The photographs are remarkable for their splendid detail throughout their whole length from D_3 to 3,300. There are about five thousand measurable lines in this region. The dispersion of the grating is about the same as for the 'Bruce three' spectrograph of the Yerkes Observatory and the Mills spectrograph of the Lick Observatory, the distance from D_3 to H being seven inches.

On the Minimum Number of Operators whose Orders exceed Two in any Finite Group: Dr. G. A. MILLER, University of Illinois, Urbana, Ill.

When just half of the operators of a group are of order 2 the order of the group is twice an odd number, and all the operators of odd order together with the identity constitute an abelian subgroup whose order is half the order of the group. Professor Miller's paper has for its main object the proof of the following theorems: If the order (g) of a group is written in the form

$$2^{a_0}p_1^{a_1}p_2^{a_2}\cdots p_\lambda^{a_\lambda},$$

$p_1, p_2, \ldots, p_\lambda$ being distinct odd prime numbers and $a_0 > 0$, the number of the operators whose orders exceed 2 can not be less than

$$\frac{(p_1^{a_1}p_2^{a_2}\cdots p_\lambda^{a_\lambda}-1)g}{2p_1^{a_1}p_2^{a_2}\cdots p_\lambda^{a_\lambda}}.$$

Moreover, it is possible to construct a group in which the number of operators whose orders exceed 2 is exactly equal to this number. If a group of order g contains the smallest possible number of operators whose orders exceed 2, the sub-group which is composed of all its operators which are commutative with one of the non-invariant operators of order 2 contains no operator whose order exceeds 2. This sub-group is a Sylow sub-group and just half of the remaining operators are of order 2.

Results of Physical Observations on the Saturnian System with the 18-inch Clark Refractor: Professor DAVID TODD, Amherst College, Amherst, Mass. (Presented by title.)

With the exception of those papers which appear upon the joint program of Friday morning, as given above, the papers of Section A were presented in connection with the program either of the Mathematical or of the Astronomical Society, according to the subject matter treated in each case. This arrangement was made in accordance with a resolution adopted at the Ithaca meeting to the effect that 'the sectional committee be empowered to turn over technical papers to the technical societies.'

LAENAS GIFFORD WELD,
Secretary

THE ASTRONOMICAL AND ASTROPHYSICAL SOCIETY OF AMERICA

II

A New Form of Meridian Mark: G. W. HOUGH.

Two years ago I established a meridian mark in order to study the change of azimuth for the 6⅜-inch Repsold meridian circle. Collimator marks, according to published statistics, as well as from theory, do not seem to be sufficiently stable for the study of azimuth changes unless supplemented by frequent observations of circumpolar stars. The method I have employed for bringing a mark in focus is simple, direct and vastly preferable to the use of a long focus lens; since the marks may be located at such distance that any probable change in the place of the pin will not sensibly change the direction.

In photographic work, when it is desired to bring objects lying in different planes to a common focus the aperture of the lens is reduced. Accordingly, I made some experiments and found when the aperture of the object-glass of the Repsold meridian circle, of 6-foot focus, was reduced to one inch, all objects at about 1,000 feet and beyond were brought in good focus.

A concrete pier 2 feet square and rising 2½ feet above the surface of the ground was erected at a distance of 1,140 feet. On the top of the pier was bolted a cast-iron box, 15 inches wide, 10 inches high and 10 inches deep. Inside the box is an adjustable brass plate with a hole 0.15 in diameter, behind which is placed a 50-volt 16-candle lamp. Electricity is supplied from a storage battery, at the observatory, which had been installed for rotating the dome and illuminating the instruments. During daylight, when the sun is not shining, the mark appears like a sixth-magnitude star; at night brighter.

In order that an object at a finite distance may suffer no change of direction, the hole in the cap which covers the object-glass must be in the optical axis. Any deviation will cause a displacement proportional to the focal length of the telescope divided by the distance of the object. At 1,000 feet one inch subtends an angle of 17″.1. Hence very great precision in the fit of the brass cap is unnecessary. In order to know whether the hole in the cap is in the optical axis, the cap may be revolved 180 degrees, or the mark may be observed with the *full* aperture of the object-glass. In the latter case there is seen a well-defined disk of light about 90″ in diameter.

In 1861 and for a number of years following, at the Dudley Observatory, I had a mark at the distance of six miles. After a rain the mark could be observed with great precision. The unsteadiness of a terrestrial mark does not depend directly on the distance.

The Significance of the Star Ratio: George C. Comstock.

The number of visible stars increases very rapidly as we extend the count to fainter and fainter magnitudes, and any rational attempt at their enumeration must involve a limit, or limits, of brightness at which that enumeration shall cease. The rate of increase in the number of stars as this limit is made to move down the scale of magnitudes is called the star ratio, and the numerical value of this ratio in different parts of the sky and at different points in the scale of stellar magnitudes has been made the subject of research by many astronomers. From these investigations it appears that in general the number of stars is increased more than threefold and considerably less than fourfold for each increase of one magnitude in the limit to which the enumeration is extended. The ratio appears to be a little greater in the Milky Way than in extra-galactic regions and possibly a little greater for the brighter magnitudes than for the fainter ones, although it seems probable that the last relation is confined to the region outside the galaxy.

The point of major interest in the discussion is, however, that in general the rate of increase is decidedly less than fourfold, while a very simple analysis shows that if the stars are strewn with some rough uniformity of distribution throughout a region of indefinite extent, the average star ratio should be very approximately a fourfold increase per magnitude. The disparity between this theoretical ratio and that found actually to obtain, throws discredit upon the hypothesis above made with respect to the distribution of the stars, and there has been reared upon it the current concept which represents the stellar system as of finite and measurable extent, broader in the galaxy because here the star ratio is relatively large, smaller at right angles to the Milky Way because here the star ratio diminishes. The idea is that the faint stars are faint because of their greater distance and are more numerous because the volume of space in which they may be distributed increases with the cube of the distance. But if the stellar system reaches out only to a certain limit and the space beyond is void, it can contribute nothing to the number of stars and the star ratio, while everywhere below the value that would obtain for an infinite system ought to diminish very rapidly as we approach the confines and deal with stars fainter than any that have been hitherto enumerated, although Professor Pickering holds that even within the range of magnitudes covered by his investigations, such a diminution in the value of the ratio is distinctly shown.

The present paper controverts the views above outlined and shows that the supposed fourfold ratio that constitutes their theoretical basis has been erroneously derived through ignoring an essential factor of the problem. The faint stars appear faint not only because of their greater distance, but because they actually emit less light than do the brighter ones, and because of this inferior luminosity they are nearer than has been assumed. This diminished distance is shown by observation of their proper motions and because of it we have a diminished space available for the faint stars, they are less numerous and the star ratio smaller than is required by the erroneous theory above considered. When the diminishing intrinsic brightness of the fainter stars is properly taken into account the author finds from a discussion of the star ratio for galactic stars down to the faintest yet enumerated (the Herschel gauges), that there is here no indication of a limit to the stellar system.

Outside the galaxy the conditions are different, the values of the star ratio are progressively smaller and suggest some one of the following alternative conditions or possibly a combination of them:

(*a*) At right angles to the galaxy the limits of the stellar system fall within the range of vision, as indicated above.

(*b*) The stars remote from the plane of the galaxy are on the average progressively less luminous than those in the galaxy.

(*c*) The transmission of light through the extra-galactic spaces is impeded by some absorbing medium which serves to diminish the brilliancy of the stars in larger measure than is the case in the galaxy.

Any of these alternatives will serve in explanation of the observed facts and it is not now feasible to make definitive choice among them.

Preliminary Wave-lengths from Flash Spectra taken in Spain, 1905: S. A. Mitchell.

The flash spectra were photographed by means of a Rowland four-inch grating ruled on a parabolic surface. The grating was used without a slit, so that the spectrograph consisted merely of grating and photographic plate. The spectra, which show wonderfully fine definition, extend from the D lines to λ 3,300 in the violet, a distance of 9.5 inches.

Measurements are now being made on the three or four thousand lines of the flash spectra. These photographs show a dispersion about the same as the Bruce three-prism spectrograph of the Yerkes Observatory and the Mills spectrograph of the Lick Observatory, or a dispersion one fifth that of a twenty-one-foot grating of ordinary Rowland mounting in the first order spectrum, or one tenth of the second order. The eclipse spectra were practically normal. Wave-lengths thus far deduced show a probable error less than five-hundredths of an Ångstrom-unit.

Comparison of Results of Observations with the Reflex Zenith Tube and Zenith Telescope at the Flower Observatory during 1905 and 1906: C. L. DOOLITTLE.

The Temperature of Mars: PERCIVAL LOWELL.

On the Absence of Long Heat Waves in the Sun's Spectrum: E. F. NICHOLS.

Formulas for the Comparison of Astronomical Photographs: HAROLD JACOBY.

This paper contains formulas suitable for the direct comparison of rectangular coordinates measured on different astronomical negatives. The problem here involved supplements what may be called the fundamental transformations in the reduction of celestial photographs; *viz.*, the calculation of right-ascensions and declinations from rectangular coordinates, and rectangular coordinates from right-ascensions and declinations.

Light Curves of New Variable Stars of the Algol Type, and of Short Period: HENRIETTA S. LEAVITT.

In a recent circular of the Harvard Observatory thirty-six new variable stars were announced, mostly brighter than 9.5 at maximum. All are within fifteen degrees of R. A. 12^h, Dec. — 60°, the region including both the Southern Cross and the Nebula in Carina. About sixty new variables in this area, mostly fainter than the tenth magnitude at maximum, had been announced in previous circulars. A large proportion of the new objects appear to have short periods, and several are of the Algol type. These are now being studied with a view to determining their periods and light curves. The periods of four have already been announced, those of three others are now made public for the first time. All but one of the seven variables are of the Algol type. The periods vary from nine tenths of a day, to five and one third days, while the smallest and the largest ranges observed are four tenths of a magnitude, and two and one half magnitudes, respectively.

The variable C. P. D.—50° 3,809 is of special interest, as the period is equally divided by a secondary minimum. The principal minimum is nine tenths of a magnitude fainter than the normal brightness, 9.3, while the secondary minimum is four tenths of a magnitude fainter than the normal. The average deviation from the light curve of a single observation is very large, being no less than ± 0.14 magn., while the average deviation for all the other Algol variables here described, is only ± 0.07 magn. This large deviation does not appear to be due to accidental errors of observation, though the variable was difficult to measure on many of the plates. It has not been found possible to improve the period on the supposition that it is constant, and it is probably to be corrected by a third term, not yet determined.

C. P. D.—49° 6,972 has a large range. The faintest magnitude observed is 11.5, which is two and a half magnitudes fainter than the normal brightness; but no observation at minimum has as yet been secured, and the form of the light curve at that point is not determined.

C. P. D.—63° 2,485 was at first supposed to be of the Algol type, but has been found instead to be a short-period variable of unusual interest. As has already been announced in a recent circular, the light curve resembles that of an Algol variable with a minimum covering about half the period, but the light appears to be changing continuously, though very slightly, even when near maximum. As the range is only four tenths of a magnitude, four independent observations were made on each plate. By taking means, the accidental errors were reduced one half. The mean magnitudes were then arranged in the order of phase, and the mean phase and magnitude was taken for each successive group of five plates. The average deviation from the light curve of the points thus obtained is only ± 0.02 magn., and may be compared with the deviations found with the best photometric measures.

The large proportion of Algol variables among those discovered in this region is interesting. Of thirty-six variables announced last month, four have already been shown to be of the Algol type, and others, of which the observations have not yet been discussed, are supposed to belong to the same class. On the other hand, of more than seventy variables in Scorpius and Ophiuchus, announced two years ago, few, if any, appear to vary in this manner. The same is true of the Magellanic Clouds. The present study of the distribution of variable stars is in a very early stage and much generalization from the results as yet obtained is rash. Yet it has already become evident that certain kinds of variables are apt to be found in groups. It is desirable that the number of persons engaged in this research should be increased, so that the systematic survey of the heavens may be completed with a reasonable degree of thoroughness during the next few years. Evidently it is of the highest importance to ascertain the types to which new variables belong, even if their number is too large to permit the computation of all the periods.

A Peculiar Binary System: ERIC DOOLITTLE.

Latitude Terms of Long Period, from the Flower Observations: C. L. DOOLITTLE.

This series embraces a period of seven years and nearly two months, and is practically homogeneous in all respects. As a term having a period of about six years has been supposed to be indicated by similar series elsewhere, it seems desirable to examine my results for evidence on this point.

Without making any assumption as to the law of latitude variation, the intervals from minimum to minimum were found graphically. The maxima were not employed for this purpose, as they were less clearly indicated. Six periods were found as follows:

1st	459 days
2d	410
3d	470
4th	439
5th	446
6th	416
Mean	440 days

5 periods = 2,200 days
6 Julian years = 2,191.5 days

The difference 8.5 days is of no importance for present purposes.

This close agreement seems to fall in line with the possible existence of a term having a period of about six years.

The method of procedure was as follows: For each interval from minimum to minimum, terms of the form $x \sin \theta + y \cos \theta$ were removed from the given values of the latitude. A series of 103 equations was then obtained of the following form:

$$\Delta + ax + \sin\theta\cdot y + \cos\theta\cdot z + \sin 2\theta\cdot u + \cos 2\theta\cdot v = n.$$

Δ is a constant correction to the latitude assumed.

x a uniformly progressive change.

The period of θ is six years.

The solution results in the following expression for $\Delta\phi$ in units of the second decimal place:

$$-1.96\sin\theta + 0.35\cos\theta + 1.25\sin 2\theta + 0.94\cos 2\theta,$$

x is quite inappreciable.

The maximum and minimum values of this expression are as follows:

Maximum .... 1898, February 1, $+0''.024$
Minimum .... 1902, April 9, $-0''.035$

The range, $0''.059$, seems too great to be altogether fictitious when the amount and character of the data employed are considered.

Period of the Solar Rotation: PHILIP FOX.

An investigation of the solar-rotation period based upon measurements of positions of 1,600 calcium flocculi on one hundred of the Rumford spectroheliograms taken at the Yerkes Observatory in the year 1904, gives the following results:

ϕ		ξ	P
0°	to 5°	14.50°	24.82[d]
5	10	14.44	24.93
10	15	14.18	25.38
15	20	13.92	25.86
20	25	13.68	26.32
25	30	13.95	25.80
30	35	13.68	26.31
35	40	13.25	27.18

The investigation will be continued, using the plates for the years 1905 and 1906.

Opportunities for Solar Research: GEORGE E. HALE.

It is safe to say that every astronomer would prize an opportunity to observe any of the fixed stars from a position where its disk would appear as large as the sun. It does not seem probable, however, that such observations of stellar phenomena can ever be made, except in the case of the sun itself. For it should ever be borne in mind, when considering the importance of solar research, that our most intimate knowledge of stellar phenomena must be derived from solar observations. In the case of the other stars, we may determine their positions, measure their radial velocities, observe their brightness and analyze their light, but we have no means of studying the details of their structure, which must be understood before we can advance far in the solution of the great problem of stellar development. Thus we are driven back to the sun and forced to the conclusion that this typical star well deserves our most serious attention, and the application of every available means of research.

One can not but be impressed, when considering the sun from this standpoint, with the comparative neglect of the numerous opportunities awaiting the student of solar physics. It is possible, by the application of easily available instruments, for any careful student, wherever situated, to solve solar problems of great importance. If time permitted, it could be shown that almost all the apparatus required in such work can be constructed at very small expense. For our present purpose, however, let us assume that the observer has at his disposal one of the cœlostats so commonly employed in eclipse work. If this cœlostat has a rather thick mirror, which is frequently resilvered, it may be depended upon to serve well for solar work, provided that the mirror is shielded from sunlight during the intervals between the exposures of photographs, and that these exposures are made as short as possible. We may assume that the sunlight is reflected from the cœlostat mirror to a second plane mirror (which should also be as thick as possible) and from this mirror to an objective, which should have an aperture of at least

6 inches and a focal length of from 40 to 60 feet. In place of this objective, a concave mirror, of similar aperture and focal length, may be employed. This apparatus will furnish the necessary means of forming a fixed solar image, of large diameter, within a laboratory, where accessory apparatus can be mounted. Let us now consider briefly some of the investigations that can be undertaken.

Direct Photography.—The routine photographic work, done under the direction of the Greenwich Observatory, provides ample material for the study of the positions and motions of sun-spots, but special investigations may well be undertaken with the aid of direct photographs. The important thing in all solar work is not merely to make observations of some single phenomenon, but to carry on two or three series of carefully correlated observations, so designed as to throw light on one another. For example, Mr. Maunder has recently found that the rotation periods of sun-spots in nearly the same latitude show differences as great as those encountered in passing from the equator to the highest latitude in which the spots are found. The cause of such differences may well be a subject of most careful investigation. The proper motions of spots, which are associated with their period of development, must be fully taken into account. We might also make the hypothesis, merely for the purpose of testing the question, that the rotation period of a sun-spot depends upon its level with respect to the photosphere. For this reason it would be desirable to investigate, in connection with the study of the rotation, the question of the level of sun-spots. A simple means of doing this will be mentioned later. But it may be added here that the question of level raises other considerations, which should not be left out of account. It is probably worth while to investigate photographically the old Wilsonian hypothesis, since visual observations have proved so discordant in attempts to determine the relative widths of the preceding and following penumbra of spots at various distances from the center of the sun. As a sun-spot is depressed below the level of the surrounding faculæ, the vexed question of the visibility of the umbra near the limb may depend upon whether faculæ are present or missing on the sides lying in the line of sight. It is quite possible that the temperature of the umbra may vary with its distance above the photosphere. Thus correlation between observations bearing on spot level and observations of spot spectra is desirable.

Spectroscopy.—The spectroscopic study of solar phenomena has been greatly retarded, through delay in adoping suitable instruments. The short-focus spectroscopes attached to equatorial telescopes are admirably adapted for visual observations, but in photography their linear dispersion is much too small to realize the full resolving power of the grating employed. In laboratory work, on the contrary, while the spectroscopes have been sufficiently powerful, they have usually been of the concave grating type, where astigmatism interferes seriously with the study of solar details, and the solar image on the slit of the spectroscope has been so small that the individual phenomena, in any event, could not be separately distinguished.

The construction of a powerful spectrograph of the Littrow type is an extremely simple matter. A small slit, mounted on a short metallic tube, is supported immediately above a long narrow photographic plate. The wooden support for plate-holder and slit rests on a pier and forms the end of a long light tube of rectangular section, which is closed at its other end by the wooden support for the lens, which serves at once for collimator and camera. The angular aperture of this lens is, of course, defined by that of the objective which forms the solar image on the slit, but if possible its focal length should be from ten to twenty feet. The rays, after being rendered parallel by the lens, fall upon a grating, which need not be larger than a four-inch (a much smaller one would do very useful work). The spectra should be photographed in the second, third or fourth order, so as to give sufficient scale.

With such an instrument, new work of great value may be done. Even with a very small solar image, a photographic study of the solar rotation should yield results of great precision. Halm believes, from his spectroscopic work, that the rotation period varies with the solar activity. This is yet to be confirmed, but the question well deserves investigation. There is some reason to think that the rotation period is not the same for different substances in the reversing layer. The iron lines, for example, may give values different from those obtained with the carbon lines. It is also interesting to inquire whether the enhanced lines of an element give the same period as the other lines in its spectrum.

Another interesting investigation, which does not require a large solar image, is the study of the radial velocity of the calcium vapor in the flocculi. It is only necessary to measure, with great precision, the wavelengths of the H_2 and H_3 lines, corresponding to various points on the solar image. In this way the rise and fall of the calcium vapor in the flocculi can be ascertained. To be of the most service, this investigation should be carried on in conjunction with some other study of the flocculi.

The photographic study of sun-spot spectra offers a most promising opportunity. It is a very easy matter to photograph spot spectra in such a way as to record for study thousands of lines which are beyond the reach of visual observation. Nevertheless, this has been accomplished only recently, simply because spectrographs of suitable design have not previously been applied in this work. At the Solar Observatory on Mt. Wilson it has been found that, in general, the lines strengthened in spot spectra are strengthened in the laboratory when the temperature of the vapor is reduced, while the lines that are weakened in sun-spots are weakened in the laboratory under the same conditions. Thus it appears probable that the temperature of the spot vapors is below that of the reversing layer. This conclusion has been confirmed by the discovery in the spot spectrum of the flutings of titanium oxide. This compound thus exists at the lower temperature of the sun-spot, but is broken up into titanium and oxygen at the higher temperature of the reversing layer. The bearing of this result upon stellar spectroscopy will be seen when it is remembered that the flutings of titanium oxide form the principal feature of the spectrum of the third-type stars. It has also been found that Arcturus gives a spectrum resembling very closely the spectrum of a sun-spot. A further study of this question will require a large number of observations of spot spectra, with special reference to the question of variations in temperature, as indicated by variations in the relative intensity of the spot lines. As already remarked, the temperature of spots may also depend upon their level, and this possibility must be borne in mind.

Work with the Spectroheliograph.—It is perhaps commonly supposed that the spectroheliograph is necessarily an expensive instrument, out of reach of the average observer. As a matter of fact, however, a spectroheliograph capable of giving the best results can easily be constructed of materials ordinarily available in any observatory or physical laboratory. It is sufficient, for many purposes, to photograph only a narrow zone of the solar image. In this case small lenses will suffice for the collimator and camera, and small prisms for the optical train. The lenses and prisms may be mounted in wooden supports, on a wooden platform, rolling on four steel balls in V-shaped tracks. The motion of the instrument across the solar image may easily be produced by a simple screw, driven by a small electric motor. Such a spectroheliograph was used to good purpose at the Solar Observatory before the permanent instrument was completed.

Brief mention may be made of some of the numerous investigations possible with such an instrument. It has recently been found at the Solar Observatory that the dark hydrogen flocculi, photographed near the sun's limb, are slightly displaced with reference to the corresponding calcium flocculi. In general, they lie nearer the limb. This probably indicates that the absorbing hydrogen clouds are, on the average, at a higher level than the brilliant calcium clouds. This subject deserves careful investigation, extending over a considerable portion of time. The type of spectroheliograph just referred to is as suitable for the purpose as any instrument that can be constructed. Another question, which seems to be somewhat more difficult to solve, is the actual difference in elevation of the calcium flocculi, as photographed in the H_1 and H_2 lines. Indeed, it is still a question as to how important a part the dense calcium vapor plays in determining the form of the H_1 flocculi. These objects resemble the faculæ so closely that they appear practically identical with them, though slight differences, which are apparently genuine, are occasionally found.

Another method of investigating this whole question of levels is afforded by the spectroheliograph. It will be remembered that when the level of sun-spots was last under discussion, reference was made to the relative radiation of the umbra and neighboring photosphere, corresponding to different distances from the center of the sun. It was pointed out that when a spot approaches the limb, its radiation decreases less rapidly than that of the photosphere. The natural conclusion was that the spot lies at a higher level than the photosphere and thereby escapes much of the absorption produced by a comparatively thin layer of absorbing matter. Recent observations at Mt. Wilson have shown, however, that the proportion of violet light in sun-spots is much smaller than in the case of the photosphere. As it is known that the violet rays undergo much more absorption near the sun's limb than those of greater wavelength, it is obvious that the light of the spot would suffer less absorption, even if it were at the same level as the photosphere. Thus the only proper method of investigating this question will be through the use of monochromatic light.

The spectroheliograph affords a simple means of accomplishing this. It is only necessary to make photographs of the spot and adjoining photosphere, corresponding to various distances from the sun's center. The camera slit should be set on the continuous spectrum (not on a line), preferably in the violet or ultra-violet, since the change of absorption would be most felt in this region. In order to make photographic comparisons easily possible, the intensity of the photosphere should be reduced to approximately the intensity of the umbra, by means of a dark glass, mounted over the collimator slit, but not covering that part of the slit through which the light of the umbra passes. It is obvious that a large image of the sun will be required in this work.

The spectroheliograph can be applied to other studies of absorption. The H_1 flocculi, for example, are reduced in brightness near the sun's limb much more than the H_2 flocculi, presumably because the latter lie at a higher level. These differences can be studied photometrically on spectroheliograph plates made for the purpose. Since it is a question just what level is represented by the background (between the flocculi) in calcium, hydrogen or iron photographs, the instrument should be arranged so as to permit photometric comparisons with the light of the photosphere, of practically the same wave-length as the calcium, hydrogen or iron line employed.

These new applications of the spectroheliograph have only recently occurred to me, and are mentioned because of their suitability for use with instruments containing prisms of ordinary height, capable of photographing only narrow zones of the solar image. Numerous other problems might be mentioned, such as the comparative study of H_1, H_2 and H_3 photographs, and of calcium, hydrogen, and iron images; the distribution of the flocculi in latitude and longitude, their varying area, as bearing on the solar activity and on terrestrial phenomena, and their motion in longitude, as measuring the solar rotation. But limitations of time forbid more than a mere reference to work and methods, the details of which are discussed elsewhere. My purpose has been accomplished if I have shown that with comparatively simple instrumental means any careful observer may secure important results. In much of this work it is desirable that investigators occupied with similar problems should cooperate with one another. The International Union for Cooperation in Solar Research was organized with this end in view. It has already inaugurated solar studies, on a common plan, in several different fields, and is preparing to extend the range of its activities in the near future.

HAROLD JACOBY,
For the Council

SCIENTIFIC BOOKS

Rock Minerals, their Chemical and Physical Characters and their Determination in Thin Sections: JOSEPH P. IDDINGS. Wiley and Sons, New York. Pp. xii + 548, with numerous figures in text.

In presenting this work the author and publishers have won the gratitude of every American student in petrography, and of every teacher as well. Heretofore, the only systematic and comprehensive treatises available have been the 'Mikroskopische Physiographie' of Professor Rosenbusch or Professor Iddings's condensed translation of the same, and the works of Fouque and Levy. While no one would for a moment wish to disparage the work of one who has fairly earned the title of father of modern petrography, yet, as may readily be comprehended, the manner of presentation in the first-named publication, as well as the language in which it is presented, is German, and not always easy of comprehension to the average American student.

There have been, it is true, other works on the subject, in English, as Professor Luquers's 'Minerals in Thin Sections' and Harker's 'Petrology for Students,' but such make no pretense at completeness, and it has remained for Professor Iddings to give us a book as comprehensive and systematic as those of the German and French writers noted.

Within the limits of some 550 pages Professor Iddings includes not merely a description of the optical properties and methods of determination of all the ordinary rock-forming minerals, but also chapters on their chemical and physical characters. The critical chapter of the book is undoubtedly that relating to the optical properties of minerals, and it is apparently in recognition of this that the author has devoted upwards of 100 pages to a discussion of the theory of light, its reflection, refraction and polarization, the manner in which it is acted upon by isotropic, uniaxial and biaxial crystals, and finally how these properties may be made of determinative value by means of a properly constructed microscope, the various appliances and method of application of which are fully explained. Pages 201–526, inclusive, are devoted to detailed discussions of the characteristics of the individual species of the rock minerals. Here the author is at his best, and has left little to be desired in the way of clear and exhaustive treatment. Each mineral is considered with regard to its chemical composition, alteration, crystal form, optical properties, color, inclusions, mode of occurrence, resemblances and laboratory reproduction, in the order here given. The arrangement of the minerals, on a chemical basis, strikes one, however, as a trifle illogical, since the methods of determination are almost wholly optical and one who has worked with the book of Rosenbusch will at first find it awkward. Pages 527–541 are devoted to tables giving these optical characters. The book is illustrated by upwards of four hundred figures—mostly crystal outlines—in the text. Many will miss the plates of micrographs of thin sections found in the work of Rosenbusch, but it is possible that Professor Iddings, as a teacher, wished to guard the student against the danger of relying too much upon the appearance of a mineral rather than upon its optical properties. The adoption of a new system of numbering with each change of subject, though the same as that used by Dana in his system of mineralogy, is a little confusing to one turning the leaves at random, but inasmuch as each figure occurs on the page of text in which reference to it is made, no serious confusion is likely to arise.

The book is of good size and form for convenient use, well printed and bound, and that it is essential to every student and teacher must be self-evident. George P. Merrill

The Human Mechanism, its Physiology and Hygiene, and the Sanitation of its Surroundings. By Hough and Sedgwick. Boston, Ginn & Company.

This book adds another to a series of very useful text-books on the subject of physiology designed for secondary schools. It is, however, rather unique among the series in several respects, the more distinctive of which the present review will endeavor to point out.

First among them is the point of view from which the authors approach the subject, namely, as implied in the title, The *human body* as a *mechanism.* This conception the authors regard as "not only the sure foundation of physiology, hygiene and sanitation, but is also surprisingly helpful in the solution of many questions concerned with intellectual and moral behavior."

A second feature upon which emphasis is placed is the conception of the mechanism *as a whole.* "Avoiding that form of physiology which looks chiefly at the organs and overlooks the organism, we have constantly kept in view the body as a whole, in order that physiology may become the interpreter of the common physical phenomena of the daily life and find in hygiene and sanitation its natural application in conduct." In this view there will doubtless be general concurrence.

Still a third feature, more or less distinctive, is the large consideration given to problems of *sanitation,* both domestic and public. So far as the reviewer is aware, this is the only text-book designed for schools in which this aspect of physiology has received the degree of attention it deserves. Just how far this may properly come within the scope of the average school course will doubtless be a matter of divided opinion; as may likewise be the further query as to how far details of sanitary principles come within the intelligent apprehension of the average pupil of the grades likely to be affected. But allowing for such debatable differences, it would seem that provision should be made for some insight into this increasingly important subject. To the vast majority of pupils of the high school this will be about the only opportunity for such insight, and it is the reviewer's conviction that the authors have done well to put it well to the front in their book.

There is another point, however, mainly a pedagogical one, which seems open to some criticism, namely, whether in the vast body of admirable and trustworthy information, which makes the book a veritable cyclopedia, the essentially didactic and disciplinary aspects of the subject have not suffered an undesirable and unnecessary eclipse. If the primary aims of physiology in the schools be informational, and the importance of this will not be questioned, then some subordination of those methods of observation and experiment so distinctive of science may in measure be justified. But even in that case it may well be questioned whether these very methods do not afford a *distinctive type* of information, more vital and impressive, and at the same time incomparably more abiding; and this the reviewer believes to be the case. It is much to be regretted, therefore, that at least some provision had not been made in the body of the text and throughout for pertinent experiment and demonstration. The almost total absence of anything savoring of laboratory directions is hardly atoned for in the brief prefatory statement that varying facilities in different schools made this less imperative. The very presence of such directions would have served to promote a larger and more uniform system of judicious laboratory practise.

Upon the whole, the book is easily among the very best now available, and indeed far and away superior to the average text-book of similar scope. It marks a decided step in advance, and will doubtless find a wide and growing field of usefulness, both in the upper years of the high school and beginning courses in college.

In its typography and other mechanical aspects the book seems exceptionally free from glaring defects, and maintains the high repute of the publishers in this line of book-making.

C. W. H.

SCIENTIFIC JOURNALS AND ARTICLES

The American Naturalist for March contains but three papers, though these are of considerable length. The first, 'Studies on the *Ophioglossaceæ*,' by D. H. Campbell, deals mainly with the morphology of the peculiar, fertile leaf segment, or sporophyll. R. W. Shufeldt discusses 'Polygamy and Other Modes of Mating among Birds,' the object being avowedly to throw some light on the question of mating among mankind. A large number of statements are made, the bearing of which is to be given in another paper. Outram Bangs writes 'On the Wood Rails, Genus Aramides, occurring North of Panama,' describing as new one form from Mexico to which the name *Aramides albiventris mexicanus* is given.

The American Museum Journal for March has illustrated articles on the mounting of 'The African Lion Hannibal,' 'The Naosaurus, or Ship-Lizard,' and 'A New Eskimo Exhibit,' and contains the lecture schedule for the month. The mounted lion, and the bizarre skeleton of Naosaurus are respectively triumphs of the taxidermist and preparator of fossils.

The Museums Journal of Great Britain for February has various articles on museum cases; the first, by H. Bantry White on 'Some Improvements in Museum Cases,' describes methods of making iron cases by which their cost has been greatly reduced. F. A. Lucas gives briefly his ideas relative to 'The Structure and Arrangement of Museum Cases,' finding a lack of effect in iron cases and emphatically endorsing the bay system of arrangement. 'The Liverpool Museum Extension' deals with the rearrangement of the zoological and anthropological collections in a consecutive, educational plan. The view of the zoological hall impresses one with the idea that it is a little too narrow for the best results.

The Springfield Museum of Natural History has issued a 'leaflet' entitled 'Bird Migration,' giving the dates of arrival of one hundred species of birds found within ten miles of Springfield, during the springs of 1901 to 1906, with spaces reserved up to 1910. The list is arranged chronologically for 1901, although there is considerable variation in the dates of arrival of the species subsequently.

The Bulletin of the Charleston Museum for March is mainly devoted to an excellent article by Mrs. Paul M. Rea on 'The Relation of the Museum to the Schools.'

The Science and Art Museum Dublin is issuing a series of 'guides' to the collections, which are sold at the nominal price of a penny. The last two of the series, devoted to armor, and to arms (European) are by M. S. D. Westropp and comprise a descriptive catalogue of the specimens in the museum, with a large amount of general information as to the classes of objects described. They are extremely interesting and models of their kind.

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

At the 187th meeting of the society, on February 13, 1907, Mr. J. S. Diller presented briefly the results of extended studies by him on the age of the auriferous gravels in Oregon and the discovery of marine Eocene fossils in the same.

Mr. Fred. E. Wright exhibited artificial crystals of cuprite and an asbestos-like mineral of the composition of tremolite, both formed at high temperatures and under considerable pressure.

Regular Program

Mr. Whitman Cross gave a brief review of the recent article on 'New Textual Terms for Igneous Rocks' by Cross, Iddings, Pirsson and Washington in the *Journal of Geology*, XIV., 692–707, and emphasized the underlying principles which guided the authors in their classification and description of the textures of igneous rocks.

The Pine Mountain Fault: Mr. R. W. Stone.

Pine Mountain forms part of the boundary between Kentucky and Virginia and is a long narrow ridge having a general elevation of 3,000 feet. This discussion deals only with the northern end of the mountain from Pound Gap to Big Sandy River. The Virginia side of the mountain is comparatively steep, the strata dipping southeast at angles up to 25 degrees, while the north or Kentucky side is precipitous and a good example of a fault scarp. The great fault which formed the mountain is on the north side and parallels the crest of the ridge for many miles. In the 'breaks' where Russell Fork of Big Sandy River passes the end of the mountain in a gorge 1,000 feet deep, a diagrammatic section shows clearly the uplifting and over-riding of the Lee conglomerate on the upturned edges of the Coal Measures. In the coal field immediately west of the Pine Mountain the Lower Elkhorn seam commonly shows a fifteen-inch bench of laminated coal. It has every appearance of squeezing and movement, the coal being crushed to a flaky condition and the surfaces of the flakes slickensided. The lamination may be parallel to the bedding, but is often tilted or contorted; it decreases and disappears at a distance of several miles from the mountain.

Phosphate Deposits in the Western United States: Mr. F. B. Weeks and Mr. W. F. Ferrier.

It has been found during the past few years that the limestone strata of the upper Carboniferous of the Central Cordilleran region include a series of oolitic beds containing a variable percentage of P_2O_5 and varying in thickness from a mere trace to ninety feet. These beds are known to occur in Idaho, Wyoming, Utah and Nevada, and future exploration may show that they have a still wider distribution. They are usually underlain by blue-gray compact limestone strata which in turn pass into sandy limestones and yellow sandstones. The phosphate series consists of alternating layers of black phosphatic material, shale and hard blue or brown compact limestone which is often fossiliferous with *Rhynchonella, Chonetes* and *Euomphalatrochus* as characteristic forms. Within the series the phosphate beds vary in thickness from a few inches to ten feet, some of which are almost entirely oolitic in character and commercially valuable because of their high content of P_2O_5, the average analysis of carload lots giving 32 per cent. P_2O_5 equivalent to 70 per cent. bone phosphate.

In Utah the phosphate series is exposed in Weber Canyon near Croydon and also near Woodruff; in Wyoming, near Sage and also near Cokeville, where it extends along the west face of the Sublette Range on the east side of the valley of Thomas Fork; on the east side of Bear Lane and along the west face of the Preuss Range in Idaho, at Montpelier, Bennington, Georgetown and in the vicinity of Swan Lakes. This discovery has opened a new industry in the west; its future development is dependent on the cost of transportation to foreign and domestic markets.

The 188th meeting of the society was held on February 27 and was devoted to a consideration of the 'Methods of Igneous Intrusion.'

The discussion was opened by Mr. Whitman Cross, who directed it to the methods by which the large igneous masses, such as laccoliths, stocks and batholiths, have come to the places where they are now visible. The three main agencies called upon in current literature to account for these masses are: (1) Mechanical displacement of the invaded rocks, (2) fusion and assimilation of the rock by the magma, (3) 'magmatic stoping.'

It was claimed that laccoliths, in the sense of Gilbert's original definition, and many closely allied bodies, are beyond question produced by a purely mechanical uplift of rocks, usually sedimentary, above the plane of intrusion; that assimilation and stoping are at the most rare and subsidiary phenomena. No instances are known to the speaker.

The origin of stocks and batholiths, viewed as very similar except in point of size, is less evident than that of laccoliths, because we can ascertain the relations for only a portion of each mass. That fusion of country rock by an invading magma, with subsequent assimilation, is a demonstrated or adequate explanation for stocks and batholiths was denied. This hypothesis is usually advanced with naïve disregard for the difficulties involved in its acceptance. Among these were mentioned: (1) The manifest impossibility of assimilating and assimilated rocks occupying the same space: (2) the physical problem of supplying and maintaining the heat necessary to keep the magma liquid in spite of conduction into wall rock and absorption in the fusion assumed; and (3) the necessity for demonstrating that an invading magma, as, for instance, one of granitic composition, had been changed in character through assimilation of quartzite, limestone, or basic igneous rocks. In most stocks and batholiths there is absolutely no evidence that fusion of wall rock has occurred. It can scarcely have taken place on a large scale without leaving evidence of such action. While fusion must surely be assumed as taking place under certain conditions, there is no good reason to believe that those conditions were realized in known stocks and batholiths. Even should extensive fusion be demonstrated for certain cases, that process is not in itself competent to explain the masses under discussion.

Mr. Cross called special attention to the hypothesis of 'magmatic stoping' advocated forcibly by Daly in the last few years. After assuming that crustal movements must result in liquefaction of rock locally through decrease of pressure, the magma is pictured by Daly as eating its way upward by a process in which the main factors are the detachment of blocks of rock from the cover of the molten mass, their descent into the lower and hotter parts of the magma and consequent fusion and absorption. It is supposed that the magma may thus quietly rise far into the crust to horizons which through erosion have in many cases become accessible to our observation.

The magmatic stoping hypothesis of Daly rests upon two fundamental assumptions—viz., that the magmas of stocks and batholiths possess a high degree of liquidity and that the specific gravity of most crystalline rocks is greater than that of even a gabbroic magma in the assumed liquid condition. The high liquidity of batholithic magmas, although assumed by Daly as a matter of common acceptance, was questioned by the speaker on the basis of recent physical investigations and observed facts. In general, the facts of field occurrence are believed to show that the magmas of batholiths have in reality a high sustaining and lifting power; that blocks of country rock do not sink, but rather float, in the magmas; that basic inclusions, often of considerable size, are brought up from the depths in batholithic magmas. The data at our disposal for estimating differences in density between magmas and solid rocks are meager and inconclusive. In any case, the hypothesis fails to account for basic stocks in highly siliceous rocks.

Referring to stocks and batholiths which he had studied, Mr. Cross stated that they testified rather to violent and powerful ascensive forces back of the magmas and expressed the belief that in such masses, as in laccoliths, the coming to place of the magmas was in first degree a mechanical displacement of the invaded rock, as such.

Mr. G. F. Becker considered the intrusive magmas from a physical and chemical standpoint and emphasized his view that such magmas are emulsions rather than liquids; that, at the time of intrusion, they consist largely of crystal aggregates with a small amount of interstitial material not yet crystallized—a fact evident from the mutual interference and simultaneous crystallization of the components of any deep-seated rock. This state was compared to that of partially melted snow which consists of ice crystals with some free water; in short, the magma at the time of intrusion is a soft solid like modeling clay and the intrusion must therefore follow different laws from those of an intrusive fluid. In particular he pointed out that semi-solid magmas may support masses of relatively large density. The presence of aplites and pegmatites in granular intrusives and not in porphyries is significant in support of this theory of the soft solid condition of deep-seated intrusive magmas.

Mr. A. L. Day directed attention particularly to the physical conditions which must be reckoned with in formulating the stoping hypothesis.

1. The wall rocks in these cases must be accounted very good conductors of heat. It is, therefore, difficult to conceive of a sharp temperature difference between the intruding mass and the wall rock existing for more than a very short interval of time, whatever the relative masses involved. If the amount of heat to be distributed is large, active resorption must occur; if small, adjacent layers of the intruding mass will very soon become solid or hyperviscous.

2. The evidence which has been gathered by the Geophysical Laboratory points persistently to the extreme viscosity of all the highly siliceous minerals and mixtures, even at temperatures far above their melting points.

3. There is a very reasonable probability that most crystalline rocks are more dense at the melting temperatures than the liquids which they form, but it will be remembered that Professor Barus's experimental proof was confined to the gradual transition from liquid to amorphous glass, and therefore leaves the important question still open.

Mr. Andrew C. Lawson criticized Daly's hypothesis from the point of view of the great diameter of certain batholiths and the flatness of the arch roofing them. With a span of 100 miles or more, if the roof were specifically heavier than the invading magma, he did not see what would prevent its complete foundering. Referring to the high viscosity of the feldspars and quartz, as determined by Dr. Day's experiments, he indicated that, while this was a property of the individual crystals, it did not finally prove that mixtures of such materials in magmatic fusion with other constituents of granite would be so highly viscous. Dr. Becker had drawn the conclusion that porphyritic structures could only be developed in fluids of high molecular mobility. Now the granite rocks of the High Sierra were highly porphyritic over a wide extent. The large well-formed crystals of orthoclase, commonly over an inch in length, showed that in that great batholith the magma had not been highly viscous. Moreover, these porphyritic orthoclases were chiefly aggregated in the upper levels of the batholith as if they had been assembled there by flotation from the lower levels, again indicating absence of high viscosity. Further, the granite of the Sierran batholith swarms with angular inclusions. These are not fragments that had been torn from the roof and caught in process of sinking. They are mineralogically allied to the lamprophyres, and represent fragments derived from the shattering of deep-seated masses ascending with the upwelling of the batholithic magma. These facts all indicate fluidity. The speaker had, however, been one of the first to argue for the high viscosity of granitic magmas in the *final* stages of their consolidation, such viscosity ensuing after the crystallization of the feldspars and due to the behavior of the residual free silica of the magma which crystallized as quartz. It was well known that while rhyolitic magmas were more viscous than basaltic lavas, they were nevertheless fluid enough to flow as expansive sheets; and many granitic batholiths approximated such rhyolites in their composition sufficiently to indicate that they were not, except in the final stages of solidification, so highly viscous as to be regarded as solids rather than fluids. The speaker was glad to hear Dr. Day minimize the influence of pressure. He recalled the case of vertical basic dykes which in horizontal section graded from very dense compact porphyritic rocks on their margins to coarse gabbroic rocks in their middle part, 50 feet or less distant. This gradation in structure and texture necessarily occurred under the same pressure, and proved that pressure exercised but little control upon the development of these features.

Mr. G. O. Smith cited observations on intrusion phenomena in Washington, Utah and Massachusetts. In the Tintic Mountains the intrusive monzonite includes angular fragments of quartzite and limestone which have been carried upward after detachment from the wall rock, showing absence of assimilation by the magma and of sinking of the fragments in the same.

Mr. F. E. Wright described briefly examples of batholithic intrusion of granites in southeast Alaska, and of local recrystallization and assimilation of invaded rocks, and emphasized the important rôle of magmatic solutions in producing such alterations, rather than direct melting and absorption by the magma and recrystallization of the whole on cooling.

Mr. Waldemar Lindgren cited a number of examples of intrusions of granitic and dioritic rocks in the Sierra Nevada from which it was clearly apparent that a very considerable pressure was exerted by the intruding magma on the surrounding, steeply dipping slates. In many cases the intrusive rocks cut across the slates in jagged and irregular lines, but in nearly all cases the lateral pressure, resulting in the bending of the slates, is extremely well marked. Practically no evidence of assimilation on a large scale in this region was obtained.

Fred. E. Wright,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 631st meeting was held on March 16, 1907. Dr. L. A. Bauer read a paper on "The Relation between 'Potential Temperature' and 'Entropy.'"

The purpose of this paper was to show the precise relationship between von Helmholtz's term 'waermegehalt' used incidentally by him in connection with his investigations 'On Atmospheric Motions' or of the alternative term 'potential temperature' suggested and used by von Bezold in his papers 'On Thermodynamics of the Atmosphere,' and entropy. It was found that a simple relation existed so that for certain thermodynamic problems the second law of thermodynamics, or the principle of the increase of entropy, could be easily and directly expressed in terms of potential temperature. For these cases whenever the entropy increased during the carrying out of a thermodynamic process, the potential temperature was likewise accompanied by an increase. This was shown by application to certain well-known typical cases of natural, or irreversible processes, *e. g.*, free expansion of gases and heat conduction.

The second paper of the evening was presented by Mr. W. W. Coblentz, upon 'Selective Reflection of Minerals and Lunar Constitution.'

Throughout the spectrum from the ultraviolet into the remote infra-red, various substances show bands of selective absorption and selective reflection. Experiments made to determine whether these bands are due to chemical composition, to molecular weight or to the arrangement of the atoms in the molecule, have always given more or less contradictory evidence; and especially as to the effect of molecular weight. Only recently had the speaker been able to account for most of the contradictions.

From various considerations one would expect to find the bands to shift to the long wave-lengths with increase in molecular weight. It was known that certain groups of atoms cause certain absorption bands, but no shift in the maximum of the band could be detected when the number of groups of atoms was increased in the molecule. The contradiction lies in the failure to make a distinction between the effect of the groups of atoms which is to *cause* the absorption and the reflection bands, and the effect of joining these groups of atoms to various elements (different atomic weight) which have now been found to determine the *position* of the bands. By studying the transmission and reflection spectra of a homologous series of compounds it was found that the position of the characteristic band shifts toward the long wave-lengths, with increase in the molecular weight of the metallic element to which the group of atoms is united to form the compound. These bands lie in the region of $4.5\,\mu$ and $6.5\,\mu$ (transmission) and $8.7\,\mu$ to $9.1\,\mu$ (reflection).

The silicates are exceptions to all the rules, for there seems to be no regularity in the reflection bands, indicating that the grouping of the atoms of oxygen and of silicon is different in the different minerals studied.

When energy is reflected from a plane smooth surface it is commonly called 'regular' (or less accurately 'specular') reflection, while energy reflected from a rough surface suffers 'diffuse' reflection. The reflecting power, R, of any substance is related to its index of refraction, n, and its absorption coefficient, k, by the equation:

$$R = \frac{(n-1)^2 + k^2}{(n+1)^2 + k^2}.$$

For 'transparent media' or 'insulators' the absorption coefficient is almost zero and the reflection power is a function of only the refractive index. Here the reflecting power is low, only 4 to 6 per cent., and decreases with increase in wave-length. For metals, 'electrical conductors,' the absorption coefficient has become so large that nearly all the energy, for all wave-lengths, is reflected (90 to 98 per cent.).

For substances having selective absorption, when the coefficient of absorption, k, attains the high values the heat or light waves no longer enter the substance, but are almost totally reflected as in the case of metals—whence the name, 'bands of metallic reflection.' If, then, the eye were sensitive to heat waves, many substances would have a 'surface color' similar to that of gold and fuchsine in the visible spectrum. Furthermore, substances having selective absorption (and reflection) will have a low reflecting power in the region where the absorption coefficient, k, is small. In other words, the reflecting power of plane surfaces ('regular reflection') of substances ('transparent media,' 'electrical insulators') having selective absorption, will be low for all regions except where there are bands of 'metallic reflection.' It is evident that a rough surface of the same material will behave similarly, *i. e.*, it will likewise be selectively reflecting.

The speaker found that the silicates have a low reflecting power, 'practically zero,' for the region of the spectrum up to $8\,\mu$ followed by bands of metallic reflection from $8.5\,\mu$ to $10\,\mu$.

It was pointed out from the curves exhibited that a surface such as the earth or the moon if composed of silicates will have bands of strong selective reflection. If, then, one were to examine the heat spectrum of a planet which shines by reflected light, and if its surface is composed of silicates, one would expect to find bands due to selective reflection. By comparing these bands with those of known substances, the composition might be determined. In the case of the moon this is practically impossible on account of the weakness of the radiation to be measured. Atmospheric absorption, and the fact that, in the case of the moon, the maximum of its proper radiation lies in the region of the reflection bands of the silicates, will interfere with the observations. But there is still another complication in that the lunar radiation curve can not be smooth and continuous (as some writers seem to think) if the surface is composed of silicates, because in the regions of selective reflection the emitted energy will be suppressed, *i. e.*, there will be emission minima where there are reflection maxima (Aschkinass, Rosenthal). But the radiation from the moon can not be detected except when it is illuminated by the sun. The result is that if the surface is composed of silicates, then the observed energy curve will be the composite of the selectively emitted energy of the moon, and the selectively reflected energy of the sun. The selectively reflected energy of the sun will to a certain extent fill up the minima in the lunar emission curve. Atmospheric absorption will decrease the intensity of the radiation, so that it is almost too much to hope to study the composition of the various parts of the lunar surface by the identification of the selective reflection bands in its energy spectrum.

R. L. Faris,
Secretary

CLEMSON COLLEGE SCIENCE CLUB

The regular meeting of the club was held on the evening of January 18, at which time Dr. F. H. H. Calhoun gave an illustrated lecture on 'Geological Changes as Factors in Life Development.' The varying relations between the extent of the land masses and the sea was a powerful factor in the life development. When land rose, restricting the habitat of the life of the sea, the weaker ones were compelled to adapt themselves to a different environment or to perish. Again when there was a sinking of the land, the faunæ of the continents were forced to find some avenue of escape for themselves. The various problems which the succession of changes caused were considered in turn, but the main portion of the address was devoted to the development of the vertebrates, especially that of the reptilian dynasty. It seemed less a coincidence that a great geological change was always accompanied by a variation in the flora and faunæ, than that they held the relation of cause and effect.

S. B. Earle,
Secretary

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA

The 171st meeting was held in the main lecture room of Chemistry Hall, Tuesday, March 19, 7:30 P.M., with the following program:

Professor J. E. Latta: 'New Developments in Electric Traction.'

Mr. N. C. Curtis: 'Architectural Composition.'

Alvin S. Wheeler,
Recording Secretary

THE ST. LOUIS CHEMICAL SOCIETY

At the meeting of the St. Louis Chemical Society, on March 11, the president, Dr. H. A. Hunicke, opened the proceedings with a brief but feeling encomium on the illustrious chemists, lately passed away in such close succession—Beilstein, Mendeléef, Menchutkin, Roozeboom, Moissan. The society honored the memory of the great ones by rising. Mr. J. J. Kessler presented a paper entitled 'The Chemistry of Electrical Engineering.' Mr. Carl Hambuechen then presented a paper on the cognate subject 'Electro-Chemistry in the Industries.' The latter paper was profusely illustrated with lantern slides.

C. J. Borgmeyer,
Corresponding Secretary

DISCUSSION AND CORRESPONDENCE

THE FIRST REVISER AND ELIMINATION

If the present discussion of the rules and regulations governing zoological nomenclature shall result in a greater degree of uniformity among the workers in this field, the space that has been devoted to the subject in the pages of Science will not have been wasted. Few things have resulted so injuriously to the best interests of natural history as the lack of uniformity in regard to the names employed by different writers, following the radical difference in their methods of procedure.

Even at the present time, however, it appears that certain writers in our midst have not a clear idea of the method of elimination as applied to the settling of the question of the true type species of the earlier genera, apparently laboring under the mistaken impression that it is distinct from, or even opposed to, the first reviser method. As a matter of fact, *it is an integral part of this method.* Thus, the author who first eliminated one of the original species from the old genus must be considered as a first reviser, since he thereby restricted the limits of the old genus. In like manner the author that subsequently eliminated one of the species from the restricted genus must also be considered a first reviser, and so on down the line. Where the old genus originally contained only two species, neither of which had been designated its type at the time the first reviser eliminated one of them as the type of a new genus, he thereby caused the remaining species to become, by elimination, the type of the old genus, although he did not so designate it. Elimination, therefore, instead of being in opposition to, is in reality a part of the first reviser method.

The action of the first reviser has been upheld by the botanists as well as by zoologists, and is in perfect accord with the fundamental law of priority. Its very reasonableness has commended it to practically all workers in every department of natural history. On the contrary, the first species rule demands that the action of the first reviser be nullified in all those cases where he had designated any other than the first species as the type of the old genus, or had taken the first species as the type of a new genus; it is, therefore, in direct opposition to the first reviser method plus elimination, and also is in opposition to the law of priority. The futility of attempting to force this unreasonable, non-scientific method upon thoughtful, reasoning workers would appear to be so self-evident as to require no further comment.

D. W. Coquillett

U. S. National Museum,
April 3, 1907

POLISHED PEBBLES

To the Editor of Science: On page 392, in the issue of Science for March 8, 1907, it is stated that wind-polished pebbles from New Jersey are facetted. The wording of the assertion is such as to justify the possible inference that wind-polished pebbles are always facetted. It is doubtless true that in regions where the wind is prevailingly from one quarter, pebbles partially imbedded and held firmly during the polishing process, are usually facetted. On the other hand, where hard unimbedded pebbles and boulderlets lie on the surface of hard rock ledges, fully exposed to strong winds, they become highly polished, but seldom or never show even the slightest tendency to facetting. Facetting can not, therefore, be regarded as an unfailing characteristic of wind-polished pebbles. At White Rock, a few miles east of Boulder, Colo., beautifully polished quartz, quartzite and other pebbles lie by thousands on the wind-eroded surface of the Laramie and Fox Hills sandstones, but probably a day's search would not secure a single pebble showing the slightest suggestion of facetting. A few miles southwest of Villa Grove, in the same state, on a hill of Carboniferous limestone, perfectly polished pebbles are plentiful. No lapidary could do more perfect work, but facetting is not found. These are not gastroliths.

A very interesting discovery of polished pebbles was made by Mr. Philip Argall, of Denver, in the Santa Eulalia mining district, Chihuahua, Mexico. In one of the mines on Santa Eulalia Mountain, the shaft penetrating the massive Cretaceous limestone cuts a fissure leading to a chimney lined with low-grade ore. At the bottom of the chimney, at a depth of 1,200 feet below the surface, there is an elliptical cave-like opening thirty by fifteen feet. The bottom of the cave was plentifully strewn with perfectly polished flint pebbles which were cemented to the calcite-covered floor like plums. In other places the pebbles were found in pot-holes in the underground water courses. The history of these pebbles is believed to be as follows: The deposition of ores was followed by a period of solution during which the caves were formed, and the limestone in places rendered open and sponge-like by solution. The walls and floors of some of the openings were covered with calcite, deposited laıgely from standing water. Where calcite was not deposited, the solution of the limestone has left nodules of flint standing out from the walls of the caves. Similar nodules loosened from the limestone by solution and otherwise, have furnished the material for the polished pebbles of the caves and watercourses. The polishing was accomplished by movement in the water-courses, aided by the carbonic acid and the calcium carbonate carried in solution. (The polishing work of such waters may be seen in certain caves in the Copper Queen mine at Bisbee, Ariz.)

The topography of the region, the character of the pebbles, their depth below the surface, their relation to the water-courses, the smallness of the joint openings at the surface and the absence of similar pebbles on the surface all make it improbable that the pebbles came from the surface.

R. D. GEORGE

SPECIAL ARTICLES

UPON THE TEACHING OF THE SUBJECT OF RESPIRATION [1]

AT least three totally distinct definitions of the term respiration are expressed or implied in current literature. These and varying shades of meaning are often confused even in the same discussion, and the result is very unsatisfactory.

The first definition occurs in works upon the physiology of the higher animals. Among the different senses in which respiration is there used, one refers to the functions of lungs and gills, processes essentially secondary and which take place far away from the cell.

A second definition is found in general works and especially in botanical ones, namely, that respiration is an exchange of gases, a sort of commerce between the cell and its environment. A majority of our botanical text-books give a categorical definition something as follows. "Respiration is the taking in of oxygen and the giving out of carbon dioxid and water." To this is often joined the idea that the amount of carbon dioxid given off is equal to that of oxygen absorbed, that in fact the oxygen which enters is the same as that which reappears immediately as carbon dioxid, and not seldom, some emphatic and sweeping statement that the living substance must obtain oxygen somehow all the time.

A third and entirely distinct meaning is given to the word in more scientific works such as Pfeffer and in at least two American text-books, namely, that respiration is a vital operation taking place within the cell, a metabolic process in which energy is released and which is ordinarily indicated by the gaseous exchange mentioned in the last definition. The steps of this process are not well known, but any discussion of them seems to include also anaerobic or intramolecular respiration and certain kinds of fermentation.

[1] Read before the Botanical Society of America at the New York meeting, December, 1906.

The confusion in words is inconvenient enough, but there is back of it a confusion of ideas which is more serious, and by which the teaching of the subject is more or less impaired. From the standpoint of the teacher it is imperative that the subject be cleared up somewhat. However, before dealing with the appropriateness of definitions, let us briefly look over the phenomena which we have to deal with. The most common forms of apparatus used by teachers in this country in the study of respiration are U tubes or thistle tubes in which flowers or germinating seeds are placed. The end holding the seeds is sealed and the other end placed in some reagent which will absorb carbon dioxid or oxygen, or both, or in mercury which will absorb none of the atmospheric gases and serve as a control. With such arrangements it is easy to obtain satisfactory and instructive proof that oxygen is absorbed and carbon dioxid given off. If the idea were carried no further all would be well, but there is a temptation to bring in also quantitative results, and, pointing out the fact that caustic potash rises about one fifth of the volume of the tube, to imply that the oxygen contained has been combined in the activity of the plant into an equal amount of carbon dioxid. Remarkable quantitative result is it not, that if in an enclosed space there is a plant absorbing oxygen and a reagent absorbing carbon dioxid, the result should be a reduction of volume to that of the nitrogen present?

Ordinary experiments and bright pupils are a combination which is likely to cause a disturbance in formal ideas of respiration. Suppose, for instance, that three U tubes with germinating peas in the sealed end are set up, the open ends being in pyrogallol, caustic potash and mercury, respectively. The pryo rises rapidly by absorption of oxygen till it occupies about one fifth of the volume, the potash slowly does the same as the plants absorb the oxygen and give off carbon dioxid till it also reaches the one-fifth mark and the mercury does not rise at all. After some days the experiment looked upon as a perfect success is ready to be taken down. Theoretically, over the mercury also the oxygen has been absorbed and its place taken by carbon dioxid. Some student, not knowing any better, concludes to demonstrate that fact by introducing potash solution through the mercury. At once the mercury begins to rise in the tube, carrying the potash before it. The student watches to see it stop at the one-fifth mark and is astonished to see it continue to mount until it has reached two fifths or even more. Then the best the teacher can do is to clumsily explain that here is something he had not meant to demonstrate; that this is *intramolecular respiration,* that carbon dioxid has been produced in quantities and that the mixture of carbon dioxid and air has bubbled out through the mercury, some of the nitrogen thus escaping. Now it will at once be urged that some other seeds than peas should have been used for the experiment. But one may at least inquire why. Peas are living things and they are convenient. Moreover, if seeds in any considerable bulk are employed over mercury, the teacher must choose the seeds very carefully indeed if the idea of an equal exchange is not to suffer. If a series of objects are used, alterations of volume will occur in nearly all. In chrysanthemum flowers and peas the volume increases. In beet, turnip and timothy seed the volume diminishes. Facts such as these have long been known. It is well known, for example, that in oily seeds generally, oxygen absorption at first outruns production of carbon dioxid. Now after such a brief and fragmentary consideration of the experimental side, let us take up the question of definitions.

In regard to the first definition (that respiration refers to the functions of lungs and gills) it may be merely noted that it is of application only in the case of differentiated animals and the question of use must therefore be left to students in that field. It may be pointed out in passing, however, that respiration in this sense is so firmly imbedded in literature that it will probably retain the meaning it has, and that this meaning is so distinct that it will be little source of confusion. For the botanist the discussion must be between the following definitions and here confusion of thought arises very easily.

In regard to the second definition (that respiration is taking in oxygen and giving out carbon dioxid) it may be observed that it is easy of demonstration and is remarkably clear of statement and these features have probably given it its wide currency. But when we press for the conception lying back of the definition, for the idea which the words convey, it seems to be little more than a physical process of diffusion. On this account a telling objection can be raised. Granting that the gaseous exchange is easy to demonstrate and that the definition is exceedingly clear, which the writer is freely willing to do; granting that only aerobic respiration need be referred to in general teaching, which the writer is unwilling to do, it is still open to the fundamental criticism that it turns the student's attention away from the vital and really important process to a superficial and physical one. For far and away the most important idea in the teacher's subject matter is this: The living substance must have energy; it can get it only by working changes in the compounds within reach in such a way as to release energy. If oxygen is at hand these changes are largely those of oxidation. If oxygen is lacking the cell will find another way. To define respiration then as a gaseous exchange is to turn away from the all-important process. In this connection it may also be noted that to imply in addition that the carbon dioxid produced is equal to the oxygen absorbed amounts to positive error as does also the hard and fast statement that all living matter must obtain constant supplies of oxygen.

In regard to the third definition it may be observed that it refers to processes which are somewhat obscure and which are, after all, covered by the term metabolism. Nevertheless, it points to the really important vital process and it is broad enough to be in harmony with all the facts we know. (If respiration is taking in one gas and giving out another, 'intramolecular respiration' is an absurdity in words.) There seems scanty warrant for the custom of setting aside anaerobic respiration as though it were something entirely different or pathological or unusual. That aerobic and anaerobic respiration and even fermentation are closely connected, has for some time been recognized and has been brought out with especial clearness by Barnes.[2]

It may be urged that respiration in this sense is too abstract and complex to be a profitable subject for general teaching. With this view the writer does not agree. Certainly no amount of obscurity or complexity in an essential process can warrant the adjustment of definitions and teaching so as to lay the emphasis on the superficial accompaniments of that process. It seems to him that fundamental conceptions are the very ones to strive for in general teaching. Teaching may not go very far, but it ought to point in the right direction.

The suggestion made by Barnes[3] that a new term 'energesis' be adopted would be an admirable one, but the word respiration has so long been prominent that it seems unlikely that it will ever be permanently associated with a minor meaning. Moreover, as already stated, some of the best authorities already use it in referring to the metabolic process of energy release. For these reasons the writer thinks it probable that this third definition is the one which will stand. If this opinion is well founded, it becomes the duty of teachers to adopt the latter definition forthwith. Whatever is done, the vital process is to be kept in the foreground in teaching.

The subject of respiration certainly deserves a more adequate treatment in our courses. The writer knows as well as anyone that topics are many and hours are few, but are there not subjects in all our courses that might better be omitted than a fair consideration of a process which is essential and universal? Fundamental ideas penetrate the student's mind but slowly, and the writer feels that two or three experiments on the great question of respiration are not too many. After an experiment of some kind bringing out clearly but only quantitatively the fact that oxygen is absorbed and carbon dioxid given off, let another be set up which shall take some account of quantities—of different phases of respiration. Peas and oily seeds are valuable objects for this purpose. Explanation of the real nature of respiration and of the different kinds of respiration—anaerobic, fermentation, etc., will then follow quite naturally.

When students are using U tubes or thistle tubes there is always a possibility that the sealed end is not perfectly air tight. Eudiometers over mercury are an admirable form of apparatus, for here the question of leakage is removed from discussion, and quantitative readings can be made readily and with accuracy. The seeds are held in the upper end of the tube by glass wool, and in starting the experiment the mercury column is introduced some distance into the tube so that either increase or decrease in volume can be noted. The carbon dioxid evolved can later be directly and accurately measured by introducing caustic potash through the mercury.

After a well-planned series of experiments it is not difficult to question a class till most of them perceive that the various phenomena can be understood only in the light of the fact that in each case energy is set free. The content of the teacher's explanations would then be something as follows: By respiration we understand the changes brought about by the cell whereby energy is released. A supply of energy is a *sine qua non*, for life and reactions which yield energy are universally carried on by living substance. Ordinarily respiration is a process of oxidation, indicated both by an absorption of oxygen and a production of carbon dioxid. A supply of free oxygen seems to be necessary for protoplasmic movement and growth, and for con-

[2] 'The Theory of Respiration,' Science, February 17, 1905.

[3] *Loc. cit.*

tinued existence in most of the higher plants. The carbon dioxid given off is sometimes equal in amount to the oxygen taken in, but this ratio is a variable one. Most plants are able to respire for limited periods without taking in oxygen. Some of the lower plants are able to do this for prolonged periods—perhaps for a whole life cycle. This phase of the process is called intramolecular or anaerobic respiration and is much the same as certain kinds of fermentation. The steps of the processes of respiration are imperfectly known.

Charles H. Shaw

QUOTATIONS

'NEWSPAPER SCIENCE'

In the last number of Science, a correspondent, dealing with 'fakes and the press,' urges Congress "by some legal enactment to check the publication of all items that convey erroneous impressions relative to matters in which the whole community is interested." The immediate occasion of this somewhat sweeping recommendation is the wide publication late in January of a paragraph to the effect that the Jamaica earthquake, by disturbing the subterranean strata, had increased the flow of oil from the wells of northern Texas and Louisiana, while diminishing that from the southern counties of both states. The tale, it appears, was not true, and certain geologists declared at the beginning that it could not possibly have been true. Nevertheless, the writer is unfortunate in having illustrated his arraignment of 'newspaper science' by an example of a plain 'fake,' which is not typical of 'newspaper science' at all.

If the term has any meaning, it applies not to malicious stock-market rumors, or to wild fantasies which no one would pretend to take seriously, but to the journalistic treatment of matters that have really a scientific status. Just now 'newspaper science' is concerned chiefly with the weight of the human soul. Two days ago some Boston physicians announced that they had demonstrated a loss in weight of from one half ounce to one ounce at the moment of death, or a little later in the case of very slow-witted persons. This morning 'an eminent physiological chemist' adds the information that 'a group of German students' determined years ago that a mouse lost a milligram of weight when it died in an open vessel, but not when in an hermetically sealed bottle. We feel sure that this is not the last, but confidently await the story of the Indiana investigator who found some years ago that the soul of a lizard could be made to keep for years in any climate when contained in a bottle of pink glass. All this will be very interesting, it harms nobody, and at the beginning of the serial story there was some definite information furnished by men of standing.

There is much less guile about newspaper science than the laboratory scientists would have us believe. For instance, last fall, if we may infer causes from results, a newspaper correspondent at a small western lake resort picked up on the beach a defunct specimen of the common 'mud puppy.' He had never seen such a creature before, and took it to a local naturalist, who told him something about its amphibious habits and superabundant breathing apparatus. He also gave the generic name, *Necturus,* which the correspondent or the telegraph operator afterwards misspelled in the account of the monster—its size was a detail not mentioned—which possessed both lungs and gills, four legs, a mouthful of sharp teeth, a long tail, and was 'believed to be the Nocturis.' The finding of a mud puppy would have been no news at all, yet by means of this ingenuous dispatch, some perfectly correct and remarkable bits of scientific information were brought within the reach of a million newspaper readers.

By and large, there are probably quite as many commonplace and elementary scientific facts thus exploited as there are outright fabrications. Set any layman to report the lecture of a scientist who, in going over the field of his own specialty, mingles old with new matter, and he is just as likely to hit upon the former as the latter. If he is looking simply for sensation, he will send out an account, like so many which have emanated from the University of Chicago, bearing very little relation to anything a competent scientist could have said, but is not likely to have the diabolical ingenuity that sometimes is put into the political 'fake.' The corroborative detail which could give verisimilitude to, say, the story that man is in danger of being ousted by birds from the primacy of the animal kingdom, could only be supplied by the trained scientist, and the trained scientist is above doing such a thing.

A dozen of the colleges are attempting to disarm the unscrupulous journalist by sending out with their own *imprimatur* accounts of the discoveries made by members of their faculties. If this does not drive out the mendacious reporter, the fault can perhaps be justly laid at the door of science herself. Man continues to ask questions about his environment, and to want them answered. Yet it is growing increasingly hard to make real science intelligible. We may instance the perennially interesting sea-urchin eggs of Professor Jacques Loeb. His experiments in developing them covered a long period. Every now and then he established a new point. To the initiated these steps were as distinct as the several equations in an algebraical demonstration; yet to the layman the official accounts of these experiments read so much alike that headline writers hardly attempted to distinguish them, and many undoubtedly thought that the same news was being sent out over and over again. If really epochal discoveries are of such character that they can no longer be described in plain language, the temptation arises to invent discoveries which, though they never really occurred, are perfectly easy to talk about.

Nor, in spite of the scientists' insistence that plain reporters ought not to write about things they do not understand, is the confirmation of a recognized authority a perfect safeguard against the charge of inaccuracy. The *Evening Post* has printed accounts of scientific matters taken down verbatim from the lips of the leaders in particular branches of science, and had them disputed vehemently by other scientists. As the scientific tortoises creep on slowly from point to point, they do not always agree who is ahead; but that, we admit, does not make any less reprehensible the conduct of the irresponsible cottontail who occasionally jumps a few rods ahead of the whole lot.—*The New York Evening Post.*

BOTANICAL NOTES

STUDIES OF TEXAN VEGETATION

From time to time it has been a pleasant task of the editor of these notes to call attention to the work of Dr. Bray, of the University of Texas, on the vegetation of his state. The vastness of the territory covered, and the exceedingly varied character of soil, temperature, rainfall and other factors make the task of the botanist one of unusual difficulty. When it is remembered that Texas covers an area nearly as large as the northeastern states from Maine to, and including, Ohio and Virginia, and that north and south its length is about that from Boston to Charleston, and its width east and west about that from Boston to Chicago, one may begin to appreciate the amount of labor involved in what Dr. Bray has already accomplished. He has now added to his previous publications a paper entitled 'Distribution and Adaptation of the Vegetation of Texas,' and published as Bulletin No. 82 of the University of Texas. He tells us that it "was begun under the stimulus of desiring to present to the teachers in the public schools of Texas a point of view from which to study the vegetation of the state." He hastens, however, to say that "the aim is not to supplant other phases of botanical study, but to supplement them," which indicates that the author is not one of those who think that a general observation of the plants of a region, without their particular study, should constitute the content of a course in elementary botany.

The pamphlet, which includes 108 pages (and 14 plates), first takes up the 'Factors of Plant Environment and how they affect Plants,' to which about one half of the space is given, and this is followed by a discussion of the 'Plant Societies of the Texas Region.' In the first the rôles of water, temperature, light, the atmosphere, soils and biological factors are discussed clearly and helpfully. For it must be borne in mind that the purpose of the publication is to help teachers and others who need help, and this is the key to the treatment throughout its pages. The second general topic includes woodland vegetation, grassland vegetation, desert vegetation, water vegetation, salt-water vegetation, the vegetation of alkaline soils, and sand vegetation. Here again the author has made a very clear statement of the subjects taken up, and no teacher will find much difficulty in following and applying his discussion. The pamphlet should prove of great value to the teachers and pupils in the schools and colleges of Texas.

GARDNER'S STUDIES OF THE CYANOPHYCEAE

In the November number of the University of California Publications Nathaniel L. Gardner publishes an interesting paper on the blue-green algae under the title 'Cytological Studies in Cyanophyceae,' in which he reviews the work of previous investigators, and adds many observations of his own. He refers to his good fortune in being located where there is an abundance and variety of material at all seasons of the year, yielding him over one hundred species which he has collected and studied. In his investigations he has been very ingenious, as when he separates Oscillatorias and related forms from sand by making use of their motility, the threads 'crawling' out of the débris and this giving him pure cultures with little difficulty. He devised also an ingenious method of getting end views of *Oscillatoria* cells without having to make microtome sections. This is done by killing the plants in a strong solution of iodine in potassium iodide (ten to thirty minutes) and then washing in 95 per cent. alcohol (ten to thirty minutes). If now the filaments are mounted in water and subjected to a slight rolling pressure under a cover glass the cells will separate and fall over, giving excellent end views. Ten pages are given to a comparison of the conclusions reached by different investigators, notably Schmitz (1879), Kohl (1903), Phillips (1904) and Olive (1904), and this is followed by a discussion of cell contents, including the nucleus (for these plants have a nucleus of a simple kind), the granules and the cytoplasm. Some studies were made of the products of assimilation, resulting in finding glycogen, but not starch. Finally he finds much similarity between the *Cyanophyceae* and certain bacteria. A helpful bibliography, and six beautiful plates, mostly colored, close this important contribution.

SHORT NOTES

With the January number the *Plant World* enters upon the tenth year of its existence. The business management is changed, the place of publication being Denver, Colorado, and there have been some changes in the editorial management and policy. On the title-page the subtitle has been changed to read 'A Magazine of General Botany' instead of 'Popular Botany.' The purpose of the journal remains unchanged, namely to present botany in a non-technical form, for general readers, students and teachers. Professor Lloyd, now of Tucson, Arizona, continues as managing editor, and Miss Bracket, assistant editor. The other members of the editorial staff are members of the staff of the Desert Botanical Laboratory, and the Arizona Experiment Station. It is likely, therefore, to have a distinctly western flavor, and may thus appeal to a much larger constituency. There is a place for a journal of this kind which will be helpful to the beginner and the young teacher, and which at the same time will be full of information as to botanical matters. It must be suggestive and helpful in regard to the many details in the work of student and teacher, yet in order to be a mere day-by-day guide, it must do more for its readers, by leading them into broader and higher fields of thought and activity.

The twenty-fifth Heft of Engler's 'Pflanzenreich' is devoted to a monograph by Fr. Buchenan, of the Family *Juncaceae,* and makes a volume of nearly three hundred pages. The first thirty pages are given to an introduction in which structure is especially emphasized, with paragraphs on geographical distribution, relationship, uses, etc. Eight genera are recognized, viz.: (1) *Distichia* (3 South American species), (2) *Patosia* (1 Chilian species), (3) *Oxychloe* (2 South

American species), (4) *Marsippospermum* (3 South Pacific species), (5) *Rostkovia* (1 Antarctic species), (6) *Prionium* (1 South African species), (7) *Luzula* (61 species, widely distributed), (8) *Juncus* (209 species, widely distributed). The monograph is illustrated by 121 cuts in the text, including approximately four hundred individual figures.

Professor Doctor B. L. Robinson's address on 'The Problems of Ecology,' given at the Congress of Arts and Science, during the Exposition at St. Louis, 1904, has been reprinted as a twelve-page pamphlet. In speaking of Ecology he closes with these significant sentences: "Dealing as it does with the vital relations of plants to their surroundings, it yields information of the highest importance to the farmer, nurseryman and landscape gardener. Indeed it bridges just that all too wide gap between theoretical and applied botany, connecting the abstruse fields of plant anatomy, plant physiology and classification with the concrete applications of botany in agriculture, horticulture and forestry. The ecologist will never lack that wonderful stimulus which comes to the investigator who is conscious that his work is important to the welfare of his fellow beings, and intimately bound up with human progress."

THE NORTH AMERICAN FLORA

LAST October Part 1 of Volume 7 of the 'North American Flora' was received by subscribers for this work. It was devoted to the *Ustilaginales* (smuts) and was from the hand of Dr. G. P. Clinton, a specialist in this group of plants. Two families (*Ustilaginaceae* and *Tilletiaceae*) were monographed, the first containing 11 genera and 133 species, and the second 8 genera and 78 species. We have now another part (part 2) of the same volume, continuing the paging from 83 to 160, and devoted to the *Uredinales*. This part is by Dr. J. C. Arthur, who is well known as the foremost American student of the rusts, and whose contributions have often been noticed in these columns. He divides the order into three families (*Coleosporiaceae, Uredinaceae* and *Aecidiaceae*), the first and second of which are completed, the third (and much the largest) being broken off near the end of the fourteenth of its thirty-seven genera. The author follows the general outline given by him in a paper presented at the International Botanical Congress at Vienna last year, with some modification, however. A fuller notice is reserved until the completion of the monograph.

The four parts now published enable subscribers and others to get some idea of the bigness of the undertaking on the part of Dr. Britton and his colleagues to bring out a complete flora of North America. These parts average 88 pages each, and they have appeared at intervals which average about seven months in length. At this rate none of us would live to see the completion of the great work, but it is to be supposed that the parts will soon begin appearing at much shorter intervals. In the meantime it is evident that every working botanist, and every department of botany in every college and university, will have to become a subscriber to this greatest systematic work ever projected for any country.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

THE NEW CHEMICAL LABORATORY OF THE RENSSELAER POLYTECHNIC INSTITUTE

THE building is of four stories, and built of Indiana limestone and Harvard brick with roof of copper. It is entirely fire-proof, the partitions being of hollow brick, plastered, and the floors of concrete with a terrazzo finish. The woodwork trimmings of doors, windows and cases are of oak. The framework of the building is of steel construction. The staircases are of iron with treads of Tennessee marble. The hallways are tiled up to seven feet from the floor with 3 x 6 white tile, the baseboard being of Tennessee marble. The windows are very large and the glazing is of plate glass.

First Floor.—Assay Laboratory (54 x 52 ft.). This laboratory furnishes desk accommodation and furnace room for seventy-four students at once, which is the largest fire-room accommodation in the country. The furnaces consist of eighteen large muffles for scorification and cupellation and fourteen pot furnaces for crucible work. Each furnace has a separate flue. The students' desks are topped with an inch and a half slab of Alberene stone and each contains drawers for the accommodation of apparatus. The supporting legs are of iron. Water is supplied at the ends of the rows of desks, where are situated sinks of Alberene stone. The other furniture of the laboratory consists of anvil blocks, tables for bucking plates, ore-crushers and racks for cooling crucibles.

Quantitative Laboratory (44 x 16 ft.). This laboratory has accommodations for twenty students at a time in quantitative analysis. It is excellently lighted. The students' desks are topped with Alberene stone supported upon iron supports and containing four drawers for apparatus, with storage room below each desk for larger pieces. The large hood accommodation is provided with hot plates and water bath, the hoods being made of angle iron painted with aluminum paint and are glazed with corrugated glass. Water is supplied in sinks of solid porcelain. Gas, air blast and suction are also at hand. A special weighing room (7 x 16 ft.) opens off this laboratory, where the balances are supported on Alberene stone bracketed to a brick wall of especially heavy construction. Next to this balance room is a second balance room similarly fitted up and containing the assay balances for the students in the assay laboratory.

Instructors' Room (19 x 13). This room opens both into the assay laboratory and into the quantitative laboratory and is fitted with hood, water, blast, suction and the other requisites for a complete private laboratory for the instructor in that department. It is also arranged to accommodate the supplies of materials issued to students as examination topics in both the courses over which it has control.

The Stock Rooms are four in number (9 x 16, 12 x 26, 9 x 16, 5 x 16) and are fitted with appropriate shelving to accommodate the chemicals and apparatus required for general laboratory purposes.

The Sulphuretted Hydrogen Room (8 x 6) is ventilated by special device directly into the open air and contains the sulphuretted-hydrogen apparatus from which the gas is piped to the thirty-six outlets in the main qualitative laboratory on the floor above. The gas is delivered under a pressure of about ten inches of water.

Fuel Bins (11 x 8), for the coal and coke necessary for the assay laboratory, are arranged so as to permit the fuel to be shot through coal-holes in the roof, which is on the level of the roadway above.

Second Floor.—Main Qualitative Laboratory (50 x 80 ft.). This room is lighted by large windows on four sides and by a skylight extending over half its area. On each end are the hoods made of metal and glass, eight in number, and each 6 feet in length, making a total hood accommodation of nearly 100 linear feet. These hoods contain the hot plates and the steam baths for boiling and evaporation and they also contain the thirty-six outlets wherefrom students can secure sulphuretted-hydrogen gas for purposes of precipitation. The steam baths are two in number, with accommodations for fifty steam evaporations at once, the steam being admitted to chambers of Alberene stone directly from the steam pipes of the heating system. Each hood has its own individual outlet flue for fumes. The laboratory has accommodations for one hundred and thirty students at a time in qualitative analysis. Students' desks are topped with Alberene stone and are furnished with four drawers and two closets for apparatus. Water is supplied to each desk and the sinks are of solid porcelain. Each student is provided with forty reagents in glass bottles which are supported by shelves of plate glass resting upon metallic uprights. The heating of this laboratory, as of all other rooms in the building, is furnished from the steam plant in the department of electrical engineering. Radiators are everywhere placed under the windows, and in addition to the heat so supplied a further quantity is furnished by the ventilation system. Air is sucked in from the outside, passed over heated steam coils, and is then blown by an electric fan through ducts opening in the side walls of the laboratory. Fumes pass out not only through the individual fume flues of the hoods, but also through numerous flues opening a little below the ceiling along all the walls. These flues have also openings near the floor, so that either opening may be used as occasion demands. Spectroscope accommodations are furnished in the four corners of this laboratory. One feature worthy of notice is the broad eight-foot aisles between the lines of desks. This gives abundant room to every student.

Supply Room (6 x 16). This room is of two stories and is intended to be used for the issuing of reagents and as a storage room for smaller glass apparatus.

Instructor's Room (12 x 16). Because of the great height of its ceiling this room has a gallery around two sides, thus greatly increasing the storage space for chemicals, because of the increased wall space for shelving. The distilled-water apparatus is here located and the supply of distilled water is here stored in a tin-lined copper tank, the water being tapped off therefrom to a spigot in the main laboratory. The room is equipped with complete apparatus suitable for a private laboratory.

Ward Room (10 x 16). Fitted with the usual accommodations for receiving hats and coats of the students.

Organic Combustion Room (8 x 16). This room is fitted with Alberene stone tables, gas, blast and suction and is arranged to receive the combustion train for the usual work in organic combustion.

Third Floor.—The Lecture Room (50 x 41 ft. 6 in.) has seating accommodations for two hundred men. It contains the large lecture table with pneumatic trough and the other essentials to fit it for lecture uses. It is lighted by three small and six very large windows. On the wall opposite the lecture table there projects a gallery intended to hold the electric lantern for illustration work. This gallery is entered from the floor above.

Private Laboratory (16 x 24). This room is fitted as a private laboratory for the professor of chemistry and contains the usual complement of heeds, water baths and other appliances suitable to such use.

The Laboratory for Gas Analysis (11 x 16) contains Alberene-stone table, water, gas and blast accommodations, with suitable shelving to accommodate the apparatus for which the room is intended to be used.

The Special Laboratory (10 x 16) is fitted in the same manner as that of gas analysis and is intended to be used for such special work as the examination of food products, etc.

Fourth Floor.—Water Laboratory (20 x 31). This laboratory is fitted with Alberene-topped tables and with water appliances suitable to the very complete examination of questions dealing with the examination of potable, mineral and boiler waters. Appliances are here established for undertaking such examinations from both the chemical and the bacteriological sides. An especially devised table for the determination of free and albuminoid ammonia permits of the analysis of six waters at once. Suitable provision is made for the sterilizers, incubators and other apparatus peculiar to a water laboratory.

Section Room (20 x 20). This room is fitted with blackboards and is intended for use as a recitation room in chemistry. It has accommodations for a section of about twenty-five men at a time.

The Halls.—The halls throughout the building are eight feet in width.

Lighting.—The building is piped for gas, but it is as a precaution only, inasmuch as electricity is to be depended upon for lighting purposes.

W. P. Mason

ALLAN MACFADYEN

At the early age of forty-six years Dr. Allan Macfadyen has been taken away from his work. By an accident in the laboratory in which he was working a preparation of the serum for Malta fever infected him—the infection seems to have been through the eyes—and death carried him away. Professor Macfadyen was educated in Edinburgh and graduated in 1886. Two years later he took his B.Sc. and then studied abroad at Berne, Göttingen and Munich. There he flung himself upon laboratory methods and bacteriology and in 1889 became professor of bacteriology in the College of State Medicine in London. He was associated with Lord Lister, Sir Joseph Fayrer and others in the inception and foundation of the Jenner Institute and was himself director of the institute. Under his direction the new and splendid laboratories were built at Chelsea. That institute is now known as the Lister Institute and into it Dr. Macfadyen built some of the best years of his life.

But that which will give him a permanent place, according to the London *Lancet*, in the history of science is his experimental work on the intracellular toxins of bacteria with which his name is so intimately associated. His many valuable papers to the Royal Society and scientific journals, English and German, testify to his activity in the investigation of important matters relating to preventive medicine. They run over a wide range of subjects, but by far the most important, as they will probably be the most enduring, are his studies on the intracellular toxins. After resigning his position at the Lister Institute, where his persistence in this line of research was, we must suppose, unappreciated, although it had the support of Lord Lister, he pursued his investigations at King's College and at the Wellcome Laboratory. Concerning his work there a friend writes: "Macfadyen's view was that serum therapeutics had reached an impasse, owing to the great difficulty of producing efficient antibodies for intracellular toxins, and he made a profound study of the delicate and volatile nature of the most active toxins and the destructive effect of heat and other agents upon most of them. He had prepared from the endotoxins of the bacilli of typhoid fever, cholera, pneumonia and other diseases serums of higher antitoxic power than had ever been obtained before. At the time when he became ill he had succeeded in his anticipation with the plague endotoxin and was working also at Malta fever. He expected to have brought to completion in the course of three or four months a research which had engaged his attention for years and which would have brought the sera into use. His anti-typhoid serum has already begun to be employed in some of the London hospitals. But, alas, it was not given to him to finish his work."

Dr. Macfadyen had made a reputation for himself as a popularizer of science. In his lectures before the Royal Institute he attained a distinct success as a public speaker. He was married to Miss Marie Bartling, the daughter of Professor Bartling, director of the Botanical Gardens at Göttingen. He leaves a widow but no children. Many of his pupils are in Canada and in this country and from all over the world expressions of sympathy have been received from those who worked with him in his laboratories at Chelsea.

GOVERNMENT APPROPRIATIONS FOR SCIENTIFIC PURPOSES FOR THE FISCAL YEAR ENDING JUNE 30, 1908

THE following list of appropriations for the fiscal year ending June 30, 1908, for the government scientific bureaus has been compiled from the various congressional appropriation acts. It is not an official summary such as will appear later in the digest of appropriations published by the division of bookkeeping and warrants of the Treasury Department.

Besides the bureaus included in this list are a number of departmental interests which involve the direct application of science in one form or another. Under the Treasury Department, for instance, the supervising architect's office, the office of the director of the mint, and assay offices, the bureau of engraving and printing, and the whole of the public health and marine hospital service, are in a sense bureaus of applied science. So, too, under the War Department, the office of chief of engineers, the bureau of ordnance, the signal office, and the surgeon general's office, and under the Navy Department, the bureau of steam engineering, the bureau of ordnance, and the bureau of medicine and surgery might be called scientific bureaus. The lighthouse board of the Department of Commerce and Labor, and the Indian office and bureau of education of the Interior Department, are sometimes included among the scientific bureaus.

The list of appropriations for scientific purposes is as follows:

UNDER THE TREASURY DEPARTMENT

Hygienic Laboratory, Public Health and Marine Hospital Service	$90,000 00

UNDER THE NAVY DEPARTMENT

Hydrographic Office	$141,500 00
Naval Observatory	62,390 00
Nautical Almanac Office	21,240 00

UNDER THE INTERIOR DEPARTMENT

Patent Office	$1,288,150 00
Geological Survey	1,476,420 00

UNDER THE DEPARTMENT OF COMMERCE AND LABOR

National Bureau of Standards	$189,620 00
Coast and Geodetic Survey	992,316 40
Bureau of Fisheries	702,760 00

UNDER THE DEPARTMENT OF AGRICULTURE

Weather Bureau	$1,413,540 00
Bureau of Animal Industry	1,032,480 00
Bureau of Plant Industry	1,052,230 00
Forest Service	2,400,000 00
Bureau of Chemistry	697,920 00
Bureau of Soils	206,980 00
Bureau of Entomology	136,010 00
Bureau of Biological Survey	52,000 00
Office of Experiment Stations	1,013,220 00
Emergency Appropriations:	
Cotton boll weevil investigations	190,000 00
Prevention of spread of gypsy and browntail moths	150,000 00
Eradicating cattle ticks	150,000 00
Special Appropriations:	
Survey of Appalachian and White Mountain watersheds	25,000 00
Agricultural colleges, to each state and territory	5,000 00
Total for the Department of Agriculture, including building and deficiency appropriations	9,698,590 00

UNDER THE SMITHSONIAN INSTITUTION

International Exchanges	$ 32,000 00
American Ethnology	43,000 00
International Catalogue of Scientific Literature	5,000 00
Astrophysical Observatory	13,000 00
National Museum	250,080 00
National Zoological Park	110,000 00
Final appropriation for the new building for the National Museum	1,250,000 00
Total under the Smithsonian Institution	1,703,080 00

MISCELLANEOUS

Government Printing Office, printing for scientific bureaus	$824,450 00
Library of Congress	616,885 00
Botanic Gardens	29,893 73
Army War College	24,400 00
Naval War College	19,200 00
Army Engineer Survey of Northern and Northwestern Lakes	75,000 00
Division of Topography, Postoffice Department	47,900 00
Alaskan Seal Fisheries	11,430 00

SCIENTIFIC NOTES AND NEWS

ON the occasion of the dedication of the new buildings of the Carnegie Institution last week, honorary degrees were conferred by the Western University of Pennsylvania on a number of the foreign guests including Sir Robert Ball, Lowndean professor of astronomy and geometry in Cambridge University; Dr. P. Chalmers Mitchell, secretary of the London Zoological Society; Sir William Preece, the British electrical engineer, and Dr. F. S. Archenbold, director of the Treptow Observatory.

THE summer meeting of the American Chemical Society will be held at Toronto, June 27–29. The following persons will act as chairmen of the various sections:

Physical Chemistry: W. D. Bancroft.
Inorganic Chemistry: C. L. Parsons.
Organic Chemistry: J. B. Tingle.
Agricultural, Sanitary and Biological Chemistry: F. T. Shutt.
Industrial Chemistry: W. H. Ellis.

DR. ALEXANDER GRAHAM BELL will shortly go to England to receive the doctorate of laws from Oxford University.

PROFESSOR W. W. KEEN, of Philadelphia, a delegate to the Surgical Congress at Berlin, has been elected an honorary member of the German Surgical Society.

J. M. STEDMAN, professor of entomology in the University of Missouri and entomologist of the Experiment Station, has been granted leave of absence for seventeen months, which he will spend in study and in travel abroad, and also in research at the Naples Zoological Station.

Sir Philip Magnus, M.P., is engaged in an inquiry for the Cardiff Education Authority with reference to possible improvements in the technical instruction and other branches of education in the city.

The council of the Royal Geographical Society has awarded the Founder's Medal to Dr. Francisco Moreno, who has for twenty years been occupied in exploring South America, especially Patagonia and the southern Andes, and the Patron's Medal to Dr. Roald Amundsen, the Norwegian explorer, who recently completed the northwest passage for the first time in a ship. The Murchison bequest has been awarded to Captain G. E. Smith for his various important surveys in British East Africa; the Gill Memorial to Mr. C. Raymond Beazley for his work in three volumes on 'The Dawn of Modern Geography,' the result of many years' research; the Back bequest to Mr. C. E. Moss for his important researches on the geographical distribution of vegetation in England; and the Cuthbert Peek Fund to Major C. W. Gwynn, C.M.G., D.S.O., R.E., for the important geographical and cartographical work which he carried out in the Blue Nile region and on the proposed Sudan-Abyssinian frontier.

The Tiedemann Prize, awarded every fourth year by the Senckenberg Society at Frankfort a. M. to the German writer who has produced the best work along the lines of comparative anatomy and physiology has been awarded this year to Dr. E. Buchner of Berlin for his researches on fermentation.

On the invitation of President Schurman, Professor Burt G. Wilder, of Cornell University, will give a memorial address on May 28, to commemorate the one hundredth anniversary of the birth of Louis Agassiz.

The Princeton correspondent of the New York *Evening Post* states that Professor Alexander T. Ormond will lecture on philosophical and educational subjects before the Johns Hopkins University, the University of Virginia, the University of Tennessee, Grant University of Chattanooga, Vanderbilt University, the University of Georgia, Tulane University, the University of North Carolina, and the University of South Carolina.

Professor W. P. Bradley, of Wesleyan University, who designed the liquid air plant recently installed for the Sheffield Scientific School, Yale University, lectured on March 21 on 'Liquid Air as prepared and used at the Cryogenic Laboratory of Wesleyan College.'

At the meeting of the Davenport Academy of Sciences on March 30, Professor Herbert Osborn, of the Ohio State University, gave a lecture on 'Eccentricities of Insect Life,' illustrated by a series of lantern slides.

On the occasion of the fiftieth anniversary of his graduation in medicine, Professor Ernst Haeckel of Jena was made a privy medical councilor with the title of *Excellenz*. He practised in Berlin before taking up the work in natural science which made him famous.

The building erected by Mr. Andrew Carnegie for the United Engineering Societies was dedicated this week. On Tuesday afternoon addresses were made, if the announcements of the program were carried out, by Mr. Andrew Carnegie and President Arthur T. Hadley. In the evening there was to be a reception at which the officers of the different societies would receive in their rooms. On Wednesday afternoon addresses were announced by Dr. Samuel Sheldon, president of the American Institute of Electrical Engineers; Dr. Frederick R. Hutton, president of the American Society of Mechanical Engineers, and Dr. John H. Hammond, president of the American Institute of Mining Engineers. The John Fritz medal was to be presented to Dr. Alexander Graham Bell and medals for distinguished services to Dr. Ralph W. Pope and Professor Frederick R. Hutton. Meetings of the societies were to be held on Thursday and Friday.

The American Electro-chemical Society will meet at the University of Pennsylvania on May 2, 3 and 4, 1907.

The College of Physicians of Philadelphia announces that the next award of the Alvarengà prize, being the income for one year ($180) of the bequest of the late Señor Alvarenga, will be made on July 4, provided that an essay deemed worthy of the prize shall have been offered.

THE National Museum of Wales has obtained a charter of incorporation.

PLANS and particulars of land for the erection of the new Solar Physics Observatory at Hindhead have been placed before the British Minister of Education.

THE Lake Laboratory of Ohio State University announces for the season of 1907 courses in general zoology, embryology, entomology, ichthyology, ornithology, invertebrate morphology, experimental zoology, vertebrate comparative anatomy, aquatic biology, research work, general botany, ecology and special work in botany. The staff for the season includes, besides the director, Professors L. B. Walton, Ph.D., Kenyon College; Malcolm Stickney, A.M., Denison University; Lynds Jones, Ph.D., Oberlin College; Charles Brookover, M.S., Buchtel College, and W. B. Herms, A.M., Ohio Wesleyan University. The laboratory offers free tables for independent investigators and will welcome any who have problems in biology which can be studied to advantage at the laboratory. The laboratory will be open for instruction courses from June 24 to August 2 and for investigators from June 24 to about September 15. Applications may be sent to the director, Professor Herbert Osborn, Ohio State University, Columbus, O.

THE University of Wisconsin Agricultural Experiment Station is conducting a campaign against the spread of bovine tuberculosis among the 100,000 dairy herds of the state. A bill has just been introduced into the legislature providing for the testing of all cattle before they are sold. The existing laws, providing for the inspection of cattle before they are brought into the state, protect dairymen from infection from outside the state, and the faculty of the college of agriculture is working toward similar protection within the state to prevent the spread of tuberculosis from infected herds to others. Dr. H. L. Russell, of the department of bacteriology of the university, who is also a member of the Live Stock Sanitary Board of Wisconsin, has given instructions in the use of the tuberculin test to 1,200 young farmers from all over the state, former students in the college of agriculture who now compose the membership of the University Agricultural Experiment Association.

Nature gives the following scientific subjects for which prizes are offered by the Reale Instituto Lombardo for the Cagnola prize, April, 1907, on the discovery of radioactivity and its influence on modern physical and chemical theories; for 1908, on the present state of metallography in relation to the physical properties of metals, particularly iron and steel, a general summary including some original results for the Fossati prize for 1907, on the so-called nuclei of origin and termination of the cranial nerves; for the Kramer prize for 1907, a discussion with certain practical applications of Guglielmini's hydraulic theories; for the Secco Comneno prize for 1907, a discovery relating to the virus of rabies; for 1911, on the physiological action of high-frequency currents. As in previous years, other prizes are offered for literary and commercial subjects and for subjects which are the same every year. For the present year the prize awards of the Reale Instituto Lombardo include a Cagnola prize of £100 and medal of value £20 to Dr. Augusto Moschini, of Pavia, for his essay on the pathology of the suprarenal capsules; a prize of £80 to Dr. Guido Sala, of Pavia, and awards of £20 to Professor Domenico Lo Monaco and G. Pitò, of Rome, for essays on the anatomy of the visual centers of higher vertebrates under the Fossati foundation; and Kramer prizes of £80 each to Ernesto Canalli, of Naples, and Mario Baroni, of Milan, for essays on the resistance of structures in cement.

THE U. S. Geological Survey has completed a line of spirit levels through Death Valley, California, and has ascertained that the depth of that area is not so great as was supposed. The final computations of the results have not yet been made, but the preliminary figures give for the lowest point a depth of 276 feet below sea level. Bennetts Well, which is near this point, is 266 feet below sea level. These figures are probably not more than three feet in error. The Geological Survey now has elevation marks on the highest and lowest points of dry land in the United States. It is a coincidence that these two extremes are both in southern California and only 75 miles apart. Mount Whitney is a foot or two over 14,500 feet above sea level, while Death Valley, as above stated, is 276 feet below. Before the Salton Sink, also in southern California, was flooded by the Colorado River, it contained the lowest point of dry land in the United States, a spot 287 feet below sea level. Previous estimates of the depth of Death Valley based on barometer readings gave for the lowest point figures varying from 250 to 450 feet below sea level.

Nature states that at the annual general meeting of the Geological Society on February 22, Sir Archibald Geikie, the president, described the arrangements contemplated for the celebration of the society's centenary next September. Invitations to attend the meetings will be sent to all the foreign members and foreign correspondents of the society, and geological societies, geological surveys, and learned institutions which have a geological side, will be asked to send delegates. Personal invitations will also be addressed to geologists of note in the old and the new world, who are not already enrolled in the foreign lists of the society. The official program will probably extend over three days in London. The arrangements for each of these three days are under consideration, but Sir Archibald Geikie proposes to give his presidential address as the *pièce de résistance* of one of the forenoon or afternoon meetings. In that address he will offer a sketch of the state of geological science outside Britain at the time when the Geological Society of London was founded, and indicate the external influences that affected its start. By this choice of a subject he hopes to interest the foreign guests, while at the same time inviting the fellows of the society into a domain of the history of science which is perhaps less familiar than it deserves to be. The chronicle of the society itself during the first hundred years of its existence has been carefully and fully compiled from all available sources by Mr. Horace B. Woodward for publication in volume form. Excursions to places of geological note in Great Britain will probably be arranged, some to precede and others to follow the meeting in London. The various museums and places of interest in the metropolis will, of course, be shown to the expected visitors, and there will doubtless be no lack of public and private hospitality. It is anticipated that the Universities of Oxford and Cambridge will both receive the foreign guests.

UNIVERSITY AND EDUCATIONAL NEWS

The Drapers Company has decided to continue its grant of £800 a year for the stipend of the professor of agriculture at Cambridge for another period of ten years. The company has also given £200 for the completion of the astronomical equipment of the University of London.

Mrs. Russell Sage has given $150,000 to the Northfield (Mass.) Seminary for a new chapel and a music building.

The Johns Hopkins University will hereafter admit women to graduate courses in cases where no objection is made by the instructors. Women have been admitted to the medical department of the university since its opening in 1893.

The London University holiday course for foreigners will be held from July 22 to August 26.

It has been decided by the council of the University of Leeds to create a separate chair of botany. This has arisen out of the resignation of Professor Miall, who combined the teaching of botany with zoology.

Dr. W. Peddie, lecturer in natural philosophy in the University of Edinburgh, has been appointed to the Harris chair of physics in University College, Dundee, in succession to Professor Kuenen.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, APRIL 26, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

PRESENT-DAY CONDITIONS AND THE RESPONSIBILITIES OF THE UNIVERSITY[1]

IT is proper that the office which I have the honor of representing to-night should be a cynosure, but it is unfortunate when in connection with any office, be it never so exalted, there is a tendency to fall into the Malapropian error of confounding a cynosure with a sinecure. I presume that it is with a view to avoiding this danger that an address has become one of the recognized duties pertaining to the chairmanship of this society, and it is but natural that the address should consider some topic connected with our scientific activities. Arguing upon this line, I was tempted to accept this as an opportunity for proposing the toast of 'our noble selves,' and to discourse upon the remarkable growth of the scientific spirit in the middle west during the last decade and the part taken by the members of this association in that growth. I decided, however, that I could not do justice to such a theme, for although 'good wine needs no bush,' yet unworthy words may mar a good tale.

But while I may not linger upon so

[1] Address of the chairman of the Central Branch of the Society of American Naturalists at the meeting held at the University of Wisconsin, March 29, 1907.

enticing a topic, the circumstances of our meeting compel a word of tribute to the part played by the University of Wisconsin in the upward movement. Fifty years ago the state universities were almost wholly teaching institutions, colleges rather than universities, institutions influenced by rather than influencing the standards of the subordinate educational grades. Thirty years ago the foresight of President Gilman made the Johns Hopkins University an exponent of the university ideal, before then but imperfectly realized, but for which the times were ripe, and in the acceptance of this ideal the University of Wisconsin was no laggard. In 1887 she gave earnest of her ideals in appointing to the presidency Professor Chamberlin, active then as now in the advocacy of investigation as a function of the university and whose words, spoken on the occasion of the jubilee of this university, might appropriately be writ large over the portals of all our state universities—"Research in every realm of a people's legitimate interests is an appropriate function of the people's organized self, the state, and of the people's organized instrument of research, the state university." The appointment to the presidency, in 1892, of Charles Kendall Adams was a pledge that the movement towards the higher ideals should not be retarded, and under his administration and that of Professor Birge the growth of the university along university lines is evidenced by the increase in the number of graduate students from 3 in 1887 to 119 in 1903.

History repeats itself. The university movement began in Wisconsin under the presidency of an active investigator, an acknowledged authority in geology, and we may with confidence look forward to a maintenance of high ideals and a continuance of the progress towards their realization under the present incumbent of the presidential chair, who has also enriched the literature of geology with the results of many careful and thorough investigations. And may we hope, Sir, that the duties of your high position may not deprive us of your active participation in investigation, and that you may continue to manifest your interest in higher university ideals by example as well as by precept. It is a privilege, and I doubt not for many of us an inspiration, to see for ourselves what the State of Wisconsin, under wise guidance, is now doing for higher education, and it is equally inspiring to note the promise the university holds out of continuing in the path of progress and of being in the future, as she has been in the past, a zealous foster-mother of productive scholarship.

In university education, as well as in other mundane affairs "the old order changeth, giving place unto the new." Time was when the sciences were but tolerated in the university curriculum, when the cultivation of the humanities was regarded as the only path leading to intellectual light. But this has all been changed and the sciences have come into their own. No longer do we find the humanists endeavoring to maintain a position of extreme pharisaism, nor is 'culture,' a term much abused and suggestive of the hot-bed or conservatory, regarded as obtainable only by a course in the humanities. No, all this has changed, and just as the dominance in higher education of the *literæ divinæ* gave place to that of the *literæ humaniores,* so the humanities have yielded to the scientific discipline or at least have been forced to admit it to a position of equality.

The causes for this are not far to seek. They are the result of the necessity for keeping the lines of higher education in touch with human interests, and never have these interests been so bound up in the application and progress of scientific methods and discoveries as they are at the present time. The nineteenth century has been termed the age of science. How, I wonder, will the twentieth century be designated? For we are even now merely on the threshold of a period of scientific activity whose outcome is far beyond the ken of even the most imaginative seer. We must anticipate, nay, we must all rejoice, whether we be scientists or humanists, in the prospect of continually increasing scientific activity in the years to come and we must therefore look forward to a greater demand for the cultivation of the sciences in our educational institutions.

The developments in all departments of science in the last few years have been simply marvelous, and one does not need the shadows of even fifty years to bring into startling relief the enormous growth which has taken place not only in the extent and solidity of the foundations of science, but in the height, the adaptability, and, we may say, the elegance of the superstructures. I may be pardoned if, for emphasis, I introduce the personal element and state that even my span of life is coincident with that of the doctrine of natural selection, and that I can remember the interest and enthusiasm with which I read in my senior year at college the but recently published papers of Flemming and Peremeschko portraying the phenomena of karyokinesis and inwardly marveled that such observations, now the ordinary routine of an undergraduate course, were possible. Truly "the thoughts of men are widened with the process of the suns."

When one considers the relative recency of the discovery of the Hertzian waves and Röntgen rays, the actual novelty of our knowledge of radioactivity and of the ionic theory; when one notes the revival of the Mendelian law and the important observations on variation and inheritance which it has evoked; when one recalls the important contributions to our knowledge of the physiology and mechanics of growth which the modern science of experimental morphology has supplied; and, not to prolong the list indefinitely, when one reviews the recent advances in our knowledge of the principles underlying serum-therapy—how can it be doubted that we are but on the threshold of an era of scientific activity whose outcome will far transcend that of the century which has passed? The importance and far-reaching possibilities of the purely scientific problems now confronting us can not fail to stimulate investigation in all departments of science and must continue to attract, in increasing numbers, men who will find their greatest pleasure in the prosecution of science for the truths it will reveal. And, furthermore, the possibilities in the way of the practical application of the results of pure science, now seen as through a glass darkly, must create a continually increasing demand for men thoroughly grounded in scientific principles, who can make realities of the hopes which pure science awakens. So certain is this and so certain is the correlation between scientific progress and national development, that the fostering of scientific investigation must become, even to a greater extent than now, a question of national concern.

But even although the tendency is strong towards the maintenance of scientific institutes by the government and although we have already witnessed the application of private wealth to the establishment of special scientific laboratories, yet it is to the universities that we must chiefly look now and in the future for the maintenance in scientific work of the high ideals which are the life of scientific progress. For it is from the universities that the ranks of those who will serve in governmental and private laboratories must be recruited, and upon the universities rests the responsibility not only of adding directly to the world's stock of knowledge, but also of supplying men capable of grappling worthily with the problems which may confront them and imbued with a proper sense of the dignity of science and of the obligations it imposes upon its devotees. The task of the university is, therefore, a double one and doubly serious, and it may not be amiss to enquire into some of the conditions necessary for the accomplishment of its task.

The prime necessity, the selection of men for positions on the staff who are competent as both investigators and teachers, need hardly be considered. It follows from what has already been said. But this much may be added, that a competent investigator, even though he be but a mediocre teacher, will do more to fulfil the ideal for which a university should stand, than will a competent teacher who does not investigate. Men there have been, like the late Sir Michael Foster, whose death we all deplore, who while taking but little part in actual investigation, have nevertheless by the healthful stimulus of their teaching created a school of ardent and brilliant investigators. But such men are *raræ aves* and the zealous investigator by his enthusiasm and example will, as a rule, do more to raise the standard of scientific scholarship than will a non-investigating teacher. And, I believe that as a general rule the investigator will prove a more capable teacher than the non-investigator for the simple reason that he will be more apt to keep abreast with the progress of his studies and inclined to rely upon original sources for the information he imparts rather than to seek it in the more accessible text-books.

But, after all, this is a matter which does not require special comment. The governing bodies of our universities are coming to recognize more and more the necessities in the case and the standards of fitness for staff appointments are rapidly rising. More serious is the failure of the authorities to perceive the conditions necessary for the full fruition of scholarship. It is a sad comment upon the ideals of a governing body when it bases its estimate of the value of a teacher upon the number of hours he devotes to actual class work and to service upon various academic committees. And yet how many of us have heard such a standard of efficiency advanced in all seriousness. A high grade of scholarship can not be maintained, investigations of a high order can not be carried on by men whose physical and intellectual energy is exhausted by the routine of the class-room and executive cares. I would not for a moment contend that even the most capable of investigators should be entirely relieved of his duties as a teacher, indeed, I am convinced that from his teaching duties an investigator may obtain much stimulation and inspiration, but I do protest against a competent man being so burdened with class-room duties that he but half fulfils the responsibilities of his position. It is neither good business policy nor good ethics. It is the office of the university not only to impart knowledge but also to add to it, and the one duty is as obligatory as the other.

But the blame for the non-fulfilment of both duties does not in all cases rest entirely with the university authorities. Frequently it rests with the teacher himself, who, in a desire, in itself a most laudable desire, to make his teaching thorough to the limits of his ability, overburdens himself with multifarious courses. Such an one is proving false both to himself and to his university; he is failing to fulfil his responsibilities. Far better were it to teach thoroughly only the principles of his subject and to devote some of his energies to the advancement of knowledge in his chosen field. And this suggests a consideration of the effects of summer sessions, now so much in vogue, upon productive activities. I do not feel justified in giving at present an *ex cathedrâ* pronouncement upon the merits of summer sessions; they seem to make for both good and ill; but whether the benefits derived from them compensate for their ill-effects in other lines remains for the future to determine. If they can be conducted on lines which will suppress an imminent danger of superficiality and which will not interfere with the investigational activities of members of the university staff, by demanding that men who have already spent nine months of the year principally in class-room work shall devote to similar work an additional period of six weeks or more of the time they have for uninterrupted devotion to investigation, if these dangers can be avoided the summer session is justifiable; otherwise its influence is pernicious.

One of the allurements of the summer session is the opportunity it affords for a small addition to a diminutive income. And in the necessity for this lies one of the obstacles to greater scientific achievement in the universities. The *res angusta domi* do not conduce to that condition of equanimity necessary for good scientific work and many a promising investigator has had his ambitions quenched and his mind turned to the more pressing material necessities of life by the lack of sufficient recompense for his work. This is a matter, however, to which attention has frequently been called of late and, it is a pleasure to say, with some prospect of remedy.

I have already pointed out as one of the causes for the position scientific studies now hold in university education, the demand for men trained in scientific methods. And, as is so often the case, the favoring current carries with it seeds of danger to true scientific progress. This danger is the commercialization of the university, and it is one which in this country, more than in any other, needs careful watch and ward. The university has been satirically defined as a place where nothing useful is taught and, taking the word useful in its intended meaning, I hold that the definition, intended as a reproach is an honor. What the university and university education should stand for is not utilitarianism, its function is not to turn out masters of the technicalities of this or that profession, but above all men with a sound training in fundamental principles. That the university should offer lectures and other forms of instruction in the history and theory of music or painting is right and proper, but that it should turn out expert pianists or finished artists is absurd. We may even look with equanimity upon courses in domestic science or on properly conducted commercial courses, but that the university should descend to the education even of good cooks or successful drummers is something horrible to contemplate. And so with the sciences. Let the first care of the university be to thoroughly educate men in the principles of the sciences, and worthy results will inevitably follow, but should the university become a technical school progress will be retarded. I do not mean to say that the practical application of scientific principles should be absolutely disregarded in the university; far from it. For the *argumentum ad rem* is often the most powerful means for pressing home a scientific deduction. But what I do maintain is that it is the teaching of the principles of pure science which underly practical application that should be the essential function of the university, its aim should not be to turn out engineers, architects, physicians, pharmacists, or dentists, but to furnish men thoroughly grounded in the principles upon which the successful and scientific practise of these professions depends.

All this will undoubtedly be regarded as purely academic theorizing by the Gradgrinds who arrogate to themselves the adjective 'practical.' But which is more practical, more beneficial to the individual and to the community which he serves, the education of an empiric or the training of a scientist? Surely there can be no hesitation in the answer. Montaigne said, long ago, 'To know by rote is not to know.' It would be instructive to test our university education by this standard and ascertain how far, by precept and example, it is, especially in the professional schools, following the straight and narrow path. To what extent is the desire for immediate financial success, and that on no modest scale, affecting the work of our students? Are our faculties yielding to this desire on the part of the students and in their teaching placing more stress on the application than on the principle? And to what extent is this same desire for financial prosperity calling our teachers from investigation to more lucrative employments and impairing their usefulness both as preceptors and as exemplars? I have not considered these questions as part of my theme, and will leave them with you for private consideration.

In the development of investigation as a prime function of the university, there is a danger, however, that its advancement may be pushed too rapidly, in the sense that men, too slightly grounded in the principles of their science, may be pushed into special lines of study and that university education may mean the training of narrow specialists rather than the development of broad, scholarly minds. Lowell said of Harvard that he "would rather the college should turn out one of Aristotle's four-square men, capable of holding his own in whatever field he may be cast, than a score of lop-sided ones developed abnormally in one direction," and surely this is what each of us would wish for his alma mater or for the college in which his lot is cast. It is a shortsighted policy that forces or even allows immature men to enter upon investigation. It may increase the quantity of the productive work of the university, but the increase will be at the expense of the quality, and in the long run will redound to the credit neither of the university nor of the individual members of its staff. And, after all, the investigator is born, not made, and for the men who have not an innate aptitude for investigation,

> Selbst Pallas kommt als Mentor nicht zu Ehren,
> Am Ende treiben sie's nach ihrer Weise fort
> Als wenn sie nicht erzogen wären.

All students can not be investigators in the ordinary sense of that word, but all should be trained along broad lines, trained to look to original and reliable sources for their information, trained to seek for the causes of phenomena and events, trained, in short, in the methods of the investigator. Only after a student has successfully undergone such a discipline, and surely the ordinary undergraduate course is none too long for its completion, should he be allowed to undertake investigation. The tendency to make a certain number of years of college training a condition for entrance upon a professional education in medicine is one of the most hopeful signs for the progress of that science, and fortunate are the schools now in a position to demand a complete collegiate course as a preliminary to the professional education. Let us hope that the example of the medical schools will soon be followed by other professional departments and that for all professional studies, including under that title investigation, a broad foundation may be demanded as a prerequisite. Thus will the dignity and usefulness of the professional schools be increased and thus will the university fulfil its trust by giving to the service of the state sons strong to withstand the wayward blasts of popular superstitions, keen to search out and expose their fallacies, and strenuous in laying secure foundations for advancement in literature, science and the arts and in fostering their development and application.

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THE CHEMIST AND THE COMMUNITY[1]

On April 18 of this year there occurred at San Francisco a vast catastrophe as the result of which more than 1,000 people are said to have lost their lives while 250,000 were rendered homeless in the midst of a conflagration involving an area of six square miles and a property loss of at least $300,000,000. On April 19 there was run over and killed in the streets of Paris a simple, unassuming, absent-minded man. The Boston *Herald* in an editorial comment upon the two events said that it might well be questioned whether of the two the accident in Paris did not in its broad relation to the welfare of mankind constitute the greater calamity. This was an amazing thing to say of the death of any man, even of one so preeminent in attainment as Professor Curie. Let us consider why it was said and upon what basis it may, if at all, be justified. It was said in tacit recognition of the fact that the quality of intellectual leadership is one of the rarest and most precious possessions of our race and that the world can better afford to lose a city or a province than one of its great investigators, philosophers or teachers. One pregnant thought, one flash of insight from a master mind, has often done more for the advancement of mankind than all the toil which built the pyramids. The result of the researches of Professor and Madam Curie has been as you all well know to change our whole conception of the material universe and to bring within the reach of our perception stupendous natural forces the existence of which had not even been suspected. The effect has been even more far-reaching for with the farther vision has come new views of what life is and of our relations to this greater universe, such views for instance as those put forth by Sir Oliver Lodge in his recent 'Life and Matter.'

In the accounts of the war between Japan and Russia frequent reference was made to the parties of chemists who far ahead of the main army were testing water supplies and posting notices which warned the oncoming troops where danger from polluted water must be avoided. It seems to me that this little vanguard well typifies what the chemist should stand for and where he should be found in his relations to the community. He is or should be essentially a pioneer rushing forward and serving the community in the best sense in serving science.

It has doubtless occurred to some of you that chemists as a professional class do not have that direct and strong hold on the regard of the community which has been established and is well maintained by physicians, lawyers and ministers. The reasons for this are not far to seek. The work of the chemist deals with things and in carrying on this work he is rarely or never brought into such direct and vital personal relations with individual members of the community as the family doctor who presides at birth, the lawyer who conducts affairs, or the minister to whom one turns in times of stress and trouble. Moreover,

[1] Read at the general meeting of the American Chemical Society, on December 27, 1906, at Columbia University.

every one knows by personal contact and experience something of the field and manner of work of the members of these professions, whereas comparatively few in the community at large have any definite or adequate notion of the scope and methods and possibilities for usefulness of the science of chemistry. This is even true of an amazing number of our manufacturers and this ignorance constitutes a very serious menace to the continuance of our prosperity. To-day as never before knowledge is power and science is only knowledge at its best. Our industrial achievements, impressive though they are, cannot be properly measured without some standard of comparison, and such a standard we have in Germany. The question for our manufacturers to answer is not what have they done, but what would Germany have done with our vast resources at her command. There is no escaping from the answer that by that measure we have failed and are repeating failure. Doctor Pritchett in a recent article has said "Perhaps at our present stage of development in such matters no other preliminary work needs more to be done than some work of popular education relative to what research is," that research which a famous German chemist quoted in the same paper declares to be 'the greatest financial asset of the fatherland.' The present and pressing duty of the chemist in his relation to the community is therefore to do his utmost in self-respecting ways to develop in the community an intelligent appreciation of his proper place within it, an understanding of the nature of his science and its potentialities for helpfulness. We shall perhaps arrive at this understanding most directly by considering for a moment something of what the chemist has already done for the community.

Chemistry enters so intimately even though unobtrusively into every phase of modern life and thought that it is perhaps impossible to present in any adequate degree the real dependence of the community upon the work of chemists past and present. Industrial revolutions are seldom chronicled and more rarely celebrated, though their influence upon the welfare of mankind may be as profound as that of other revolutions the records of which are traced in blood. It can no longer be said as was said to the father of chemistry as he passed out to execution, 'the republic has no need of chemists.' If we were to take away what chemists have contributed the whole structure of modern society would break down at once. Every commercial transaction in the civilized world is based upon the chemist's certificate as to the fineness of the gold which forms our ultimate measure of values. Faith may remove mountains but modern society relies on dynamite. Without explosives our great engineering works must cease and the Panama canal no less than modern warfare becomes impossible.

Prices rise and fall with the variations in the gold supply as the barometer responds to the changing pressure of the atmosphere, so that to the cyanids and chlorination processes which have so greatly increased the world's supply of gold must be ascribed a potent influence on market prices everywhere. With the development of the steel industry have come great fortunes and greater corporations bringing with them social benefits and social problems hitherto unknown. This industry rests preeminently upon the work of chemists as its greatest master has been quick to testify and is to-day at every point under the strictest chemical control. The Bessemer process alone was estimated by Abraham S. Hewitt to add directly and indirectly $2,000,000,000 yearly to the world's wealth. Of this vast sum Bessemer himself retained in all about ten million dollars, or one half of one per cent. of his contribution to the community in a single year. And this is characteristic generally of the rewards which come to chemists. They are not taken from the common fund, no man is poorer for them, their recipient has made others richer in those rare cases in which he has become rich himself.

In the last century the United States has grown from a narrow fringe of feeble states along the Atlantic coast line to an imperial domain which spans the continent, and yet for the purposes of business and administration it is a smaller and more compact community than it was a hundred years ago. One reason for this anomaly is found in the development of our great transportation systems and as to these it may be said that every signal lamp burns more brightly, every pound of freight is hauled more cheaply, and every traveler carried with greater safety because of the work of chemists and preeminently the work of Dr. Dudley and his confreres in standardizing and holding to the standard the materials entering into railroad equipment of every kind.

All the activities of the community are based in the last analysis on those which have to do with agriculture and as to those in the United States Secretary Wilson has said "Every sunset during the past five years has registered an increase of $3,400,000, in the value of the farms of this country," which farms have produced in a single year wealth aggregating six and one half billion dollars. Chemists from Liebig down have done much to contribute to these amazing totals by their analyses of soils and of plant products, the adaptation of fertilizers to soil requirements and the needs of special crops; the utilization of what were once waste products like corn oil, cotton oil, the gluten from starch factories, casein from skim milk, cream of tartar from the lees of wine and so on through an almost endless catalogue, and yet great as are the figures given as the output of American agriculture there can be no doubt that they might be doubled by the general application of the best teachings of agricultural chemistry and science. So much agriculture already owes to chemistry while for the immediate future is the promise of the commercial fixation of atmospheric nitrogen with all that that implies in increased productive power of the soil.

The relations of modern life, the interdependence of communities far distant from each other and the adjustments and readjustments which are constantly made necessary in these relations, has brought it about that chemistry has not always benefited agriculture but has on the contrary in some signal instances been disastrous to special though important agricultural interests. The synthesis of alizarine from anthracene in 1868 by Gräbe and Lieberman and their later commercial preparation of the coloring matter from anthraquinone proved for example a death blow to the cultivation of madder of which forty years ago the annual production was about 500,000 tons, substantially all of which, as the Avignon peasants sorrowfully say, is now 'made by machinery.' Similarly, Baeyer's synthesis of indigo upset the social economy of great regions in India where his name was never heard and today at least one half of the entire consumption of this dye stuff is produced in German chemical plants. The manufacture of these coloring matters is among the great triumphs of organic chemistry, but the inorganic chemist can point with equal pride to the production of ultramarine now sold for half the price of copper, whereas in the form of lapis lazuli it was in the time of Liebig a dearer thing than gold.

Chemists here and abroad have hardly

finished celebrating the fiftieth anniversary of the discovery of mauve, the first of the coal tar colors, and have been happy in the knowledge that its discoverer was still among them to receive their congratulations and rejoice with them at the splendid outgrowths of his work. In the addresses to Perkin at the time it was estimated that in the industries based on his discoveries no less than $750,000,000 is invested. As a result profound economic changes have been brought about not only in England and Germany, but in India, South America, Mexico, China and Japan. Our fastest dyes are now produced synthetically, the range of the dyer's art has been widely extended and through collateral channels new and powerful agencies for combating disease and suffering have been placed in the hands of physicians everywhere.

Few of the industries upon which the prosperity of the country and the comfort and material well-being of its inhabitants depend have not experienced within the memory of those before me changes so profound and so far-reaching in their effect as to be fairly described as revolutionary. I believe it to be within the truth to say, that in the great majority of cases these changes have been initiated or accelerated by chemists. For our present purpose and before this audience, it is unnecessary, even if it were possible, to catalogue the materials for which, at prices permitting their general use, the community is indebted to the chemist. They comprise a large proportion of the things which are regarded as among the necessities of life, without which comfortable, or even decent, living, would be impossible. With reference to productive industry generally, it may be said that in many instances the chemist is the most effective agent for standardizing materials, controlling the course of processes, and minimizing wastes.

The chemist has been similarly active in respect of matters pertaining to the public health. One has but to recall the splendid pioneer work of Drown in connection with the study of public water supplies in Massachusetts, work which is still regarded everywhere as the standard for other communities. The sanitary engineer can not work without the chemist, the physician relies upon him for the most potent means for avoiding or arresting disease, or alleviating suffering and domestic economy and science make increasing demands upon the laboratory.

In no way has the community benefited more through the 'diffusion of useful knowledge among men' and few if any agencies for the diffusion of such knowledge have worked to better purpose than the Smithsonian Institution, which stands as an enduring monument to the wisdom and public spirit of Smithson, who was a chemist.

We, who are 'heirs of all the ages,' have no more imperative duty upon us than that of transmitting to our successors the experience and wisdom which has been handed down to us, and in the execution of this duty the chemist has nobly borne his part. To Harvard the profession has given Eliot; to Stevens Institute, Morton; to the Massachusetts Institute of Technology, Crafts; to Lehigh, Drown; to Johns Hopkins, Remsen; to the University of Iowa, Schaeffer; to the Columbia School of Mines, Chandler. Through such educators as these, chemists have had a direct and lasting influence on public opinion, and the thought of the generation which is to follow us. Similarly, but more intimately, the heads of the chemical departments in our universities and technical schools come into contact each year with thousands of students who are influenced far more profoundly by the personality of their teacher than by the subject matter of his pedagogic efforts.

The chemist has another and more general claim upon the community by reason of the intellectual interest which his researches add to life. Moissan extends the range of our activities to the highest temperature of the electric furnace, and we produce within our laboratories the conditions obtaining in the sun. Dewar brings us within a few degrees of the absolute zero. Bunsen and Kirchoff teach us the composition of the stars. Avogadro and Ampère picture to us the mechanism of gases. Dalton supplies a hypothesis which for almost a century suffices to explain the constitution of matter and the course of chemical change. Curie opens out new vistas in which the old thought is seen in new relations, which give to the universe, as we have known it, entirely new aspects.

Briefly and baldly as I have set forth the claim on the community which chemists may fairly make, it is, nevertheless, a showing for which no apology is required. There are perhaps as many as ten thousand chemists in the country; the census of 1900 gives 8,847 as contrasted with 125,000 lawyers and 93,000 doctors. In the light of these figures who shall say that the chemist has not borne his part as should the happy warrior in the fight against ignorance, material obstacles and the phantasms of the mind.

So much at least the chemist has done and may be counted on to do for the community; but this by no means ends his obligation, if the profession as a class is to attain its true success which is the achievement of the best of which it is capable in its broadest relations to the community at large. Dewar has said that the 'one great object of the training of a chemist is to produce an attitude of mind,' and Principal Caird has defined the scientific habit of mind as 'the faculty of grasping the universal element in all human knowledge.' Karl Pearson puts the same thing in a slightly different way by saying "The scientific man has above all things to strive at self-elimination in his judgments, to provide an argument which is as true for each individual mind as for his own." When we add to this the absolute honesty toward himself and others and toward things as well which should characterize the chemist who has responded to his training and supplement the whole by precise and special knowledge and the ability to do the things within his sphere, we not only have all the essentials of good citizenship, but an ideal basis for leadership in the great work of coordinating and utilizing and making amenable to law the new powers and resources and discoveries with which the world is now congested. The chemist from the very nature of his work and training should be the unswerving enemy of graft in every form. He should not be content with a mere passive resistance and a merely personal honesty, but should take an active and aggressive part in the fight against corruption and frauds, whether these involve sea-water gold, salted mines, corporation mismanagement or politics. He should more frequently be found on school boards and boards of health and special commissions, and I venture even to suggest that chemical societies should far more often act as a body or through committees to expose abuses or battle for their remedy. Such conditions as prevailed for years in the water supply of Philadelphia, where they are not yet fully remedied and which still prevail in many sections of our country, the stagnation and inefficiency of our patent office, the fraud on and danger to the community involved in the methods of some makers of proprietary medicines, the petty graft which many manufacturers of honest products meet with in their sale

—such things as these are things for the profession as a whole to fight if the community is to have the benefit of its best service.

The number of chemical problems with which manufacturers, large and small, throughout the country are grappling, consciously or unconsciously, must be very great indeed, and their inability to solve them readily constitutes a heavy drag upon production. There is little doubt that a large proportion of these problems have either been solved already without their knowledge or are of such a nature that they require little more than their statement to a chemist of experience to permit of their immediate solution. They still remain problems either because the manufacturer has no proper conception of what chemistry can do for him, or because the chemist to whom they may have been submitted is ignorant of the conditions injected into the problem by the requirements of practise. Were both parties to the matter properly informed I have no doubt whatsoever that ten times the number of chemists now at work in the United States could be employed to the great benefit of our industries and the advancement of the position of our country in the world. One obvious step towards a remedy for the situation is a closer touch and cooperation between societies of chemists and associations of manufacturers. Manufacturers might well appear from time to time before the one and chemists before the other to the good of both. As James P. Munroe has said in a paper on applied science and the university, "Not, broadly speaking, what the bachelor or doctor knows, but how he knows it, and to what use he can put this knowledge measure his real education."

As a technical chemist I speak with some diffidence to those engaged in pure science, but I believe the question may be fairly put, whether both science and the community might not be benefited by some readjustment of our ideas as to what constitutes pure science and the extent to which the exponent of pure chemistry may properly allow himself to be led into industrial work. Scientific research in Germany is 'the greatest financial asset of the fatherland' because there the greatest minds in chemistry come into close touch and contact with the problems of commercial enterprise. The synthesis of indigo is no less a triumph of pure chemistry because of its industrial importance, and a synthesis of the resins in the juice of the milk weed can hardly be regarded as more commendable from any point of view than a synthesis of India rubber would be. Where, then, there is so much to do is it not possible to pick the problems with more direct reference to the immediate needs of the community? By far the larger part of our best research is carried on now in the laboratories of our great industrial plants, and if the teacher and the individual investigator are to match it they must in case of most of them have the broadening influence of personal contact with the conditions and needs of industry. Through them their influence will be transmitted to their pupils whose grasp upon the science will be thereby strengthened at the same time that their possible usefulness to the community is increased.

Under such conditions the relation in which the chemist stands to the community in respect of its affairs can not fail as time goes on to become one of increasing dignity and power for good unless chemists themselves forget that the surest path to influence and position is through altruistic service.

As we look back upon the great achievements of the past and view the monumental figures from whose trained brains and hands they came, as we study the vast accumulations of fact and the broad generalizations by which these stores of knowledge are bound together, we are apt to conclude that where so much has been wrested from the unknown there can be little left for new discoverers. The true view, of course, is that which regards our present knowledge as a sphere floating in the infinite of the unknown. As the sphere enlarges so it touches upon more points of the unknown. As our knowledge grows so also does our ignorance increase.

We have only to consider the chemical processes as carried out by plants and animals to realize how crude and clumsy our own present methods are. There is still plenty for the chemist to do and the prospect which lies before us is not only rich in promise for the material welfare of mankind, but one which in its realization must affect profoundly man's view of the universe and of his relation to it. Few of us can remember the intellectual stimulus which followed Wohler's discovery that a compound which seemed peculiarly to represent the product of vital forces could be reproduced within the laboratory, but most of us, I firmly believe, will witness the breaking down of the line which now separates living matter from dead matter. With it will come an intellectual revolution the result of which can only be to bring the whole world closer to 'the God of things as they are.'

Arthur D. Little

ANTHROPOLOGY AT THE NEW YORK MEETING

The joint meeting of Section H of the American Association for the Advancement of Science, the American Anthropological Association and the American Folk-Lore Society held at Columbia University, New York City, December 27, 1906–January 1, 1907, was notable for the number of working anthropologists present as well as for the length and excellence of the program. Coming, as it did, so soon after the International Congress of Americanists in Quebec, fear had been expressed that the New York program might be but the gleanings of a field already thoroughly harvested. That new fields were entered may be readily seen by a survey of the program, which included fifty-six numbers in addition to the addresses of the president of the Folk-Lore Society and of the retiring vice-president of Section H.

BUSINESS AND SOCIAL FUNCTIONS

The Council of the American Anthropological Association and the Sectional Committee of Section H held a joint business meeting on December 27, at which the retiring vice-president of Section H, Dr. George Grant MacCurdy, presided.

Professor William H. Holmes presented an official communication from the Anthropological Society of Cologne, Germany, inviting the American Anthropological Association and members of Section H to take part in the International Congress of Anthropology to be held at Cologne[1] in August, 1907; and recommended that the chair appoint a committee to further the interests of the Cologne Congress. On formal motion to that effect the chair appointed the following committee: W. H. Holmes (chairman), Franz Boas, Chas. Peabody, W J McGee, F. W. Putnam, A. L. Kroeber, K. von den Steinen, G. B. Gordon, G. A. Dorsey, C. V. Hartman, J. C. Merriam, G. F. Wright, J. W. Fewkes, S. Culin, David Boyle, A. Hrdlicka, F. M. Palmer, C. A. Peterson, S. Hagar and G. G. MacCurdy (*ex officio*).

The question of the advisability of changing the name of Section H, Anthropology, so as to read 'Section H, Anthropology and Psychology' came up for discussion. On motion the chair appointed a

[1] Place of meeting has recently been changed to Strasburg; the date is August 4–8.

special committee with power to act and to submit their action for the approval of the Council of the American Association for the Advancement of Science: Franz Boas (chairman), W. H. Holmes, A. L. Kroeber and J. McK. Cattell. The resolution submitted to the council by this committee is as follows:

First. The recommendation of the Committee on Policy to change the designation of Section H from 'Section of Anthropology' to 'Section of Anthropology and Psychology' is approved.

Second. The Committee recommends to the consideration of the Council and of the Committee on Policy the desirability of a better coordination of the sections and of the affiliated societies, particularly the desirability of having the president and the secretary of one of the affiliated societies act at the same time as sectional vice-president and sectional secretary. The Committee also recommends to the Council and to the Committee on Policy a consideration of the question whether, in view of the close affiliation of scientific societies, the discontinuance of sectional meetings and of the sectional organization may not be desirable.

In harmony with the foregoing resolutions, the section deviated from its custom in regard to officers and named for vice-president the president of an affiliated society, the list of sectional officers elected, subject to the approval of the General Committee of the American Association for the Advancement of Science, being:

Vice-president—Franz Boas.

Member of the Council—W J McGee.

Member of the Sectional Committee to serve five years—W. H. Holmes.

Member of the General Committee—M. H. Saville.

The officers of the American Anthropological Association are:

President—Professor Franz Boas, New York.

Secretary—Dr. George Grant MacCurdy, New Haven, Conn.

Treasurer—Mr. B. Talbot B. Hyde, New York.

Editor—Mr. F. W. Hodge, Washington.

A number of social functions were arranged by the local executive committee for the American Association for the Advancement of Science and the affiliated societies.

The president of Columbia University received in Earl Hall from nine to eleven o'clock on the evening of December 27.

A luncheon was given at the College of the City of New York, 138th Street and Amsterdam Avenue, on December 29, with addresses preceding, and an inspection of the new buildings following.

An invitation was extended by the board of trustees of the American Museum of Natural History to be present at the ceremonies attending the unveiling of the busts of ten American men of science presented to the Museum by Mr. Morris K. Jesup, which took place on the afternoon of the twenty-ninth. In the absence of Mr. J. Pierpont Morgan, Professor Henry F. Osborn presided. The presentation on behalf of Mr. Morris K. Jesup was made by Dr. Hermon C. Bumpus; and the acceptance on behalf of the trustees, by the Honorable Joseph H. Choate. Brief memorial addresses were made: Benjamin Franklin, by Dr. S. Weir Mitchell; Alexander von Humboldt, by His Excellency Baron Speck von Sternburg;[1] John James Audubon, by Dr. C. Hart Merriam; John Torrey, by Dr. Nathaniel L. Britton; Joseph Henry, by Dr. Robert S. Woodward; Louis Agassiz, by the Rev. Edward E. Hale;[2] James Dwight Dana, by President Arthur T. Hadley; Spencer Fullerton Baird, by Dr. Hugh M. Smith; Joseph Leidy, by Dr. William K. Brooks; Edward Drinker Cope, by Dr. Henry F. Osborn.

[1] Address read by Count Hatzfeldt, first secretary of the embassy.

[2] Letter read in the absence of Dr. Hale.

On the evening of the twenty-ninth there was a reception at the American Museum of Natural History by the trustees of the museum and the New York Academy of Science, with an exhibition of scientific progress by the Academy, including demonstrations and short addresses.

A dinner and smoker was given by the American Ethnological Society on Friday evening, December 28, at the Explorer's Club, 23 West 67th Street, to the American Anthropological Association. After the dinner those present were invited to the Knabe Building to inspect an archeological collection made by Professor M. H. Saville in Ecuador for Mr. George G. Heye.

ADDRESSES AND PAPERS

Dr. A. L. Kroeber's address as president of the American Folk-Lore Society was on the 'Musical Systems of the Indians of California.' Myth, ceremony and song are fused into one among the Mohave. The Indian music of California is noted for its simplicity. The elements are few and repeated endlessly; but the repetition is accompanied by slight variations that may be detected by the accustomed ear. The elements with variations were shown by means of lantern slides and the phonograph. Dr. Kroeber's studies included the Mohave Yelak, a myth told in song (25 songs), the Mohave Nyohaiva (6 songs) and the Mohave Raven (4 songs). He also gave by way of comparison a Kwakiutl song, a Yurok Deerskin Dance Song and a Yuki Creator Song.

The address of Dr. George Grant MacCurdy, retiring vice-president of Section H, was on 'Some Phases of Prehistoric Archeology.' Two phases only were discussed—the *eolithic question* and *paleolithic mural decorations*. This address has already appeared in Science.[3]

[3] January 25, 1907, pp. 125–139.

The program was arranged so as to group related subjects in a single session. One session, for example, included only papers on Folk-Lore, the president of the Folk-Lore Society presiding. At another papers of interest to students of economic and social sciences were read, the members of Section I being present and taking part. The Saturday afternoon program was devoted to the reports of standing committees and was of unusual interest to professional anthropologists.

Brief abstracts of the papers read are given in so far as material at the disposal of the secretary will permit.

Dr. C. Hart Merriam read three papers: 'Totemism in California,' 'The Yummĕ or Mourning Ceremony of the Mé-wuk' and 'Mé-wuk Myths.' That totemism exists in California seems to have escaped the notice of ethnologists. It is in reality quite general. Totems are chiefly animal. They are rarely natural objects. Among certain tribes the totem governs marriage. In the northern division of the Mé-wuk it has a marked influence over the individual. The means by which the individual is led to recognize his totem was given in detail. Dr. Merriam described under three heads the annual mourning ceremony of the Mé-wuk which he saw on October 10 and 11, 1906, illustrating by means of diagrams the round house in which the ceremony occurred. The last paper by Dr. Merriam was a description of certain myths of the Mé-wuk Indians in which the coyote, bear, deer, lizard, mouse, condor, turkey-buzzard, robin, sand-hill crane and other animals played a prominent part.

Dr. Clark Wissler presented some 'Notes on the Blackfoot Myths.'

The myths of the Blackfoot are classified under the following heads:

1. Old Man Series.
2. Culture Hero.
3. Ritualistic Origin Myths.

4. Moral and Entertainment Tales.

A comparison of the myths of these groups with the published mythologies of the Arapaho and Crow indicates a very close relation between the mythologies of the Arapaho and the Blackfoot. Of eighteen myths in the Old Man Series, eleven have direct parallels among the Arapaho and five among the Crow. Of twenty-seven moral and entertainment tales, ten have direct parallels among the Arapaho and two among the Crow. Of fourteen culture hero tales, four have direct parallels among the Arapaho. Thus, out of fifty-nine tales, twenty-four were directly parallel to Arapaho and seven to Crow tales. All the ritualistic origin myths seem to be peculiar to the Blackfoot, and may be regarded as their own contribution to their mythology.

Mr. Edward Sapir's 'Notes on the Takelma Indians of southwestern Oregon' are to be published in the *American Anthropologist;* while Mr. Frank G. Speck's 'Notes on Chickasaw Ethnology' are to appear in the *Journal of American Folk-Lore*. Mr. Speck read a second paper, entitled 'Outlines of Culture in the South-eastern States.'

In her 'Report on the Book on Maryland Folk-Lore,' Miss Anne Weston Whitney gave extracts from the material that is to form a forthcoming volume of memoirs of the American Folk-Lore Society. The compilation of the memoirs has been assigned to various members of the Baltimore Branch. Negro folk-lore predominates—witchcraft, death, hoodoo, conjuring, spells, etc., and the beliefs connected therewith, comparison being made between negro folk-lore of Maryland and that of negroes elsewhere, as Jamaica and Africa.

Mr. Stansbury Hagar's paper on 'Cherokee Star Lore' is to be printed in the *American Anthropologist*.

'Philippine Märchen' was the topic chosen by Mr. W. W. Newell, who spoke of an interesting collection of material that came to him from various sources in the Philippines. Though interesting, the derivation is largely European, especially Spanish.

'Recent Activity in Folk-Lore in Missouri' was one of the themes discussed by Professor W J McGee. He said that a branch of the American Folk-Lore Society had just been organized in Missouri, largely at the instance of Dr. A. L. Kroeber and through the energy of Professor H. M. Belden, of the University of Missouri. The members and officers are drawn from different sections of the state, especially Columbia, St. Louis and Kansas City; the headquarters will be in Columbia at the state university. It is the purpose of the organization to record existing traces of aboriginal lore in conjunction with the English, German, French and Spanish folk-lore which are interestingly combined in the remarkably composite population of the state.

In 'Notes on Puebloan House Construction,' by Mr. Frederick S. Dellenbaugh, the query was made as to how far house construction alone could be depended on in tribal or race classification. By itself the house frequently gave small indication of culture or race affiliation. The Icelander, of purely European ancestry, exhibited in his houses none of the architectural skill of his race. Conditions were against it. A turf or peat house was the easiest thing for him to build. The Iroquois made a flimsy bark house, yet ranked high in culture. Conditions favored bark construction. In the southwest conditions forced other, more permanent forms, from all peoples. There gypsiferous clays and stone slabs were at hand everywhere; bark was scarce. Different people, therefore, may build in the same way, while similar people may build in different ways. Without other evidence, house construction is an uncertain guide. Sites, too, were chosen for physiographic reasons and site can not be used as a gauge for race or tribe. Because houses and villages were built in cliffs, we can not deduce a race of cliff-dwellers, any more than we can deduce a particular race of forest-dwellers because we find houses in the woods. House construction and house site in themselves indicate no racial differences, or even cultural differences. An otherwise advanced tribe is sometimes prevented from constructing permanent houses by superstition, as the Navajos, who would not live in a house where a death has occurred.

The Colorado River seems to be a line of demarkation between villages of the terraced many-roomed village and the one-story few-roomed type. Here is perhaps a suggestion that the Apache and Ute entered the country from the north, driving the sedentary groups before them. The canyons of the Colorado then were utilized by the latter to hold the roving tribes at bay. Indications of fortifications are found at all fords and passes.

Puebloan houses seem sometimes to have been built to imitate the site, as in the case of the village of Wolpi, where the breaks and angles of the cliffs on which it stands are reproduced in the walls till at a little distance it is difficult to separate the natural from the artificial.

Puebloan construction was mainly of two materials: stone and clay. The stone was (1) *slabs,* (2) *blocks.* These were laid generally with clay mortar, but sometimes there was no mortar, and the stones were put together so neatly as to look like a fine mosaic. Where mortar was used the wall was frequently plastered outside with clay and sometimes whitewashed.

The clay construction was of, at least, five kinds: (1) *Adobe bricks,* either round balls or the ordinary block form so well known. Clay mortar was used. (2) *Cajon,* a form of ramming wet clay into frames. (3) *Single wattle,* plastered on one or on both sides. (4) *Double wattle* with wet clay rammed between. (5) *Jacal,* a wall of upright stakes or rods, plastered with clay on one or both sides. This last construction was also in use east of the Mississippi. In some early Puebloan construction the jacal was used for upper stories, while the lower were of adobe bricks or of stone.

Physiography controls house construction more than does race or culture. In addition there are the factors of daily habit and superstition. The Lapps, after centuries of close contact with a highly developed people, still dress in their primitive way and live in lodges covered with earth.

In 'The Archeology of Manabi, Ecuador' and 'Notes on the Andean Cultures' Professor Marshall H. Saville gave an interesting account of a successful expedition to those regions. He obtained an unrivaled collection of so-called stone seats from the environs of Monte Cristo in the coast region of Manabi. The entire absence of stone implements except hammer-stones was noted. Objects of copper are also rare. There are very few ruins in Ecuador, this being especially true of Manabi. In the interior or Andean region only two ruins are known. The present language here is Quichua, but Inca influence is very slight on the archeology of the district. As one goes north the Inca influence becomes less and less apparent. Most of the antiquities found in the Andean district came from near Rio Bamba. Many fine examples of pottery decorated by the so-called lost color process that characterizes a certain group of Chiriquian pottery as described by Holmes were obtained at Rio

Bamba. This ware is also found in northern Ecuador and southern Columbia. The valuable collections made by Professor Saville belong to Mr. George G. Heye, of New York, who bore the expenses of the expedition. The report on Manabi will be published privately very soon.

In 'Notes on the Occurrence of the Mineral Utahlite as a Prehistoric Gem,' Professor Henry Montgomery described the mineral as a hydrous phosphate of aluminum somewhat similar to turquoise and capable of being highly polished. Although rare, its occurrence has been noted in certain prehistoric ruins.

Mr. Edgar L. Hewett's two papers were on 'The Art of Glazing among the Ancient Pueblos' and 'The Relation of Pueblo Indians of the Rio Grande Valley to the Ancient Cliff Dwellers of the Adjacent Plateaus.' As regards the art of glazing in pre-Columbian times, so many specimens have been found that they can not be considered as intrusive. The ruins in question are certainly pre-Spanish. The glaze has been examined by Washington chemists and found to be saline. It may have originated accidentally about salt works. Immediate firing after applying a saturated solution would produce the glaze, which seems to have been used for decorative purposes solely. The Jemez Plateau is the chief center for glazed ware. Mr. Hewett's second paper is printed in *The American Anthropologist*.

'Recent Archeologic Work in Missouri' was the title of Dr. W J McGee's second paper. During 1905 Mr. D. I. Bushnell, of St. Louis, with two or three associates explored certain mounds on the Illinois side of the Mississippi which yielded abundant relics described in a special publication; later in the season the same gentleman had a number of additional mounds, also in Illinois, excavated by Mr. Gerard Fowke, who found moderately abundant relics not yet fully described. During the summer of 1906 Dr. C. A. Peterson, president of the Missouri Historical Society, with several members of the association (including the writer) made a number of archeologic reconnaissances in both Missouri and Illinois, in the course of which certain caves and mounds were examined—one of the trips being to an alleged aboriginal mound larger than Cahokia or Etowah, near Mascoutah, Illinois, which was found to be a paha with a few small earthworks on its summit. The most noteworthy event of the year was the creation of the St. Louis Society of the Archeological Institute of America with W. K. Bixby as president and Professor F. W. Shipley as secretary, which resulted in the commencement of a systematic survey of the antiquities of the state. Under the auspices of this society (including a subsidy from the institute and a special contribution by President Bixby), Mr. Gerard Fowke reconnoitered the lower valleys of the Gasconade and Osage with a portion of the valley of the Missouri in the central part of the state—the territory comprising what may be known as the Osage district; subsequently detailed surveys were made and over sixty mounds were excavated. In general the mounds are poor in artifacts though rich in much-decomposed osseous remains; the most notable type of artifact is represented by vaults or chambers of well-laid stone, found in a number of mounds.

Professor George H. Perkins showed a number of specimens to illustrate his paper on 'Pottery and Bone Objects found in Vermont.' Entire jars have very rarely been found in New England, and of the half dozen or so which are now in existence the three largest and finest were found in Vermont and are in the museum of the university at Burlington. Photographs of the most recently found specimen were shown. This is ten inches high, hexagonal at rim, globular below, decorated in the usual manner of Vermont pottery by indented figures and lines over the entire upper portion. It holds twelve quarts. Numerous fragments of highly decorated rims have also been found recently at what appears to have been a camp site, on Mallett's Bay, the largest of the numerous bays of Lake Champlain. At this same locality, in a stiff clay which underlies the loose surface soil many bone awls, scrapers, etc., have been found within the last two months. These objects are interesting in themselves, but they are especially so, as they are the first bone objects found in Vermont, with the exception of one or two which were obtained some years ago at another locality. Marine shells and bits of coral have also been found with these bone objects.

In 'Recent Geological Changes as Affecting Theories of Man's Development,' Professor G. Frederick Wright characterized the Tertiary as a period of stability and the Quaternary as one of great and rapid changes.

'Harness Mound Explorations' was the subject discussed by Mr. William C. Mills. The Harness Mound was opened in 1846 by Squier and Davis and again in 1885 by Professor F. W. Putnam. In these earlier explorations fifty burials were uncovered. Mills has recently found 133 additional burials. Cremation was quite generally practised. In cases where cremation took place at the grave no artifacts were found with the remains, but where cremation had taken place prior to the deposition of the remains, artifacts accompanied the latter. Mr. Mills discovered a series of post-holes surrounding the burials. Long awls made of the leg bone of the deer were described and differences noted between the bone implements from the Harness Mound and those found at Fort Ancient.

Mr. Alanson Skinner gave the results of his 'Recent Discoveries at a Prehistoric Indian Village Site at Mariner's Harbor, Staten Island.' In the spring of 1903 recent railroad excavations at Mariner's Harbor, Staten Island, N. Y., exposed a prehistoric site of the Hackensack Indians, a local branch of the Leni Lenape. Shell pits and burials were found, and up to the spring of 1906 these were opened whenever exposed and many skeletons were found. Pottery was abundant, and this, usually in Algonkin style, often showed Iroquoian influence. Grooved axes occurred, but no celts, and no implements were found with burials.

Saturday morning's program being of interest to students of social and economic science, members of Section I accepted an invitation to be present and take part. Professor Franz Boas opened the session with a paper on 'Heredity in Head Form.' Dr. Robert Bennett Bean followed with 'Some Racial Peculiarities of the Negro Brain,' it being a résumé of his studies recently published in the *American Journal of Anatomy*.[4] In 'Brain and Education,' Dr. Thomas M. Balliet traced the development of the sensory, motor and association centers.

'Selection and Elimination by Immigration' was discussed by Dr. Maurice Fishberg. From available data, collected during the enrollment of soldiers for the civil war, it appears that immigrants to the United States are, on the average, taller than the people in the countries from which they come. It was found that natives of England, Scotland, Ireland, Germany, France, etc., were, on the average, about one inch taller than the soldiers in armies of the countries of their birth. Not only were the immigrants from foreign coun-

[4] September 1, 1906, pp. 353–432.

tries superior to their compatriots at home, but native Americans who enrolled in other than their native states, were on the average taller than those who enrolled in their native states. Measurement taken by the author showed that the Jewish immigrants to the United States are also taller than their co-religionists in eastern Europe. While there are no definite measurements, still it appears superficially that the Italian and Slavonian immigrants are also a selected class physically. This phenomenon is deserving of careful study by anthropologists. It has been attributed to 'social selection' or selection by immigration, and is said to be due to the fact that it is generally the stronger, the more energetic and adventurous who venture to leave the country of their birth, their friends and relatives and travel thousands of miles in search of a possible improvement of their condition. The diseased, the weakly and the defective lack the amount of courage and perseverance necessary to undertake a long journey with small funds.

Not all those who come to the United States remain here. Over twenty per cent. of all the immigrants return sooner or later to their native countries. The author observed that most of those who return to their homes are individuals who, by reason of some physical or mental peculiarity, could not adapt themselves to the conditions in the United States. On the whole, there appears to be going on a process of elimination of many of those immigrants, who for various reasons, are unable to gain a foothold in their new homes. Among those who are compelled to return to Europe—and there are said to be about 300,000 returning annually in the steerage—there are many who would be classed as undesirable immigrants by the immigration authorities, but who in some manner passed through the inspection at Ellis Island. Our social, political and industrial conditions eliminated all these sooner or later.

'Certain Aspects of Human Heredity,' the third paper to be presented by Dr. W J McGee, closed the morning's program. Among the Ainu of Japan (of whom a group participated in the Universal Exposition of 1904) two fairly distinct ethnic types prevail, dividing—so far as known—on lines of sex; the males being of Caucasian aspect in color, pelage, features, stature, etc., while the females approach the Malayan type. Among the Cocopa Indians of the Lower Colorado there is a notable variability in stature, ordinarily divided on sex lines, the males ranking among the tallest and the females among the shortest of the North American tribes; in this respect contrasting strongly, *e. g.*, with the Pueblo peoples, among whom both sexes are below, and the Seri Indians, among whom both sexes are above the medium stature. These and other phenomena lead to a consideration of hereditary tendencies of which some incline either to 'regression toward mediocrity' as shown by Galton or 'reversion to type' as shown by Mendel, while others appear to incline toward increasing and even cumulative variability in special characteristics.

At the afternoon session of Saturday, reports of certain standing committees were read. The report of Professor Franz Boas for the committee on the concordance of American mythologies was adopted, with the recommendation that the committee be continued with power to publish.

Dr. Charles Peabody reported for the Committee on American Archeological Nomenclature. The committee was empowered to print Dr. Peabody's report in full and distribute copies to members of the association in order to form a basis for discussion and final action.

A similar disposition was made of Mr.

F. W. Hodge's report for the Committee on Nomenclature of Indian Linguistic Families North of Mexico. Mr. Hodge also reported for the Committee on Book Reviews. The conditions in regard to book reviews are improving. The present policy is to ask the reviewer in advance of sending the book; but reviews are not always furnished promptly. It was suggested by Professor Boas that a book be published by title immediately giving the scope of the work, a more extended review to follow later if desirable. The report was adopted and the committee continued.

Mr. Edgar L. Hewett spoke for the Committee on the Preservation of American Antiquities. He reviewed the new law, which seems to have been not only highly satisfactory but also administered to the letter. No permits under the law have been granted pending the adoption of uniform regulations, the making of which are entrusted to a committee. The announcement of regulations is expected soon. The president has already created the Petrified Forest National Park and also certain national monuments, such as Devil's Rock, El Morro and Casa Montezuma. Mr. Hewett reviewed the bill creating the Mesa Verde National Park. The report was adopted and the committee continued with power to observe the operations of the law; to represent archeologists in the interpretation of the law; to place before the proper authorities information as to desirable sites to be preserved; to facilitate applications for permits to excavate, etc., and to act as a joint committee with the committee from the Archeological Institute of America.

A resolution was passed to the effect that no distinction should be made between foreign and domestic institutions relative to permits for excavations.

Monday's program opened with an account by Dr. A. L. Kroeber of 'Recent Results of Anthropologic Investigations by the University of California.' The department of anthropology at the University of California is only six years old and owes much to the generosity of Mrs. Phebe Hearst. Its object is threefold: (1) the formation of collections, (2) publication and (3) instruction. The department has undertaken two surveys of California, one being anthropological and ethnological and the other archeological. In discussing the latter reference was made to two papers recently published by Professors F. W. Putnam and J. C. Merriam in the *American Anthropologist.*[5] Dr. Kroeber also referred to the discovery of a Quaternary cave in a new region and to the numerous shell mounds on the Bay of San Francisco, probably one hundred in all. Only a few of these have been explored. In some instances the lowest shell deposits are below the level of the sea.

The ethnological survey is to cover the whole state. Among the special researches may be mentioned Dr. Dixon's work on a linguistic stock that is fast disappearing. In studying the three distinct culture regions special attention is given to environmental differences.

Additional evidence of anthropological activity in California came in the form of a paper by Miss Constance Goddard DuBois on 'The Sandpainting among the Luiseños and Diegueños Mission Indians of Southern California,' which is to be published in bulletin form by the University of California. The sandpainting forms an integral and important part of some of the chief ceremonials of the religion of Chung-itch-nish, which religion was first described by Boscana in 1825, and has remained almost unknown since his day. It came to the mountain Indians of San Diego County from the coast Indians, and

[5] April–June, 1906, pp. 221–235.

to them from the islands of the ocean. Since it was given later by the Luiseños to their neighbors the Diegueños, the religious ritual in both tribes is the same. The sandpainting is therefore found in both; but has been most fully described among the Luiseños.

It was used in Mah-ne, the initiation ceremony for boys when the Datura juice mixed with water was drunk from the sacred stone bowl; in Wu-kún-ish, the girls' *fiesta;* in Ah-nut, the ant-ordeal; and in Ú-nish Ma-tá-kish, the ceremony for burying the feather head-dress, etc., belonging to a toloache initiate after his death. A central hole was dug, and the sand removed from it was used to make a heaped-up circle of a size varying in the different ceremonies. This was painted by sprinkling with powdered paints, the outer edge white; the middle, red; the inner edge black; which circles signified the Milky Way, the Sky and the Spirit of man, the Indian words all meaning spirit; the Milky Way being the Spirit to which the spirits of men go at death.

Three included rows of nine points each in succession make a geometrical figure colored in the same order, white, red and black; and the circle about the hole is similarly painted.

Small heaps of sand in several divisions have each a special significance. The whole of the sandpainting represents the earth. The sky arching above it is supposed to rest upon the circle of the Milky Way. There is a door to the north to allow of escape of the spirit after death.

The candidate in all the ceremonies mentioned except the last, knelt before the sandpainting facing the north with arms extended and a hand placed on the ground on either side of the painting, and spit into the central hole a lump of sage seed mixed with salt which signified the conclusion of a period of fasting. The hole was then filled by carefully sweeping the sand from the circumference towards it, thus obliterating the painting and ending the ceremony.

Mr. Charles H. Hawes, as guest of the American Anthropological Association, presented some very interesting 'Notes on Cretan Anthropology.' In 1903 Dr. Duckworth, of Cambridge University (Eng.), measured 85 Cretan crania belonging to the Bronze Age and 200 living subjects. In 1905 Mr. Hawes added records of 11 ancient skulls and 1,440 living Cretans, making on the latter about 29,000 measurements and observations.

The data for prehistoric times gives an average *cranial* index (for 62 ♂) of 73.4 and an estimated stature of 1,625 mm., with a dolichocephalic percentage of 65.3 and a brachycephalic of only 8.5. From these and the archeological evidence of a non-Aryan culture, we conclude that prehistoric Crete, like neighboring lands, was peopled by a branch of the 'Mediterranean race.'

But a brachycephalic minority existed even in the earliest period of the Bronze age, and the writer inclined to attribute this to an infiltration from the Anatolian highlands, of a people in the Neolithic stage, whether the so-called 'Hittites' or stragglers of the 'Alpine race.'

The records on living Cretans yield an average *cephalic* index (for 1,605 ♂) of 79.2 and stature of 1,686 mm., with a dolichocephalic percentage of only 12 and a brachycephalic of 36.9.

This broadening of the head and increase in stature is attributed to immigration. A marked increase of brachycephalism is noticeable towards the end of the bronze age and this tallies with the tradition of an invasion from the north of the Achæans and Dorians.

Both tendencies owed something to the Venetian occupation, but more to the

Turkish of the last 250 years. Although the Cretan Mussulmans are mainly of native extraction and include only a small minority of Turkish half-breeds, yet their cephalic index is (79.9) a unit higher than that of Christians (78.9) in the same provinces.

The tendency from Neolithic times, to increasing brachycephalism in Crete has a parallel in Italy and Greece, where the greater immigrations of northern peoples have produced the same phenomenon in a more marked degree.

Dr. Berthold Laufer made 'A Plea for the Study of the History of Medicine and the Natural Sciences.' A museum of the history of medicine from prehistoric times to the present would be of special importance. Such a museum should include the medical lore of the Indians. Reference was made to the two professorships of the history of medicine in the University of Berlin.

The paper by Dr. K. S. Kennard on 'Ellis Island as a Field for Anthropological Study,' dealt with the large quantity and variety of material presented at this station. The ease and rapidity with which it could be examined at this point would save delay and expense in accumulating data. Over four million aliens in the last six years had entered this port—comprising those nations which had been but scantily examined anthropologically—namely, the Magyars, people of the Balkan states and Hebrews.

Anomalies of head forms were witnessed among the southern Italians, who are generally believed to be a long-headed people. These unusual head forms resembled that of the Armenians. This was believed to be a racial trait—not an artificial product. The stature of Neapolitan women being greater than that of the men was noted, but could not be explained, also the lighter pigmentation of their eyes. Opportunities for study in folk-lore, linguistics and elementary music of these people were here offered. Advantage should be taken to make use of all this material, for nowhere else in the world could it be effected with so little expense and such complete results.

Dr. K. D. Jessen discussed 'Geometrical Design in Primitive Decoration.' Although Ernest Grosse, in his discussion of the so-called geometrical decorative design found among primitive races, argues convincingly for the original imitative character of it, this view is not at all, it seems, universally accepted. The paper tries to show that the geometric design is, by origin, of an imitative character, naturalistic, not imaginative, esthetically speaking, representing objects or phenomena found in nature or made by culture. It is conventionalized just as the later botanical design becomes conventional, the imitative origin of which no one can deny. The facts, as represented by ethnological observation corroborated by the facts concerning the beginnings of art in the child, are best explained by Grosse's theory. In fact any other theory would involve a most extraordinary break in the evolution of the human mind and would have to be excluded, perhaps, logically, under the law of contradiction..

Miss H. Newell Wardle's communication was on a kindred topic—'Studies in the Life History of Primitive Art.' The art of primitive man was, at its inception, bound by no laws, governed by naught save size and contour of the object whereon he wrought. It was realistic. With the invention of basketry, geometric figures were introduced. The discovery of pottery furnished a new field for the growth of the esthetic sense. The clay vessel inherited the geometric decoration from its predecessor, the basket, but ornamentation of pottery was by means of incising and painting, and these, more ancient than the textile arts, came unbiased to the clay of the new field. Realistic and geometric decoration upon pottery of necessity reacted upon each other, tending to produce angularities in the former, and scrolls in the latter. The predominance of either form in the art of a people depends not so much upon culture level as upon the peculiar genius of that people. Geometric designs degenerate in two ways; by complication—the reduplication of parts and addition of apparently meaningless flourishes; and by simplification to some striking characteristic—the law of essentials in primitive art.

For primitive man, the world around was filled with sentient beings. Of these he made his gods. Their symbols were, of necessity, life-forms. The life-form passes into the geometrical, and this, with the growth of philosophic and religious thought, is reinterpreted or degenerates into meaningless ornament. A good example is the swastika. The origin, meaning and decay of the symbol were fully discussed.

Professor William P. Blake described an 'Aboriginal Race Course.' In the southern portion of Yavapai County, Arizona, at Peeples Valley, not far from the rancho of Coles Bushford there is a remarkable paved way, race course or stadium of unknown but undoubtedly aboriginal origin. It is in the form of an ellipse some hundreds of feet in major length, and is paved with rough blocks of granite of irregular form for the full breadth of the roadway, about a rod, as nearly as I can now remember. This way is bordered on each side by large outlying boulders of gray granite now partially overgrown by live-oak trees. The largest of these boulders would appear to have been convenient for spectators, but were probably placed by nature along the borders of the two adjoining and nearly parallel water courses, now dry.

It may be supposed that this paved way was designed and used for foot-races. It appears to be worthy of measurements and a map.

The closing number on the program was a communication from Professor E. H. Barbour on 'The Nebraska Loess Man,' presented by Professor Henry B. Ward. The discovery in question was made by Mr. Robert F. Gilder in October, 1906, on Long's hill facing the Missouri River, ten miles north of Omaha. Long's hill stands 200 feet above the river. It is a hill of erosion and no discoverable land slip has complicated its simple geology. On its summit is Gilder's mound, in the superficial layer of which were found moundbuilder remains, and in the deep layer eight skulls and many bones of a still more primitive type. According to Professor Barbour, there is evidence of burial in case of the upper bone layer, but none in case of the lower. The bones found in the undisturbed loess doubtless antedate the hill itself. The loess in question rests on Kansan drift, and though as young as the later Wisconsin sheet or younger, it is nevertheless old. A more extended account may be found in SCIENCE for January 18, 1907; and in the Nebraska Geological Survey, volume II., Part 5.

Papers were read by title as follows:

DR. NICOLAS LEÓN: 'Foc-Lor Mexicano.'

MRS. R. F. HERRICK: (*a*) 'The Volcano of Bell Springs'; (*b*) 'On the Preparation of Bone for Certain Implements.'

MR. WILLIAM NELSON: (*a*) 'Witchcraft in Northern New Jersey in the Nineteenth Century'; (*b*) 'The Use of Water Witches in Railroad Building.'

DR. A. M. TOZZER: 'Maya Religion.'

DR. GEO. F. KUNZ: 'On the Aboriginal Use of Turquoise on the American Continent.'

DR. ALES HRDLICKA: 'Racial Characteristics of the Humerus.'

MAJOR C. E. WOODRUFF: 'The Disappearance of Blond Types from the American Population.'

MR. JAMES MOONEY: 'The Decrease of the Indian Population.'

MR. S. P. VERNER: (*a*) 'Iron and Copper Metallurgy in the Kasai'; (b) 'The Pygmies and the Anthropoid Apes'; (c) 'Phallic Influence in Bantu Art and Mythology.'

Col. Paul Beckwith: 'The French-Egyptian Medal in Commemoration of the Savants who accompanied General Bonaparte into Egypt.'

Dr. Alton H. Thompson: 'The Ethnology of the Teeth.'

Dr. Cyrus Thomas: 'Some Suggestions in regard to Primary Indian Migrations in North America.'

Dr. Samuel S. Laws: (a) 'The Physiology of Second Sight'; (b) 'A Main Factor in remedying Deafness'; (c) 'The True Object of Vision.'

George Grant MacCurdy,
Secretary

Yale University

SCIENTIFIC BOOKS

The Evolution of Culture and Other Essays. By the late Lt.-Gen. A. Lane Fox Pitt-Rivers, D.C.L., F.R.S., F.S.A. Edited by J. L. Myres, M.A., Student in Christ Church, Oxford; with an introduction by Henry Balfour, M.A., Fellow of Exeter College, Oxford, Curator of the Pitt-Rivers Museum. Oxford, Clarendon Press. 1906. Pp. 232; 21 pls. 8vo. 7s 6d net.

Here you have together, in attractive form, the principal writings of one of the pioneers in culture-history, or the story of mankind recorded in the works of their hands. The volume includes: Principles of Classification (1874), On the Evolution of Culture (1875), Primitive Warfare (1867, 1868, 1869), three chapters, Early Modes of Navigation.

Two loving disciples have prepared the volume and written the introduction. Precise references have been identified and given in full, and obvious errors in the text have been either amended or corrected in a foot-note. The volume was prepared to supply the needs of candidates for the Oxford diploma in anthropology and of the numerous visitors to the Pitt-Rivers Museum, in Oxford; but every student of culture will feel happier with a copy at hand.

Colonel Fox's text was that in the arts and customs of the still living savage and barbaric peoples there are reflected to a considerable extent the various strata of human culture in the past, and that it is possible to reconstruct in some degree the life and industries of man in prehistoric times by a study of existing races in corresponding stages of civilization. Professor Balfour wisely says: "The fact of our not agreeing with all his details in no way invalidates the general principles which he urged." In all our best museums the exhibits that attract the most people and interest those in every walk of life are the synoptic series, easily leading the mind from a shadow in the snow to the chronometer; from a bow and arrow to the latest carbine; from Triton's horn to the cornet; from a woman's back to the express train; from a raft to the gorgeous ocean steamer.

O. T. M.

March 30, 1907

Organische Zweckmässigkeit, Entwicklung und Vererbung vom Standpunkt der Physiologie. Von Dr. Paul Jensen, Professor an der Universität Breslau. Pp. 251. Jena, G. Fischer.

Dr. Jensen has attempted to state some of the general and fundamental problems of biology — adaptiveness, heredity, evolution, variation, selection, and the like—from a purely physiological standpoint, and to indicate the lines along which physiology would lead us to look for a solution. The result will be found most interesting and suggestive to those working along these lines. The processes taking place in development, individual as well as racial, are occurring in the same complex of material as are the processes of (for example) metabolism. They are as much a part of a proper science of physiology as are the latter. Further, there seems to be no reason why physiology should proceed on essentially different principles in different cases in the investigation of the various processes with which it deals. This consideration leads the author to a criticism of certain theories which do appear to be based on principles fundamentally different from those which have been found valuable in unravelling the processes commonly assigned to physiology. On the one hand all doctrines which attribute the characteristics of organisms, hereditary and otherwise, to certain units, as *ids, biophors, micellæ,* and the like, are arraigned as not in accordance with the tendency of modern physical chemistry, which physiology has found so illuminating in its application to the organic processes. Many of the modern ideas of chromosome significance are included in this criticism, which is certainly one that deserves careful consideration. On the other hand such vitalistic doctrines as that of Driesch's entelechy are set forth as equally out of the line of progress. Dr. Jensen is a man of broad reading, of judicial mind, and one that has long been known as an investigator in general physiology. To the reviewer his views seem unusually just and well balanced, so that the paper is one to be highly recommended.

In the latter parts of the work Jensen develops a general theory of development, based largely on various manifestations of the selection principle, working on the materials offered by the physico-chemical universe. In such matters tastes will of course differ; to the reviewer it appears that this, like the critical part of the work, is judicious and valuable.

The present paper is preliminary to an extensive work dealing with general physiology. If the whole is maintained at the high level shown in the preliminary part, its appearance may be looked for with great interest.

H. S. Jennings

SOCIETIES AND ACADEMIES

THE NATIONAL ACADEMY OF SCIENCES

At the meeting of the National Academy of Sciences beginning on April 16, the following papers were presented:

W. T. Swingle and Lyman J. Briggs (introduced by C. Hart Merriam): 'Utilization of Ultra-violet Rays in Microscopy,' and demonstration of the apparatus employed (with lantern illustrations).

Karl F. Kellerman (introduced by Theo. Gill): 'On the Purification of the Isthmian Potable Water Supply' (with lantern illustrations).

J. W. Gidley (introduced by C. D. Walcott): 'A New Horned Rodent from the Miocene of Kansas' (with lantern illustrations).

F. H. Knowlton (introduced by Arnold Hague): 'The Laramie Problem.'

David White (introduced by W. H. Dall): 'Permo-Carboniferous Climatic Changes in South America.'

F. W. True (introduced by W. H. Dall): 'On the Occurrence of European Genera of Fossil Cetacea in America.' (By title.)

J. M. Crafts: 'A New and More Accurate Form of Normal Barometer.'

J. M. Crafts: 'The Catalysis of Sulphonic Acids in Concentrated Solutions.'

F. H. Bigelow (introduced by Cleveland Abbe): 'A Solution of the Vortices in the Atmospheres of the Earth and the Sun' (with lantern illustrations).

L. A. Bauer (introduced by S. Newcomb): 'Results thus far obtained by the Oceanic Magnetic Survey of the Carnegie Institution of Washington, and their Bearing' (with lantern illustrations).

Richard B. Moore (introduced by Arnold Hague): 'The Relation of Radium to Hot Spring and Geyser Action' (with lantern illustrations).

Henry F. Osborn: 'Exploration in the Upper Eocene of the Fayoum Desert' (with lantern illustrations). (By title.)

Lewis Boss: 'Remarks on the Solar Motion' (with lantern illustrations).

Horace L. Wells: 'Biographical Memoir of Samuel L. Penfield.' (By title.)

A. L. Day (introduced by Geo. F. Becker): 'Some New Measurements with the Gas Thermometer.'

Simon Newcomb: 'On the Optical Principles involved in the Interpretation of the Canals of Mars.'

Simon Newcomb: 'Methods of Detecting Correlations between the Variations of Fluctuating Quantities, with an Application to the Question of the Variability of the Sun's Radiation.'

W. W. Campbell: 'The D. D. Mills Expedition to the Southern Hemisphere' (with lantern illustrations).

C. D. Perrine (introduced by W. W. Campbell): 'Results of the Intramercurial Planet Search.'

Alexander Agassiz 'The Eggs of Flying Fishes.' (By title.)

Alexander Agassiz: 'The Elevated Reefs of the Windward Islands.' (By title.)

E. W. Hilgard: 'Biographical Memoir of Joseph Le Conte.' (By title.)

Bailey Willis (introduced by Arnold Hague): 'Continental Structure of Asia.'

Wirt Tassin (introduced by W. H. Dall):

'The Occurrence of Elemental Silicon in a Meteoric Iron.'

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

A SPECIAL meeting of the society was held on March 21, 1907, at the U. S. Bureau of Standards, for the purpose of hearing a paper on 'The Determination of the Temperature of the Sun, and Recent Solar Theories,' read, by special request of the society, by Professor Dr. Otto Lummer, of the University of Breslau. An informal reception followed upon the close of Dr. Lummer's discourse.

The 632d regular meeting of the society was held on March 30, 1907, President Hayford in the chair. The first paper of the meeting was presented by Mr. Elliott Woods (read by Mr. Mark A. Woodell at the author's request) upon 'Recreations in Wireless,' the writer introducing his theme with a brief mention of his early laboratory experiments in electricity and a statement of how his attention and interest were first drawn to wireless telegraphy. Some of the characteristics of wireless telegraphy were then described. Wireless telegraphy speaks by means of vibrations impressed upon that assumed substance, the ether, in which case two capacities are electrically charged to a point where the resistance of the air breaks down and a spark passes between the terminals of the capacities when these are suitably disposed according to the potential of the E.M.F. employed. This spark is the seat of the impressing energy on the ether. The waves thus produced proceed in a circular surface formation. The next step is the receipt of these energy waves. It requires that there be some mechanical device which will absorb the incoming energy of the waves and which in its details must vibrate in unison with these incoming oscillations or waves. It was then stated that messages are received in two ways, either by automatic registration on a tape or by ear, receiving the signals by means of the so-called electrolytic receivers.

Two things appear essential for wireless communication, *i. e.*, harmony in wave vibration between source of power with which we impress the energy and the wave period of vibration of the medium upon which our power is impressed to form the waves, *i. e.*, the closed sending circuit and the aerial. These desirable conditions are found by measurements with a specially designed apparatus known as electric wave meters. A brief description was given of the different parts of a wireless equipment, and the uses of the electric wave meter in measuring the vibratory wave-lengths of the two branches of the system to accomplish harmony of relation. In describing the process of receiving the waves it was stated that as aerials vary in length in nearly every case, so their periods of vibration vary, hence 'tuning' must be resorted to if stations at any distance are to be heard. In the course of the author's remarks concerning some of his experiences in the operation of a wireless station it was stated that daylight signals as received were weaker than night signals, and he was inclined to attribute this to the fact of fewer signals being sent at night. Experience also showed that signals could be heard better on cloudy nights than on clear ones. Instances were cited which seemed to indicate that intervening land areas have an effect upon the receiving of messages. Static wave disturbances were discussed as to their general effect, as revealed by the author's experience. One of the conclusions reached by the author in studying the relations of atmospheric conditions to 'wireless conditions' was that there was no doubt in his mind as to the ability of the wireless receiver, aided by the static conditions which it registers, to show even ahead of the barometer the oncoming of a serious change in the weather, even when the sky doesn't indicate such a change. It was the writer's opinion that the greatest field of labor lies in studying the relations of atmospheric conditions to form of waves sent out under present method of wireless transmission. In the concluding remarks of the paper the commercial benefits of wireless telegraphy were briefly noted.

The last paper of the evening was presented by Mr. Asaph Hall, Jr., under title of 'Discussion of some Errors of Meridian Circles.' The introductory remarks included a description of some meridian circles. The principal idea of the newest instruments is that of reversal so as to eliminate constant errors. Diagrams were exhibited showing the method of determination and the magnitude of errors of graduation of certain English, French and American observatory circles. The mechanical process of graduating these circles was briefly described. The relative merits of the circles of different sizes used were discussed and it was stated that the tendency is towards smaller circles, one of the advantages of which is possibly that the smaller circles are easier to cast and to graduate than the large ones with which some observatories had been equipped. It was stated, as a general conclusion, that the division errors of the circles mentioned by the speaker, and shown on the diagrams exhibited, are smaller than one would expect them to be.

R. L. Faris,
Secretary

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

The sixth regular meeting of the session of 1906–'07 was held at the Chemist's Club, 108 West 55th Street, on April 5.

Dr. C. A. Doremus presented a review of the life of M. Berthelot and showed some interesting pictures and memoirs. Dr. C. S. Palmer added a few remarks regarding Berthelot's work and finally the assembly rose out of respect to his memory.

The following papers were presented:

On the Danger of Over-specialization: L. H. Baekeland.

Some New Double Phosphates of Chromium: L. J. Cohen.

1. The addition of a slight excess of diammonium phosphate to a hydrochloric acid solution of ferric chloride precipitates a double phosphate of iron of the formula $NH_4H_2PO_4 \cdot FePO_4$ which is perfectly white, soluble in mineral acids and readily hydrolyzed by water and ammonia; prolonged boiling with hot ammonia dissolves the salt with the formation of a reddish brown solution from which 95 per cent. alcohol precipitates a basic double phosphate. On ignition, the double salt decomposes, giving off ammonia and water.

2. When aluminum chloride is used instead of iron, a corresponding double phosphate forms of the formula $NH_4H_2PO_4 \cdot AlPO_4$ having the same solubilities as the iron salt, except towards alkalies; in the latter it dissolves completely, behaving like aluminum phosphate.

3. With slightly acid solutions of chromium chloride diammonium phosphate precipitates a double salt of the formula $(NH_4)_2HPO_4$-$2CrPO$, possessing a green color and retaining three molecules of water at 98° C.

4. The addition of disodium phosphate to an acetic acid solution of chromium chloride, precipitates a double phosphate of the formula $Na_2HPO_4 \cdot 2CrPO_4$, which retains five molecules of water at 98°, and which is considerably lighter in color than the corresponding ammonium salt.

Reply to Criticisms of Dry Lead Defecation in Raw Sugar Analysis: W. D. Horne.

In clarifying sugar samples for polarization it is customary to add a solution of lead subacetate to remove coloring matter. As the precipitate formed occupies some space within the 100 c.c. to which the solution must be made up before filtering and observing in the polariscope a corresponding concentration of the solution of sugar ensues, causing the polarization to be too high, in proportion to the volume of the precipitate. To obviate this error W. D. Horne proposed that the solution of sugar be made up to the full 100 c.c. before clarifying and then that a powder of anhydrous lead subacetate be carefully added. This has the effect of causing the precipitate formed to lie outside of the 100 c.c., the acetic acid radicle taking up the space within the solution formerly occupied by the organic radicles now combined with the lead. H. and L. Pellet attacked this method, claiming that 1° the lead precipitate absorbed from solution enough sugar to counterbalance the error due to the volume of the precipitate, and 2° the addition of anhydrous lead subacetate dilutes the solution enough to account for the differences of polarization by the ordinary method and by Horne's dry lead defecation.

In this paper Horne shows that the precipitate does not absorb sugar, by demonstrating that the ratio of sugar to water is a trifle lower in the unwashed lead precipitate with its adherent sugar solution than it is in the filtrate from the precipitate, while if the adsorption theory were correct a difference of ratio of sugar to water would exist in the other direction and be more than seven times greater than that actually found. The author also shows by analyses of pure sugar solutions to which known quantities of organic matter precipitable by lead were added, that the increase in polarization is strictly in accord with the volume of the precipitate, leaving no room for the claim of absorption.

In regard to the second criticism it is pointed out that analyses of the filtrates after clarification with lead subacetate showed the presence of only very small quantities of lead, which calculated to anhydrous lead subacetate and taking into account the volume which this salt occupies when dissolved, would only have been capable of influencing the results to the extent of 0.044 per cent. and 0.042 per cent. respectively, in two very low test sugars, which are quantities smaller than the allowable analytical error. High test sugars would be influenced even much less.

It is pointed out in conclusion that the critics of the dry defecation method have explained away by their gratuitous assumptions twice as much difference between the old and the new methods as ever can exist, while these later researches of Horne's all go to strengthen the claims he originally made.

C. M. Joyce,
Secretary

THE AMERICAN CHEMICAL SOCIETY. NORTHEASTERN SECTION

The seventy-fifth regular meeting of the section was held at the Trade Club, 77 Summer Street, Boston, on Friday evening, March 29, at eight o'clock, President L. A. Olney in the chair. About ninety members and guests were present.

The section was addressed by President Ira Remsen, of the Johns Hopkins University, who gave some 'Reminiscences of Liebig and Wöhler.' Having been a student under both these great leaders, and knowing one of them (Wöhler) rather intimately, President Remsen was able to throw many side-lights upon the characters of them both. While at Münich as a student in Volhard's laboratory in 1867, he attended Liebig's lectures, and the description of Liebig, his pompous personality, his irritable temper and his overbearing attitude towards his assistants and subordinates, his bent towards sensational experiments and striking situations, was most interesting and amusing. Then followed a résumé of Liebig's investigations, his great work in agricultural and physiological chemistry: the question of possible extraction of the essential food principles of meat, and the resulting production of 'Liebig's extract'; the scientific preparation of bread, and the appearance of 'Liebig's bread' in the market; finally his lively controversy with Pasteur, on the subject of fermentation, was most interestingly told. Liebig's greatest work was done in Giessen, in what Dr. Remsen considers the 'greatest school of chemistry the world has ever known.' At Munich he rested largely on his reputation, became a court favorite, and gave much less attention to chemical work.

Through the good offices of Volhard, the speaker was introduced to Wöhler, and as a result soon became one of the latter's students at Göttingen. Wöhler was most emphatically the opposite of Liebig in every way; a small-statured, quiet man, who was very methodical and painstaking and given to very minute explanations of the various phenomena observed in his experiments. He was also a most kindly man in his relations with his family and subordinates. His lectures were elementary as best suited to the needs of his audience, but his investigations were of a high order. For three semesters, the speaker was Wöhler's assistant and took part in the researches on aluminium, silicon, boron, etc., and came to know him very well. Wöhler had but little interest in the theories of Kékulé, who was then exciting a great influence in organic chemistry. Wöhler's home life was ideal and his friendship for Berzelius very strong. Liebig and Wöhler exerted a great influence upon chemistry during the second and third quarters of the last century.

Following President Remsen's address, short accounts were given of the following honorary members of the society, whose deaths have been recently announced: Mendelejeff, by Professor H. P. Talbot; Roozeboom, by Professor A. A. Noyes; Berthelot, by Professor J. F. Norris; Moissan, by President Remsen.

A vote of thanks was tendered to President Remsen and the other speakers, for the very interesting addresses and the section adjourned at 10:25 P.M. As usual, a light lunch was served immediately after adjournment.

Frank H. Thorp,
Secretary

DISCUSSION AND CORRESPONDENCE

THE MISLEADING AND THE NON-INFORMING TITLE

To the Editor of Science: There is a matter to which frequent reference has doubtless been made in print, but to which I now recall attention.

I allude to (1) 'the misleading title' and (2) 'the non-informing title.'

1. I need give no particular instance. Entomological magazines are full of 'Entomological Notes in Spain,' 'A Trip to Switzerland,' etc., referring actually to Lepidoptera only; or 'Coleoptera from Moray,' to which a list of the Hemiptera captured is added as a foot-note. These are a great nuisance to the specialist.

2. I take as an example, the *Proc. Linn. Soc.: New South Wales* (2), VI., part 3 (1892), not because the publication is alone in its misdemeanor, but because I have just been referring to it.

There are four papers in the 'list of contents' which convey no idea of even to what class they refer, unless one indeed chances to have heard of the forms previously.

(*a*) 'On the synonymy of *Helix* (*Hadra*) *gulosa* Gould.' On the second page (322), 'Conchology' and 'Mollusca' are mentioned.

(*b*) 'Observations on the Chloræmidæ,' etc. Who, but a student of the worms, knows what a chloræmid is? We find no help till the middle of the first page, when it is mentioned as a chætopod and it is quite likely that some specialists do not know what a chætopod is. It is, however, termed an annelid on the fourth page.

(*c*) 'Descriptions of two new species of *Carenum* from West Australia.' I do not think there is anything in the six pages of this paper to inform us to what class *Carenum* belongs, unless by inspection of the horismology used. I presume, from certain words employed, and from the fact that Mr. Sloane is the author, that it is a carabid beetle.

(*d*) 'Description of a new *Diplomorpha.*' 'Shell,' 'Conchology,' etc., are used, informing us that the genus is molluscan.

The above remarks are made on the supposition that the 'Proceedings' are in front of us; but what is the unlucky wight to do who only sees a list of the contents as an advertisement in some other journal?

The instances cited and the countless other similar ones are a disgrace to the authors and editors concerned. The simple method adopted by, *e. g.*, the Entomological Society of France in their *Bulletin*, is now urged. An abbreviation of the order, or orders, concerned is placed in square brackets after the title, thus 'Note sur *Corœbus fasciatus* Vill. = *bifasciatus* Ol. [Col.] et ses parasites [Hym.]'; so that we know at once that this paper deals with Coleoptera and Hymenoptera. This is sufficient for entomological publications; for those of wider scope, the addition of the class would be useful, thus '[Ins. Col.]' or '[Crust. Dec.].'

There are already troubles more than sufficient, in the path of the present-day worker who strives to keep himself informed of the literature of his chosen subject, in the shape of multifariousness of publications and of languages, false dates of publication, false pagination of separata, and so forth, without his being compelled to resort to Scudder and Waterhouse, often only to find that the generic name desired has been employed three or four times, perhaps in different phyla.

G. W. Kirkaldy

THE DISPUTED ERUPTIONS OF VESUVIUS

To the Editor of Science: It occurs to me that two important bibliographical references omitted by Dr. Eastman in his note upon the disputed eruptions of Vesuvius are L. Riccio, 'Bibliografia della eruzione vesuviana dell'anno 1631,' in *Arch. Stor. Napol.*, XIV., pp. 437 et seq., and the most exhaustive bibliography of Vesuvian eruptions that I know about, that compiled by Herr Furcheim (E. Prass, Naples, 1897). This latter is a work that took many years to complete.

As Professor B. Croce, of Naples, says, the terrible eruption of 1631 produced a great corpus of scientific and poetic literature concerning it. Perhaps the most noted of the literati who treated of it was Giambattista Basile (1575–?–1632) the celebrated author of the Pentameron entitled Lo Cunto de li Cunti, one of the chief monuments of Neapolitan dialect. One of the three sonnets which Basile composed upon the memorable occasion is a masterpiece of poetic visualization:

Con vomero di foco, alto stupore,
Mostruoso arator solca il terreno,
E il seme degl'incendii accoltoal seno
Vi sparge, e'l riga di fervente umore.
E, quindi, a fecondarlo in rapid'hore,
Di cenere ben ampio, ilrende pieno;
Onde, quanto circonda il mar Tirreno,
Messe raccoglie di profondo horrore.
Ma, se danno produce a noi mortali
Cotanto aspro Vesuvio; ond'ogni loco
Arde, nè scampo ei trova in mezzo al verno;
Pur raccoglier ne giova in tanti mali
Dal cener sparso, e dal versato foco,
Membranza de la Morte, e dell'Inferno!

In connection with this topic one ought not to omit mention of the eruption of 1794 as described by the historian and engineer General Colletta in his Storia di Napoli.

T. D. BERGEN

CAMBRIDGE, MASS.

SPECIAL ARTICLES

A PLANT-TUMOR OF BACTERIAL ORIGIN

THE number of vegetable galls known positively, *i. e.*, by exact experiment, to be due to bacteria, is not very great. The discovery of a new one of undoubted bacterial origin is, therefore, of considerable interest to plant pathologists, and may be of some interest to animal pathologists, especially to those interested in determining the origin of cancerous growths.

For two years the writers have been studying a tumor or gall which occurs naturally on the cultivated marguerite, or Paris daisy. It has been difficult to isolate the organism and to demonstrate it unmistakably in stained sections. Recently the bacteria (seen in small numbers in the unstained tissues on the start) have been plated out successfully. With subcultures from poured plate colonies, thus obtained, the galls have been reproduced abundantly and repeatedly during the last few months, the inoculations having been made by needle-pricks. From galls thus produced the organism has been reisolated in pure culture and the disease reproduced, using subcultures from some of the colonies thus obtained and puncturing with the needle as before. More than 300 galls have been produced by puncture inoculations. Under the most favorable conditions (young tissues) the swellings begin to be visible in as short a time as four or five days, and are well developed in a month, but continue to grow for several months, and become an inch or two in diameter.

In some of our experiments one hundred per cent. of the inoculations have given positive results (40 punctures out of 40 in one series; 62 punctures out of 62 in another), while the check plants have remained free from tumors, and also, in nearly every case, the check punctures on the same plant. In the two series just mentioned there were 110 check punctures on the same plants, all of which healed normally and remained free from galls. Old tissues are not very susceptible. The tumors grow rapidly only in young fleshy organs. The organism attacks both roots and shoots. It frequently induces abnormal growths on the wounded parts of young cuttings. Its power to produce hyperplasia is not confined to the marguerite. Well-developed small tumors have been produced in a few weeks on the stems of tobacco, tomato and potato plants and on the roots of sugar beets. More interesting economically is the fact that galls closely resembling the young stages of crown-gall have been produced on the roots of peach trees by needle-pricks, introducing this organism. In eighteen days these growths have reached the size of small peas, the checks remaining unaffected. It is too early, perhaps, to say positively that the cause of the wide-spread and destructive crown-gall of the peach has been determined by these inoculations, but it looks that way. Of course, the most that can be affirmed absolutely at this writing is that we have found an organism which when inoculated into the peach produces with great regularity galls which in early stages of their growth can not be distinguished from the crown-gall. The matured daisy galls also look astonishingly like the peach gall. Numerous experiments which ought to settle the matter definitely in course of the next three months are now under way. In the best series of experiments on peach roots (that inoculated from a standard nutrient agar culture five days old) 14 groups of needle-punctures (5 in each group) were made on nine trees, 13 tumors resulting. The fourteenth group was on a weak tree which did not leaf out, and might therefore be left out of the count. In that case we have 100 per cent. of infections. On the roots of nine young trees from the same lot, held as checks, 75 punctures were made, using a sterile needle, but no galls resulted. In another series of 9 peach trees inoculated at the same time as the preceding and examined on the twenty-third day, 75 per cent. of the punctures had yielded galls (9 tumors on 7 plants). These roots were inoculated by needle-pricks from a culture believed to be rather too old (glycerin agar streak 6 days), but the plants were set out again, and it is not unlikely that galls will finally develop on the roots of the other two plants. The plants, inoculated and uninoculated, were set, immediately after making the needle-punctures, in good greenhouse soil, in new ten-inch pots, and have been subject to the same conditions as to light, heat and water.

That crown-gall of the peach is due to a myxomycete the writers have never been willing to admit, because the inoculation experiments described by Professor Toumey do not clearly establish such fact. He saw often in the tissues of the galls what he interpreted to be the protoplasm of a slime mold mixed in with the protoplasm of the host plant, and he obtained sparingly what he supposed to be the fruiting bodies of this organism on the cut surface of the galls. He made, however, only two series of inoculations with the spores of his *Dendrophagus globosus*, four trees in the first case and six trees in the second, one developing the disease in the first instance and two in the second. Why did not the other seven trees contract the disease when the spores were thrust into the wounded tissue? He did not fully exclude the possibility that the three infections were due to some other cause accidentally introduced on his needle point. The *Dendrophagus* sporangia furnishing spores for the inoculations grew not on culture media but on the cut surface of a gall (an infectious substance). What if a few bacteria had been carried up from the surface of the gall, contaminating the surface or interior of the sporangia? Then the needle might occasionally have introduced two organisms into the wounds instead of one, as believed, and the unsuspected one might have been the cause of the disease. This supposition is not excluded by any of Professor Toumey's experiments.

The fact remains well established, however, by experiments of various persons: Thaxter, Halsted, Selby, Toumey, Smith, Von Schrenk and Hedgcock, etc., that when minced galls are buried in the earth near the roots of sound trees, the latter develop galls. The disease is therefore a communicable one, but the cause, in spite of much study by many persons, is still in dispute.

For the organism causing these tumors the name *Bacterium tumefaciens* is proposed with the following brief characterization: *B. tumefaciens* n. sp., a schizomycete causing rapid multiplication of the young tissues of *Chrysanthemum frutescens, Prunus persica*, etc., the result being the production of tumors or galls. The organism is motile, especially in young cultures; it is non-gas-forming and aerobic (twelve days) with all of the sugars and alcohols tried (dextrose, saccharose, lactose, maltose, mannit and glycerin). It is white on standard nutrient agar and potato and in peptonized bouillon. In tubes of bouillon it grows best at the top, producing a stringy ragged rim easily separable on shaking. It does not cloud bouillon heavily. The surface colonies on agar, 25° C., are small, round, smooth and rather dense. In agar streak cultures the organism is inclined to pile up along the track of the needle rather than to spread widely. It is inclined to be viscid on agar, after three days. It gradually blues litmus milk, throwing down the casein by means of a lab ferment, or at least not by the production of any acid, finally the litmus is reduced. It does not liquefy standard nutrient gelatin (fifteen days) and does not grow in the thermostat at blood heat (agar, bouillon). In young agar streak cultures it is a medium-sized, short rod, with rounded ends, often in pairs with a plain constriction, the elements usually being 1μ or less in diameter and two to three times as long as broad. The one to three flagella are polar. It is not yellow on any medium, or green fluorescent, nor does it brown the agar. It is rather short-lived on agar. It does not grow in Cohn's solution and does not infect olive shoots. It occurs principally at the bottom of the tumor rather than uniformly distributed in its tissues. It is best isolated from that part of the stem where the tumor joins the healthy tissues. There are slight indications of metastasis. Non-pathogenic yellow organisms are frequently obtained on plates made from older portions of the galls.

ERWIN F. SMITH,
C. O. TOWNSEND
BUREAU OF PLANT INDUSTRY,
U. S. DEPARTMENT OF AGRICULTURE,
April 4, 1907

NOTES ON ORGANIC CHEMISTRY

CATALYTIC ACTION OF ETHER AND OF TERTIARY BASES ON THE CLAISEN CONDENSATION AND ON THE FORMATION OF GRIGNARD'S REAGENT

SINCE its discovery the Claisen condensation has excited a considerable amount of interest, not only on account of the compounds which can be obtained by its means, but also because of the rather complex changes which attend its progress and which are far from being understood. In its simplest form, the reaction consists of the elimination of one molecule of alcohol from equal molecules of an ester and an aldehyde or ketone, and it proceeds under the influence of sodium or sodium ethylate. Thus, for example, acetone, CH_3-$COCH_3$, and ethyl oxalate, $C_2H_5OCOCOOC_2H_5$, under the conditions mentioned, readily form ethyl acetoneoxalate, $CH_3COCH_2COCOOC_2H_5$, and alcohol, C_2H_5OH.

In the course of some work on which we have been engaged for a number of months, we have found that the Claisen reaction is very greatly accelerated by the addition of small quantities of ether or of a tertiary base, such as pyridine or quinoline, the reacting materials being dissolved in low boiling ligroin. We have shown that this accelerating action is not due to the fact that any of the intermediate sodium compounds have a solubility in such a mixture, materially different, from their solubility in pure ligroin. In short, the ether and the bases act as typical catalytic agents. We believe that these observations put the Claisen reaction in an entirely new light.

About eighteen months ago it was found by Tschelinzeff,[1] that the formation of Grignard's reagent,

$$Mg{<}^{X}_{R},$$

(X = halogen; $R = C_2H_5$, C_6H_5, etc.) is also influenced in the same manner by the presence of ether or of a tertiary base. We have confirmed this result and have made some new observations of our own. There is thus, experimentally, a very striking parallel established between the Claisen condensation and the formation of the Grignard reagent. The object of this note is to call attention to our results, which we think have some general interest. A fuller account of the subject, together with a description of the experiments

[1] *Ber. d. Chem. Ges.*, **37**, 2081, 4534; **38**, 3664 (1905).

which we have made, will be found in a paper published quite recently.[2]

J. Bishop Tingle,
Ernest E. Gorsline
Johns Hopkins University,
Baltimore, Md.,
April, 1907

CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

VON BEZOLD, PAULSEN, RUSSELL

Death has claimed three men whose work in meteorology has made their names well known the world over: von Bezold, Paulsen and Russell.

Wilhelm von Bezold died on February 17, 1907. Born in Munich in 1837; Ph.D. of Göttingen; professor at Munich; organizer of the Bavarian Meteorological Service; professor of meteorology at Berlin; director of the Prussian Meteorological Institute at Berlin; his best-known and most important writings concerned meteorology as the physics of the atmosphere. A collection of his contributions to meteorology was published in October, 1906, by Vieweg, of Braunschweig.

Adam F. W. Paulsen (1833–1907) died at Copenhagen on January 11. Physicist; director of the Danish Meteorological Institute; active in organizing and supervising the extended meteorological work of that institute; critical student of the aurora borealis and of the meteorological conditions of Greenland; established permanent telegraphic communication between Iceland and Europe for the transmission of meteorological despatches; member of the International Meteorological Committee.

H. C. Russell died at Sydney, New South Wales. Since 1870 government astronomer and director of the Sydney Observatory; organizer of the New South Wales meteorological service; untiringly enthusiastic in increasing the number of his observers and in publishing the results of their work; fellow of the Royal Society.

SOUNDING THE AIR OVER THE OCEANS

The Prince of Monaco recently addressed the Scottish Geographical Society on 'Meteorological Researches in the High Atmosphere' (*Scot. Geogr. Mag.*, March, 1907), giving a popular account of the work which he has carried on during the past three years in exploring the free air over the oceans by means of kites, captive balloons, *ballons-sondes* and pilot balloons. On his recent expedition to Spitzbergen, the Prince reports that these pilot balloons were followed by means of special theodolites up to an altitude of 82,000 feet at the very least. At the moment one particular balloon disappeared it was at a distance of 49½ miles from the observers. The pilot balloon ascents of 1906 showed that near the 80th parallel north latitude, at a height of 13,600 meters, more or less, there are winds of 132 miles an hour, with a direction S. 68° W.

RAINFALL AND THE SALTON SEA

Professor A. J. Henry (*Monthly Weather Review*, December, 1906) discusses briefly the, at present, very pertinent question of the possible effects of the new Salton Sea on the climate of the surrounding area. The excessive rainfalls of 1905, which have been attributed to the influence of the Salton Sea, are shown to have had nothing to do with that body of water. It is undoubtedly true that the relative humidity in the immediate vicinity has been somewhat increased, but this does not mean that there must be an increase in rainfall.

CLIMATE OF VIRGINIA

In a recent paper on 'Climate and Boundaries of Virginia' (*Bull. Amer. Geogr. Soc.*, February, 1907) G. T. Surface gives a brief account of the climatic features of the state, subdividing into three provinces, tidewater; middle piedmont; and Blue Ridge, valley and Appalachia. The discussion is inadequate so far as giving any very clear picture of the actual conditions is concerned. A table of mean annual temperature, rainfall and snowfall, and length of the growing season (1900–1905) is given for 'representative' stations. We note that in the western districts "the most successful growers plant their orchards on the mountains, because the valleys are not

[2] *Amer. Chem. Jour.*, **37**: 483 (1907).

only more subject to frost, but the winter temperature is lower than for the mountains." This is a common result of inversions of temperature, generally noted in mountainous regions everywhere.

RAILROADS AND VEGETATION IN THE TROPICS

ONE of the chief difficulties in the maintenance of way along railroads in the moist tropics is the constant struggle against tropical vegetation. This is also a source of great expense. Along the Tehuantepec Railroad, as pointed out by Dr. E. O. Hovey (*Bull. Amer. Geogr. Soc.*, February, 1907), mechanical means have proved inadequate, although they are still used, and the railroad company has adopted a chemical which is applied, from a tank car, at a high temperature and under pressure, by means of a steam sprayer. This kills the roots, as well as the superficial growth.

NOTES

BULLETIN 59, New Mexico College of Agriculture, by J. D. Tinsley, contains the meteorological observations taken at the experiment station between 1892 and 1905; also the results of temperature and rainfall observations at various stations in the Mesilla Valley for most of the years 1851–1890, previously published in Gen. Greely's Report on the Climate of New Mexico some years ago.

Dr. W. N. Shaw, director of the British Meteorological Office, has been appointed reader in meteorology in the University of London.

R. DEC. WARD

EXPERIMENTS ON HUMAN NUTRITION

THE Carnegie Institution of Washington has for several years been interested in the study of problems in human nutrition, which it has aided with grants. One of its first undertakings in that line was in connection with the investigation carried on by Professor W. O. Atwater, at Middletown, Conn., in cooperation with this department, the work being continued under his successor, Dr. F. G. Benedict. This joint effort has been directed to increasing the efficiency and precision of the respiration calorimeter, previously developed with the aid of this department, and especially to providing the oxygen annex, making it a closed-circuit apparatus.

So great has been the interest of the institution in this work and its belief in the possibilities open to it, that it has decided to establish it as one of its permanent lines of research and to provide a special laboratory for it, as has already been done for a few other lines. The nutrition laboratory will probably be located in New York, in connection with one of the large hospitals, and will be devoted particularly to inquiries in relation to medicine, physiology and hygiene. The fitting up of the apparatus and laboratory will be in charge of Dr. Benedict, who will direct the subsequent investigation.

There are many problems concerned with nutrition in disease and convalescence, and with the energy output and hence the food requirements of the body under various pathological conditions, as well as many questions of ventilation and other branches of hygiene, to the study of which the respiration calorimeter is especially adapted. Such questions have a wide interest and are of far-reaching importance, and as the department's researches have developed there have been urgent requests that they be taken up. They are, however, distinctly separate from the investigations of the nutritive value of agricultural food products, to which the department's efforts have been directed, and have seemed rather to belong to some other agency than one working primarily in the interests of agriculture and looking to annual appropriations for continuation.

It is especially gratifying, therefore, to all interested in the subject of nutrition in its broadest aspects, that the Carnegie Institution should have recognized its importance and decided to provide for it as one of its special departments of research. It is thus given greater permanency and greater freedom in scope than could be the case under legislative appropriation, and the possibilities are opened for extending the investigation into theoretical lines where it is much needed.

Especially is this departure gratifying to those who have been interested in the nutrition investigations under the department, for it is a direct outgrowth of the latter's work which has led up to it and made it possible. The development of the respiration calorimeter under the department's cooperation, and the fundamental inquiries which have been conducted with it for several years past, have stimulated research in this field; and as the apparatus has remained the only one of its kind in the country it has been looked to as the means of putting to exact test the deductions and conclusions from other investigation.

The wide interest awakened in nutrition studies, and the experimental methods which have been elaborated, are substantial products of the department's work and influence in this line, and the step taken by one of the highest research institutions of the country is an indication of the way in which the department's work may prepare the way for and stimulate research in the broader fields of science.

While plans for the continuation of these studies are not fully matured, it is expected that the respiration calorimeter at Middletown will be moved to Washington and installed in the new laboratories now building on the department grounds.—*Experiment Station Record.*

COMMEMORATION OF THE TWO HUNDREDTH ANNIVERSARY OF THE BIRTH OF LINNÆUS

THE committee of the New York Academy of Sciences having in charge the commemoration of the two hundredth anniversary of the birth of the eminent Swedish naturalist Linnæus has completed a program of exercises and exhibits, the main features of which are as follows:

Observance of the anniversary, May 23 next, will begin at the American Museum of Natural History with an exhibition from 10 A.M. to 12 M. of specimens of American animals known to Linnæus, in charge of Frank M. Chapman, Professor W. M. Wheeler, William Beutenmueller and L. P. Gratacap, curators of the museum. These exhibitions are to be continued until May 30. At 10:30 A.M., Dr. Hovey, as secretary of the New York Academy of Sciences, will read letters concerning the anniversary received from other societies. Mr. Archer M. Huntington, president of the American Geographical Society, will deliver at 11 A.M. an address on 'North American Geography in the Time of Linnæus.' Dr. Joel A. Allen, curator of ornithology and mammalogy in the Museum of Natural History, who is one of the oldest members of the Linnæan Society of New York, has been invited to make an address at 11:30 on 'Linnæus and American Zoology.'

In the museum building of the New York Botanical Garden, from 2 to 3:45 P.M. there will be an exhibition of American plants known to Linnæus, in charge of Professor L. M. Underwood, Dr. John K. Small, Dr. P. A. Rydberg, Dr. M. A. Howe, Dr. C. B. Robinson and George V. Nash, all of the Garden staff, and an exhibition of the botanical writings and of portraits of Linnæus in charge of Miss Anna M. Vail, librarian of the garden, and Dr. John H. Barnhart, editor of the publications of the Torrey Botanical Club.

At 2:45 P.M. Dr. Per Axel Rydberg, curator of the Garden herbarium, will deliver an address on 'Linnæus and American Botany,' and an hour later there will be an exhibition of selected lantern slides of flowers of North America known to Linnæus, in charge of Professor H. H. Rusby, honorary curator of the Garden's economic museum and dean of the College of Pharmacy.

From 3:45 to 4:30 P.M. such visitors as desire will walk south from the museum building through the grounds of the garden, and Dr. W. A. Murrill, first assistant to the director, will point out characteristic American trees with which Linnæus was acquainted. Carriages will be at hand for persons who prefer to ride.

A bronze tablet commemorative of Linnæus, a gift to the city from the New York Academy of Sciences, will be unveiled, at 4:30 P.M. at the bridge over the Bronx River in Pelham Parkway. The position selected for the tablet, on the bridge itself, has been approved by Samuel Parsons, landscape architect of the park department, and the design is now before the Municipal Art Commission for acceptance. The site of the memorial of the great Swedish naturalist is notably appropriate, as it lies between the Botanical Garden and the Zoological Park.

At the unveiling exercises an address will be made by Dr. N. L. Britton, president of the Academy of Sciences, and a number of documents will be deposited within the tablet. A speech accepting the tablet on behalf of the city will be made by Joseph I. Berry, park commissioner of the Bronx, and a representative of the New York Historical Society will accept the key of the tablet for safe keeping until May 23, 1957. Addresses will be delivered by Dr. George F. Kunz, president of the American Scenic and Historic Preservation Society, and Emil F. Johnson, president of the United Swedish Societies of New York.

In the New York Zoological Park, from 5 to 6:30 P.M., there will be an examination of the collections, with special reference to animals known to Linnæus, in the charge of William T. Hornaday, the director, and C. William Beebe, R. L. Ditmars and Dr. W. Reid Blair, of the Park staff.

At the Museum of the Brooklyn Institute, between 7:30 and 9 P.M., there will be addresses, including one by the director, Mr. F. A. Lucas, on 'Linnæus and American Natural History.' From 8:30 till 10:30 P.M., at the New York Aquarium, in Battery Park, there will be demonstrations of features of marine life recognized by Linnæus, by Charles H. Townsend, the director; Professor C. L. Bristol, of New York University, and Professor Bashford Dean, of Columbia University. All the exercises will be open to the public.

The committee consists of Dr. N. L. Britton, Dr. H. C. Bumpus, F. A. Lucas, William T. Hornaday, Charles H. Townsend and Professor Wm. M. Wheeler.

SCIENTIFIC NOTES AND NEWS

At the meeting of the National Academy of Sciences, held in Washington last week, President Ira Remsen, of the Johns Hopkins University, was elected president to succeed Mr. Alexander Agassiz. The vacancy in the vice-presidency thus created was filled by the election of Dr. Charles D. Walcott, secretary of the Smithsonian Institution, and Mr. Arnold Hague was reelected home secretary. Members were elected as follows: Joseph P. Iddings, professor of petrology, University of Chicago; Harmon N. Morse, professor of chemistry, Johns Hopkins University; Franklin P. Mall, professor of anatomy, Johns Hopkins University, and Elihu Thomson, Thomson-Houston and General Electrical Companies. The four following foreign associates were elected: Sir James Dewar, LL.D., F.R.S., London; Professor A. R. Forsythe, Cambridge, England; Professor Dr. David Hilbert, Göttingen, Germany; Professor J. C. Kapteyn, Gröningen, Holland. The council is constituted as follows: Mr. Alexander Agassiz, Cambridge, Mass.; Professor R. H. Chittenden, New Haven, Conn.; Professor Geo. E. Hale, Pasadena, Calif.; Professor Henry F. Osborn, New York City; Dr. W. H. Welch, Johns Hopkins University; Dr. R. S. Woodward, Carnegie Institution, Washington. The autumn meeting will be held at Columbia University, New York, the sessions beginning on November 19.

Dr. Franz Boas, professor of anthropology in Columbia University, was presented on April 16 with a volume of researches by his colleagues and former students in honor of the twenty-fifth anniversary of his doctorate. The presentation was made by President Butler in the presence of the council, members of the division of philosophy, psychology and anthropology and invited guests.

Count de Montessus de Ballore, of Abbeville, France, one of the leading authorities on earthquakes, has accepted a call from the government of Chili to establish for them a seismological service of the first rank. This action on the part of the Chilian government is a direct result of the disastrous Valparaiso earthquake of last August, and sets a good example to some more advanced nations. The service in question will, at the beginning, include one station of the first rank and three of the second, to be subsequently further enlarged. The count will travel *via* New York and San Francisco, arriving in New York early in May.

Professor Emile Goldi, director of the Museum Goldi, at Belem, Para, Brazil, has resigned after thirteen years of service and twenty-five years of residence in tropical Brazil. He has been appointed honorary director of the museum and will hereafter be connected with the University of Bern, where he will work up the scientific material of the natural history and ethnography of the Amazon region. Dr. Jacques Huber, chief of the section of botany, has been appointed director of the museum.

Dr. Edward S. Morse has been elected a foreign member of the Astronomical Society of France.

Sir James Dewar, Jacksonian professor of experimental philosophy at Cambridge University and Fullerian professor of chemistry at the Royal Institution, London, has been elected a corresponding member of the Academy of Sciences at Copenhagen.

The Royal medals of the Royal Geographical Society have been awarded to Dr. Francisco Moreno, for more than twenty years work in South American exploration, and Captain Roald Amundsen, the Norwegian explorer, for his voyage through the Northwest Passage, and observations in the neighborhood of the North Magnetic Pole.

The grand prize of the Milan Exhibition of 1906 has been awarded to the Wellcome Chemical Research Laboratories for their exhibit illustrating researches conducted in the laboratories. Gold medals were awarded to Dr. F. B. Power, director, and to Mr. F. Tutin and a silver medal to Mr. P. E. F. Perrédès.

Professor W. Kükenthal, director of the zoological laboratory at Breslau, and Dr. H. Hartmeyer, of the Berlin Zoological Museum, have been sent by the Berlin Academy of Sciences to make collections and studies in the West Indies.

Commander Peary has been given three years leave of absence by the Navy Department, and it is said that a fund of $200,000 has been provided to enable him to continue his researches in the Arctic regions.

Dr. William Osler, regius professor of medicine of Oxford University, will deliver the principal address at the celebration of the fiftieth anniversary of the Pathological Society of Philadelphia, which will occur on May 10.

During March Professor T. C. Chamberlin, of the University of Chicago, delivered three lectures on geological subjects at the University of Wisconsin.

Dr. Friedjof Nansen will read a paper entitled 'Polar Problems' at the meeting of the Royal Geographical Society on April 29. On May 13 Lieutenant Boyd Alexander will describe 'An Expedition from the Niger to the Nile.'

On the thirteenth of May Augustana College, Rock Island, Ill., will celebrate the bicentenary of the birth of Linné. Professor Bessey, of the University of Nebraska, is to deliver the address, which will be on 'The Place of Linné in the Scientific World.' This date (old style) has been selected in order not to conflict with the exercises of commencement week, which come at the anniversary, new style (May 23).

A joint session of the Departments of Mathematics and Physics was held at Clark University, on April 15, in commemoration of the two-hundredth anniversary of the birth of Leonhard Euler. Addresses were made by Professor W. E. Story on Euler's life and work, and by Professor A. G. Webster on his achievements in connection with physics and astronomy.

A monument in honor of Eugène Risler, director of the National Institute of Agriculture at Paris from 1879 to 1900, was unveiled in the garden of the institute, on March 24, by M. Ruau, minister of agriculture.

The Rev. Dr. James Addison Quarles, D.D., LL.D., for twenty-one years professor of moral philosophy at Washington and Lee University, died on April 13, at the age of seventy years.

Professor Arthur Baessler, known for his work on the archeology of Peru and for his collections from Peru, given to the Berlin Museum, died on March 31.

To the list of government appropriations for scientific purposes for the fiscal year ending June 30, 1908, published in SCIENCE last week, should be added the following, under the head of Permanent Appropriations:

To the Agricultural Colleges in 48 States and Territories, under act of August 30, 1890 (Morrill Act)	$1,200,000
To the Agricultural Experiment Stations in 48 States and Territories, for original research, under act of March 16, 1906 (Adams Act)	$432,000

MEDICAL journals state that among the appropriations recently voted by the German parliament is $50,000 for repression of typhoid fever and $30,000 for tuberculosis; $16,000 for study of sleeping sickness; $17,500 for the approaching International Congress for Hygiene at Berlin, September 23–29; $25,000 for research on syphilis; $6,500 for investigation of the statistics of accidents and $10,000 for combating infant mortality.

IT is reported that Mr. A. P. Widener has offered to erect in the city of Philadelphia a municipal art gallery at a cost of $10,000,000 and to cooperate with others in providing a collection of paintings.

SIR ANDREW NOBLE has given £200 to the Royal Institution for the fund for the promotion of experimental research at low temperatures.

THE committee on seismology appointed at the recent meeting of the American Association for the Advancement of Science, held its first meeting at the Cosmos Club, Washington, on the morning of April 19. The question of a National Bureau of Seismology was one of those considered by the committee.

THE American Academy of Political and Social Science held its eleventh annual meeting at Philadelphia, beginning on April 19. The special topic for consideration was 'American Colonial Policy and Administration.' Ambassador Bryce made an address on 'Some Difficulties in Colonial Government encountered by Great Britain, and how they have been met.'

THE American Mosquito Extermination Society held its fourth annual meeting in New York City on April 19. Addresses were made by Dr. L. O. Howard, chief of the Division of Entomology, U. S. Department of Agriculture, and Dr. E. P. Felt, New York state entomologist.

A TELEGRAM was received at Harvard College Observatory on March 15 from Professor G. C. Comstock, director of Washburn Observatory, stating that a comet was discovered by Mellish at Madison, Wis., on April $14^d.679$ G. M. T. in R. A. 6^h 50^m and Dec. $+8°$.

Daily motion in R. A. + 3°.
" " " Dec. + 7°.

The comet is large, faint and diffuse, and is visible in a small telescope. The comet has since been observed at the Yerkes Observatory, the Lick Observatory and the U. S. Naval Observatory. At the latter institution the elements and ephemeris have been computed from observations made on April 15 and 16 as follows:

Elements

Time of passing perihelion	$= T =$ 1907, March $27^d.56$ G. M. T.
Perihelion minus node	$= \omega = 328° 47'$
Longitude of node	$= \Omega = 189° 07'$
Inclination	$= i = 110° 12'$
Perihelion distance	$= q = 0.924$

Ephemeris

G. M. T.	R. A.	Dec.	Light
d	h m s	° ′	
1907 Apr. 18.5	7 19 49	+26 01	0.59
" " 22.5	7 45 40	+35 36	
" " 26.5	8 3 41	+40 58	
" " 30.5	8 17 08	+44 17	0.11

The British Medical Journal says: "Just twenty-five years ago, on March 24, 1882, Robert Koch gave to the world his discovery of the tubercle bacillus. At a meeting of the Berlin Physiological Society he read a paper on tuberculosis, in which he reported fully his culture experiments, and ended by saying that the result of these researches was that certain bacilli were invariably present in tuberculous tissue, that these bacilli could be detached from the organism and preserved in pure cultures for a length of time, and that animals infected by various methods with the isolated bacilli become tuberculous. From this it is fair to conclude that tubercle bacilli constituted the true cause of tuberculosis, which therefore must be considered a parasitic disease. Six months earlier, in the autumn of 1881, Koch had demonstrated his culture and staining methods to the International Medical Congress in Lord Lister's laboratory. On his return to Berlin he continued his researches, and, making use of Ehrlich's methylene-blue pigment, discovered a bacillus present in none but tuberculous matter. In order to bring this out clearly on the plate, Koch stained with Bismarck-brown, and found that the bacilli retained their blue color, while all the rest grew brown. This gave him a method of demonstrating the presence even of isolated tubercle bacilli."

UNIVERSITY AND EDUCATIONAL NEWS

Yale University has received two bequests of $100,000 each—one from William C. Eggleston, of New York City, and one for a scholarship from Lura Currier, of New York City, accruing by the death of E. W. Currier.

At the University of Nebraska a general advance of two to three hundred dollars in salaries of deans and professors and of about one hundred dollars for instructors has been ordered by the regents.

At the same institution the regents are planning to begin the erection of an Engineering Hall. At least $50,000 will be spent upon the foundation and the lower stories during the present year, and it is hoped to complete it later with as much more. On the Experiment Station Farm they plan to erect a new heating plant, a stock pavilion, a feeding barn and to complete the woman's building, for which the sum of $100,000 was appropriated by the state legislature. They will expend $25,000 during the next two years upon the North Platte substation, mainly in permanent improvements, and have set aside a fund of $15,000 for state farmers' institutes for the biennium.

The Massachusetts Institute of Technology has received a bequest of $5,000 by the will of Alexander S. Wheeler, of Boston.

The medical building of McGill University was destroyed by fire on April 15. The building was erected at a cost of about $350,000 and contained collections and apparatus of great value. It is said that about half the loss is covered by insurance. It will be remembered that an equally disastrous fire destroyed the engineering building of McGill University about two weeks ago.

The University of California announces the establishment of a resident graduate fellowship in anthropology of a value of five hundred dollars. Applications for the award for 1907–'08 should be sent to the secretary of the Department of Anthropology of the University, at the Affiliated Colleges, San Francisco, on or before May 10, and should be accompanied by all information or records submitted in support.

Dr. C. Judson Herrick, professor of zoology at Dennison University, has been elected professor of neurology in the University of Chicago, the appointment to take effect at the beginning of the next academic year. The chair at Denison University will be filled by Dr. George E. Coghill, professor of biology at Willamette University, Salem, Oregon.

At the Johns Hopkins University, Dr. H. S. Jennings has been promoted to the position of professor of experimental zoology.

At the recent annual meeting of the regents of the University of Nebraska the following promotions to full professorships were made in the scientific departments: A. L. Candy from associate professor of mathematics to professor of pure mathematics; H. H. Waite from associate professor of bacteriology and pathology to professor of the same; C. C. Engberg from assistant professor of mathematics to professor of applied mathematics.

Dr. Corbett, professor of pathology in Sheffield University, has been appointed lecturer in bacteriology in Cambridge University. He is succeeded at Sheffield by Dr. J. M. Beattie, senior assistant to the professor of pathology at Edinburgh University.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

EDUCATION OF THE PROFESSIONAL CHEMIST[1]

IN conforming to the time-honored custom of presenting an address as retiring chairman of Section C, it occurred to me that I might not select a subject more worthy of thoughtful consideration than the education of the professional chemist. In view of the fundamental changes in conditions throughout the commercial world, affecting educational institutions by bringing forward aspects of mental training and practical applications that have no precedents in our earlier experience, it is certainly essential that this influence on modern progress receive critical attention. The recent excellent papers expressing the views of eminent teachers and practical chemists on suitable requirements for the training of young men in chemistry have ably presented the various aspects of this important subject. Yet I have thought that the last word has not been said concerning certain features of professional education, especially relating to the mental attitude of the student, and of the embryo chemist about to enter business life. It can not be doubted that the courses offered by schools of science deserve all the attention they have received in the form of suggestions and criticisms, particularly from the experience of older graduates gained in close touch with great operations in the industrial world. The

[1] Address of the vice-president and chairman of Section C, New York meeting, December, 1906.

term professional chemist is suggested to include the chemical engineer, the engineering chemist, the expert and the consulting chemist.

Even those of us who have been actively occupied in the broad field of chemistry as teachers or professional workers during the last quarter or third of the century, find it difficult to realize the great transition within this period. It is inevitable that the all-pervading commercial spirit of our generation should be felt in institutions of learning, perhaps more forcibly in schools of science, where students have an immediate prospect of seeking their fortunes in the absorbing vortex of business activity. The benefits that have come to those institutions as one result of the recent unparalleled industrial and business expansion have been accompanied by certain disturbing influences that are difficult of control. With such a golden flow into the educational coffers, new institutions have grown to large proportions like the traditional mushroom, and the older ones have doubled or quadrupled in capacity and power.

Naturally, every student within college walls has his attention attracted to the courses of wealth which provide these great benefactions. It suggests a possibility of a similar pecuniary reward even to the extent of limiting his interest to subjects and their especial features that shall in his estimation lead to immediate advancement and profit on his exit from the collegiate environment. I believe I am stating a part of the common experience of teachers, with reference to the mental attitude of a considerable proportion of the student body. Impatient of attainment along the lines of self-improvement and broad culture, the student regards his institution merely as an influence in gaining an initial foothold, satisfied with the minimum requirements that it will accept for his respectable graduation. I do not wish to express the belief that this feeling is universal, but it is sufficiently evident both as to outward expression on the part of some, and through the influence of association on the part of many, to interfere seriously with the highest ideals of intellectual training. It tends to limit the efforts of teachers to a monotonous routine, and the maintenance of an acceptable standing of students in this routine.

It can not be doubted that the conditions of instruction in the highest institutions may be greatly improved by certain changes in the secondary schools. Much has been done within the last few years mainly with the aid and cooperation of teachers in the higher institutions; but the suggestions for better work in chemistry have been limited rather to improvement in methods already in use than to fundamental changes to eliminate primary causes. Judging from the results of entrance examinations, the pupil is taught a text-book rather than chemistry, a fatal error with young pupils, for when they once begin to depend on the printed page, their vision of what they should learn is obscured by their false support. Observation of natural facts should be the only guide until the pupil has a sufficiently well-grounded method to enable him to distinguish between his own observation and printed descriptions. On account of the inherent inertia of young pupils and their inclination to lean on the teacher or the text-book, it is not safe to permit the use of a book until they have acquired a good knowledge of facts and deductions.

Why chemistry should be relegated to the fourth year of the high school, and then suddenly launched on the pupil as a full-fledged science, usually prefaced, as in the ordinary text-book, with a series of definitions and a statement of theoretical principles, almost before the correct observation of a single fact, it is difficult to understand. I believe the reason usually suggested is that chemistry can not be taught to immature pupils of the lower grades. With the text-books at present in use, I appreciate the force of this suggestion, and it applies with the same weight to the grade to which it is introduced, the third or fourth year of the high school. At the age of most susceptible inquiry the child is in an environment in which he is constantly confronted with chemical changes, yet he is permitted to know nothing even of the air he breathes, or the water he drinks, except that the former is hot or cold, and that the latter quenches thirst, until he reaches the age of fifteen or sixteen years. At this period, having spent one third or more of his time on arithmetic and higher mathematics, and the remainder in text-book drill on other subjects, his mind has lost to a large extent the elasticity of earlier years, and he naturally attacks any new subject by the same routine methods. It may easily be demonstrated that correct habits of observation and inference in chemistry and certain elementary facts that every person should know can be taught in the grammar grade. The pupil whose school life terminates with the grammar school may demand as his due that he shall receive such instructions. His capacity is fully equal to it; it is necessary with whatever department of business he may be connected; it is one of the most important elements of his education. Why should he not have it? No extensive laboratories are needed; no expensive apparatus. The expense is but a trifle. But it does demand the teacher; one who can put aside the text-book, and present chemical changes from nature's standpoint; one who has the aptitude to lead the youthful mind from the simple observations of every-day life to less obvious chemical changes, until before the pupil is aware of it he has acquired the habit of thought essential to the proper study of elementary chemistry. I grant that the latter requirement is the most difficult of all; but it is one of the pressing needs to place instruction in the grammar grade on a sensible basis; teachers properly trained to teach the elementary facts of nature. With such a foundation the high-school course may be made more effective, and it should then properly be assigned to the maturer years. It should include a comprehensive study of facts and elementary reasoning, but with limited theoretical conceptions, which should be left to the broader courses of the higher institutions.

Teachers of large experience who are fortunately in touch with the details of manufacture in large plants, have a vivid sense of the marvelous expansion that is in progress all over the world and especially in our own country. While the practical chemist must know something of all the important branches of manufacture, it is beyond his capacity to acquire a working knowledge of more than a limited range of processes, and to keep pace with the advance in improvements. The outcome of this situation is the intense specialist, one who not only knows what is common knowledge of his subject, but who has carried his study and application beyond the border. This sort of man is in active and increasing demand; his service is much sought for, and he can command his remuneration. The successful man of business, especially the chemical manufacturer, is of necessity a specialist in his particular field. He is keenly alert in acquiring knowledge of new advances. But in the many ramifications of his business, he often finds himself beyond his depth, and it is here that he appreciates the service of the broadly trained man. As never before, successful manufacturing operations are dependent on the application of scientific principles in a scientific manner. Such a position promises most to the young chemist, and the sooner he comprehends its possibilities the more rapid will be his promotion. When he steps out into the great school of active life, so far as his practical knowledge of details are concerned, he should understand in all modesty that he must begin at the lowest round of the ladder. While gaining his business experience and patiently waiting for the opportunities, which will not be long delayed, when his scientific attainments shall be called into service, he will escape the criticism frequently made by manufacturers that the young graduate is apt to assume a grasp of practical details that he has not acquired.

Most manufacturers have a high respect for the advantages afforded by scientific education, and are ready to receive the young graduate with open cordiality, expecting service that they stand in need of. With too great confidence in his grasp of practical details, often the manufacturer assigns duties involving business experience which the young man has yet to gain, with disappointing results. In thus rashly undertaking such work he not only brings discredit on himself, but arouses a feeling of distrust concerning the practical utility of scientific training in general.

It may, perhaps, be said of the professional chemist that his career depends in no small degree on ancestral inheritance. I am inclined to the belief that this is true with reference not only to inborn energy and ability without which the best success is not possible in any direction, but especially to a certain inherent quick perception and logical turn of mind, that many persons do not possess and can never acquire. Certain it is that no amount of training can produce a successful chemist out of an individual who lacks these essential qualities. An extended experience in fitting young men for employment in chemistry leads, I think, to the conviction that it is not a large proportion of the average body of students that have the peculiar mental poise and manual dexterity to become successful chemists in a broad sense. As to what should be regarded as success, pecuniary compensation may in general be accepted as a measure of ability, although many men in responsible positions will not accept such a standard since they feel that their service is not recognized in proportion to the profits they help to accumulate in the business treasury. Generally speaking, however, with the unprecedented demand for good men, ability is well compensated. With respect to young men just gaining a foothold there is evidence that their service is appreciated. Statistics have recently been collected from graduates of five years' standing from Case School of Applied Science in all departments, with reference to what salaries would induce them to accept other positions on the basis of what they are now receiving. The average in chemistry was $3,000. After ten or fifteen years of service, I have reason to believe that salaries of professional chemists equivalent to $5,000 and $10,000 a year are not uncommon. While the standard of pecuniary compensation is not unreasonable from the point of view of the man of business, who looks on his ledger balance at the end of the year as a measure of success, the professional chemist seeks also a higher reward. He can not sever his mental connection from the profound truths of nature, and the more he struggles for the mastery and utilization of those truths, the deeper becomes his reverence for the immutable laws that control and direct his efforts. He is constantly under the stimulating influence of new discovery and an expanding field of usefulness. It is, perhaps, unnecessary to suggest that this does not apply to the man who is content with a daily routine, with no connections outside of his laboratory walls, but, as mentioned above, to one who is equal to present demands; who is alive to the advance of knowledge in his particular field the world over; who is able to keep in touch with new applications and new processes, and to apply them with discretion. To such a man, imbued with the spirit of research and the instinct of successful application, the best reward for unselfish effort is its appreciation from the point of view of professional standing, which even the manufacturer, after he is assured that the ledger balance is satisfactory, is eagerly ready to recognize.

An incidental advantage to the professional chemist, one of the features of his education, is the readiness with which he is able to grasp the essential principles of business operations. I have been impressed by the frequent expressions from older graduates as to their feeling of indebtedness to a chemical education for success in the application of business methods. They recognize the fact that the close observance in experimental study, constant exercise in inductive reasoning, and the consequent truthful attitude of mind, the directive forces in the study of chemistry, establish in the individual the habit of looking at all matters that concern him from a similar point of view.

If this be the situation that confronts the young graduate when he enters business life, what shall be his preparation for the duties that await him? What is the responsibility of the institution that sends him forth? First of all, he must be a well-balanced man physically and mentally, alert to the individual duties of life. The best success in any department of activity depends primarily on the service of a well-nourished, healthy body, and such health and strength is dependent on the observance of correct habits of bodily attention and nutrition, established in early youth. During the critical period of school and collegiate training, it is imperative that the excessive mental strain be supported by systematic physical exercise, or the individual will suffer. It is incumbent on an educational institution to provide suitable physical exercise and to require it of every student. Man is a creature of habit, and correct habits early formed are more readily observed in later life.

In the technical preparation for professional employment, as suggested above, the education in chemistry should begin in early years in the grammar grade; correct habits of observation and reasoning should be made a part of the youthful mental equipment. In the higher institution, without interfering with the time devoted to strictly professional studies, sufficient time should be allotted to language and literature, history, philosophy and economics, to impart breadth of thought and to supplement the culture of the science courses. I am not in sympathy with the thought sometimes expressed that science and science studies do not contain the elements of culture. On the contrary, I believe that the best culture is derived from the proper study of physical science. Why should it not be? Is it reasonable to assume that all culture is limited to the study of man with his imperfections, and his works, what he has been, what he has done, excluding the perfect laws of his environment, the universe? It is evidently true that science courses alone do not lead the student to broad conceptions of his duty to society and his relations to the community, unless they are supplemented by courses, as suggested, in other departments of learning.

In the training of the professional

chemist, two facts should be kept in view. Primarily, he must understand chemical changes. Secondly, he must have the ability to devise and operate machinery and appliances in which chemical changes are to be applied on a manufacturing scale. It would seem that the tendency in some of the modern courses in chemical engineering is to give too great prominence to the engineering features at the expense of time that should be devoted to chemistry, with the production of neither an engineer nor a chemist. There is not time in a course of four years to make a good chemist and a finished engineer in the same individual; neither is it possible to make a good chemist without sufficient time and attention for comprehensive training in chemistry. But it is possible to make a good chemist and to allow him adequate time to gain such knowledge of the engineering features as will give him a good foundation for expansion when he enters the factory. In all departments of business the individual who has learned how to do things himself is best fitted to direct the efforts of others; the superintendent who can saw off a board by the square or set a post straight and true, can instantly see whether another is doing his duty. The broadly trained chemist must, therefore, have had practise in handling tools, in working wood and iron; he needs the elements of contruction, of machine design. He must understand the economic production and application of power from different sources. An important part of his equipment is the nature and manipulation of electrical currents and machinery. Such breadth of knowledge may be gained in courses on shop practise, thermodynamics, applied mechanics, heat and steam, hydraulics and machine construction, attendance on courses in electricity with laboratory practise.

In this connection the recurring question of adequate time in the ordinary course may be satisfactorily answered by the tendency in most institutions to extend the limits of practical work. In Case School of Applied Science, the entire month of June is given up to laboratory and field work as a practise term, which adds about one year of practical study to the course of four years. The student is under constant supervision, with sufficient oral instruction to keep him intelligently occupied. Continuous laboratory practise accomplishes very much more that the interrupted hours of the other terms. In answer to the possible objection that it is some infringement on the time for lectures and recitations, it is true that two or three weeks a year are lost to these exercises; but what is gained in practical application in chemistry makes it about equivalent to a course of five years, and the average student is easily able to accomplish the work mentioned above in mechanics and electricity. Indeed, before this recent change in the additional time devoted to the laboratory, our best students and even those of average ability by dint of hard work were able to finish this course. With no sacrifice, therefore, of necessary training in chemistry, the student receives what is needful when he enters on the practise of his profession. Such a course may be designated as engineering chemistry, in which the student becomes primarily a chemist.

To acquire an adequate grasp of foundation principles in chemistry it is necessary to include thorough comprehensive courses in general, inorganic, analytical, theoretical and physical, and organic chemistry, with much time devoted to laboratory work. This routine is practically the same in all institutions, with increasing attention to experimental physical chemistry. It seems scarcely necessary to allude to the subsidiary subjects: English, modern languages, mathematics, including calculus, descriptive geometry and drawing, physics, mineralogy, elementary use of surveying instruments, and metallurgy, without which a course in engineering chemistry could not be regarded as complete. The increasing importance of bacteriology in the fermentation industries and sanitary chemistry, render it indispensable that the student have some practise in the manipulation of bacteria, and general use of the microscope. He must understand the composition, methods of analysis, valuation and uses of solid liquid and gaseous fuels, and the methods of heating and lighting. In the work of the fourth year, it is essential that the student be led to some extent at least out of the routine of the previous years, to develop greater self-reliance, and to be brought into somewhat close touch with chemical literature, to establish the habit of ascertaining what has been done in any special line of work before undertaking it himself. It is important that he gain a clear understanding of the proper methods of attacking new problems and that he appreciate the necessity of depending on his own resources. In the ordinary course of four years the time that can be devoted to such study is much too limited, but it should be insisted on to the fullest possible extent. The atmosphere of the laboratory should have the stimulating influence of original research. If students feel that they have something before them besides the routine courses it keeps them up to their best effort; they quickly perceive the difference between the spirit of an instructor who has no ambition beyond the hourly recitation and that of one imbued with enthusiasm of original study, and eager to impart something of his own impulse.

Since the chief aim is the preparation of the chemist for efficient service in the manufactory, an essential feature of our discussion relates to the method of teaching industrial chemistry. Evidently in a well-balanced course this instruction should be preceded by the routine work of the first three years, general, analytical, physical, and organic chemistry, all of which is a necessary preparation. Since there is at present nearly as great a demand for men in positions where organic chemistry is requisite as in places which concern inorganic processes alone, the student must be well grounded in theoretical and practical organic chemistry, when undertaking industrial chemistry. I have found it an excellent plan to carry along the course of organic industrial chemistry in the same term with lectures and laboratory practise in organic chemistry. Every operation in applied chemistry is so closely attended by physical conditions affecting economic results, it is necessary that attendant conditions of processes be kept constantly before the student. As one of numerous examples that may be suggested, vapor tension, heat of solution, vaporization, specific heat, are closely connected with distillation, solution, crystallization and purification.

The schools of science owe their foundation primarily to the labors of self-educated men, who without the advantages of systematic training have devised and perfected on a business basis the fundamental methods of the factory. In taking up the burden of his forebears the young graduate is expected to extend it with the advance of modern demands. Shall the school of science attempt to duplicate in a miniature fashion the factory methods with suitable machinery and appliances, or shall its efforts be directed toward broad training in fundamental principles with such laboratory applications as are needed to comprehend an expansion to a manufacturing scale? Evidently this question should be considered from several points of view. It must be remembered that application on a laboratory scale of any manufacturing process in a manner to afford satisfactory results needs more time than can be found for it in the course of four years, unless it be taken as a subject of a thesis when sufficient time may be devoted to a single subject. But general application should include a considerable number of processes. Depending on the nature of the process, not less than two weeks to two months must be exclusively devoted to any important single operation, and the benefit derived is limited mainly to this process. Another serious hindrance is the magnitude of an adequate equipment. A separate plant of two or more buildings is needed, for the dust and dirt of one variety of process could not be tolerated in the cleanly conditions necessary in others. With sufficient equipment a graduate course should afford ample time for such work on a broad plan. Such a course should evidently include a study of details and conditions with reference to possible improvements. It can not be doubted that an adequate equipment in teaching force and plants working in conjunction with manufacturing establishments, and with sufficient endowment for the costly operation, should form the basis for the expansion of a profitable graduate course. A school of science fortunately situated in the midst of a great variety of manufacturing operations has the immense advantage that the manufacturing plants really constitute an important adjunct to its equipment. After thoroughly reviewing the details of a manufacturing process in lectures and recitations, next to actually conducting the operation, the student receives a part of his most valuable experience in inspecting such operation under intelligent guidance during his visits. It is a mutual advantage to the institution and to the manufacturer, for it is an invaluable aid to the courses of instruction in the former, and it insures to the latter a more practical knowledge of processes to the student whom he may later employ.

With the most elaborate experimental preparation, however, the graduate approaches a different atmosphere when he enters the factory where every effort has a pecuniary value. Outside the factory it is not possible to take into account the element of costs, which is the controlling principle in any business. It is easy to ascertain the market values of crude materials and finished products, but the long array of numerical details intervening which constitute the business of manufacture is a closed volume to every one outside the counting room. The guiding thought of the student is the acquisition of accurate knowledge of principles and methods. The chief aim of the manufacturer is to apply those methods in such a manner that the pecuniary results may be entered on the right side of the balance sheet in the shortest possible time. As an example of this difference in motive it is often suggested that students in quantitative analysis be given practise in rapid application of methods; but the fact is forgotten that the student is fully occupied in the attainment of accuracy, and that rapidity is soon acquired. It may take him a day or two to master the necessary dexterity in applying the method for the determination of phosphorus in a single sample of steel. When he goes out into the factory laboratory he may be required to hand in results on thirty samples in a single day. This distinction seems to be best expressed by the difference in environment; the student acquires his knowledge in the quiet atmosphere of the educational institution; the man of business applies his knowledge under the stress of manufacturing conditions.

The service of the school of science for the benefit of the community is well established, but it has not yet secured the tenacious hold of the older college, which, in educating generation after generation, father and son, has established the traditions of the family on scholarship and culture, and extended the influence of collegiate training into the professional and business world. The first generation of graduates from the scientific school has had barely time to make its record, but an honorable record is assured, and it will be continued by their sons and grandsons. Traditional family support of an institution is one of its most valuable assets intellectually and pecuniarily; fortunate is the institution that has this support in the best families.

The school of science in its breadth of training and scope of applications is peculiarly an American institution. In England, until recently, the spirit of progress along similar lines has been somewhat inactive. But aroused by the recent great industrial activity to the fact that their old-time plants must be rehabilitated and that our young men are being called on for assistance, large grants have been made to establish schools of science, and the managers of these funds are inquiring with much interest into the foundation and operation of our scientific institutions. The fact is being recognized that the business world is receiving aid from practical methods that they must adopt to regain their industrial standing.

In Germany the conditions are essentially different. While the labors of the early French and English investigators contributed very largely to the foundation of chemical science, our immediate inspiration came from the German laboratory. More recently the unprecedented expansion of our national resources has developed circumstances so fundamentally different, our institutions have of necessity directed their efforts toward meeting business requirements to the extent that they are unique. Yet the atmosphere of quiet scholarly inquiry of the German institutions still retains its influence. The great body of German scholars in happiness and contentment devote their lives to discovery and elaboration with sufficient remuneration for economical living and simple habits, and the world is benefited by their labors. In the great manufactory such as that of the Badische Aniline Fabrique or of Meister, Lucius and Bruening hundreds of chemists are employed, a large proportion with doctorate degrees from the universities. Those men are devoting their best efforts to the interests of their employers in the national spirit of faithful application and with very moderate compensation. If such establishments were compelled to pay salaries equivalent to those granted for similar service in our own factories, it would doubtless make a serious inroad in the very generous dividends they are now able to declare.

It is extremely doubtful whether the conditions in the German university, including their long vacations, their leisurely habits during the semesters, and with no restraint on the student except attendance at final examinations, are applicable in American schools of science. Certainly their traditional method of allowing the student, whatever his capacity, to work out problems on his own resources, even before he is well grounded in breadth of practical methods, could not meet our requirements.

I am sure there will be no dissent from the view that original research is of equal importance in the school of science as in the university, although evidently it should take a somewhat different form in the scientific institution. In the university laboratory, research has for its object the advancement of knowledge with no visible practical utilization. The same spirit must pervade all research; that is, its foundation rests on the principles of pure science. In the scientific school the great field includes the application of the principles of pure science to the solution of commercial problems. To one who is conversant with the conditions in the manufacturing plant that lacks the aid of men skilled in science it is evident that much of the work is the result of disconnected observations loosely made in a rule-of-thumb fashion, rather than the outcome of systematic study of underlying principles with expansion to methodical application. There is urgent need of the extension in all directions of the critical comparison of methods of analysis and testing which has made such a good beginning. To appreciate the importance of this work it is only necessary to glance at the want of concordance in the results of analysis of a given substance even from the best laboratories. The recent establishment of the national bureau of standards is an excellent foundation for greater accuracy and closer agreement in the results of different workers.

The education of the professional chemist is concerned in the recent discussion on the relations of the scientific school to the university, and the cogent reasons suggested for its rehabiliment as one of the professional departments with law, medicine, etc. If such a change were feasible doubtless scientific education should proceed along much the same lines, for the seeker after its benefits could not otherwise secure his training, and the instruction would of necessity conform to the demands of business. The endowment of other schools of science is not probable; the present tendency is rather to expand scientific training in institutions already established. This is especially true of the state universities, in some of which the best practical training in science is given. The permanency of those institutions in their reliance on the state gives promise of the broad development of practical science. What may be said in this direction relates, therefore, to a comparatively few institutions, and it evidently applies to a few conditions concerning which there are doubtless differences of opinion—culture, educational atmosphere, policy of management, economy in resources. That the student of applied science should gain in culture and breadth of thought in the university atmosphere may be true with corresponding changes in his work; but the strictly professional training can not be abbreviated, and it is believed that culture should be imparted in these courses as now conducted in the scientific institution. As to whether the university atmosphere is well adapted to the close application and vigorous effort required in the school of science, may be questioned. Besides certain small economy in office supervision, it does not appear that any important reduction in running expense is possible. There is a limit to the number of students in a single recitation or laboratory division. A given number of students in the scientific school needs the same number of teachers and the same equipment as in the university.

In respect to policy and administration it would seem that the school of science has an essential advantage over the professional department in the university. The head of the scientific school devotes his best thought and energy with an intense personal interest to his institution. Such devoted service from a president thoroughly imbued with the scientific spirit, with such expert knowledge and force of character that his word has carrying power with the public and with governing boards as well as within the institution, and with business instincts that enable him to meet men of affairs on grounds of mutual interest and understanding, must of necessity be more efficient than that of the dean who is interested in his department as one element of a great university, not wholly independent in applying his convictions nor untrammeled by other considerations incident to his department. It is no doubt true that the conditions affecting the school of science in its relations to the business world are essentially different from those of law and medicine, as integral elements of the university.

With reference to a combination in resources which is the chief source of strength in the great modern business corporation and its application along similar lines in the university, the successful element in the manufacturing corporation is the elimination of manual labor with its uncertainties and imperfections, and its more general displacement by machinery that is invariable in its operation. It is admitted that without the larger substitution of labor by machinery, the same efficiency and economy are not secured in manufacturing plants in which details are altogether in the hands of salaried employees, as was possible in the earlier period when smaller branches of business were under the direct personal supervision of the owners. On the other hand, in the educational field, mind is the immediate controlling and directing power in every detail, with no intervention of mechanical appliances. The teacher can not direct his students and carry on his instruction from a distant city, or from another continent.

The essential principle of combination in business, that modern conditions demand great accumulations of capital and resources for control in competition and commerical fluctuations, does not apply in a similar degree to any successful educational institution, for there is a far greater demand for graduates than can be supplied, and there is little difficulty in securing necessary funds for carrying on fundamental lines of educational work. If it be true, as has been stated, that the German universities are much overcrowded with respect to employment after graduation, and that strenuous efforts are put forth through German residents in this country to place their graduates here, the element of competition is not wanting, or may not be in the immediate future. Such competition may even be a stimulating advantage, inciting the workers here to greater effort. Already we have cause for congratulation in the standing of our research laboratories, especially in physical chemistry. It is an indication of a leading position in research and the advancement of knowledge to which the devoted labors of the numerous able investigators in this country during the last thirty years have contributed. A powerful aid, which it is all too soon to appreciate in its desirable results, is the immense funds for research recently established. Although primarily these grants are made in furtherance of work in pure science, evidently applied science and professional chemists will be greatly benefited. Perhaps the wisest grant of all is the Carnegie foundation for the retirement of teachers, for in a measure it relieves the teacher during his earlier years from the anxiety of later need, and gives him courage to devote his residual energy in some efforts for the advancement of knowledge. Every institution named in this grant is benefited in its standing, for it may insist that applicants for positions have adequate training for research, and that their ambition lie in that direction.

In the changes in business management during the last decade, and the transfer of business control from private ownership to great corporate bodies in which the vested property rights have passed into possession of the general public by the distribution of shares and bonds, there is an indication of a tendency toward the creation of a class distinction in labor as well as in property control. It is not at present so apparent in its results as it will doubtless appear later when the very prosperous conditions of business and consequent increase in private fortunes and lavish expenditure shall inevitably be affected by enforced economy of a more stringent commercial situation. At present all forms of labor are employed with increased compensation and with a serious deficiency in many directions. But there is an evident tendency toward a lower valuation of mere manual force and an increased valuation of mental agency. In corporate control success depends, in some directions altogether, on the personal qualifications of a superintendent or manager, and the right sort of men are much sought for and difficult to secure. The pressing need is for managers of executive capability for manipulating men and methods, and it has developed so suddenly, it is all too soon for the production of an adequate supply. A great business corporation can be created in a day; but years are necessary to produce men who can manage its departments.

Class distinction in labor will appear in the elevation of skilled labor, the thoroughly trained man of science of executive ability, and the manager, and the degradation of brute force as it is embodied in the individual whose possession consists only in what nature has conferred on him. What will become of the latter as the labor of his hands is more and more completely replaced by machinery that can do his work better, is one of the great problems of the future. It will be a question of the survival of the fittest, and one that has a lively interest for the youth who is now deciding what his future shall be, for this, and other similar phases of social and industrial life, will be brought prominently into view during his generation.

A feature of modern progress that has an intense personal interest to every individual in active business life is the tenure of activity. Formerly the connection with affairs of business was coincident with life itself. But the old-time methodical, slow-moving habits of the last generation of business men whose attendance in the counting-room included the daily working hours, has given place to close connections by telegraph, telephone, limited trains and other inventions that tax human endurance. The father attended to his correspondence, writing all his letters; the son takes up a pen only to sign checks and documents. The youth of the present day must amass his fortune, make his reputation, get his enjoyment and pleasure in business affairs and be ready to resign his place to another at the age of fifty or sixty years, at just the period of life when the normal man should be in his prime, and able to render his best service by reason of his experience and mature judgment. This is a serious limitation for young men who have spent eight years or more of their lives in gaining an education, although the graduate of the school of science has an advantage over those who select other professions that require three years or more of additional preparation, which with subsequent office practise render it scarcely possible for the young lawyer or doctor to start out in business for himself much before the age of thirty years. In taking a position immediately after graduation in the manufactory, the man of science is making his way while he is gaining his business experience. Ten or fifteen years at least are necessary to develop the productive capacity of any man; and the remaining few years permitted to him for accumulation demand the utmost concentration of effort. What will be the result of this intense strain, and the effects of such a time limit on what the average man hopes to accomplish, can only be ascertained by future observation. But the situation is inevitable, and the young man is wise who heeds for his future the counsel and experience of his elders.

It would seem that these various influences have an important bearing on the present and future conditions of practical education in determining what the student may expect who seeks a suitable preparation for the profession that will absorb his best energies and that shall constitute his life-work. It is of less consequence where he is educated, provided he attains the mental poise and attitude that enable him to grasp fundamental truths and to apply them correctly in the accomplishment of great undertakings.

C. F. Mabery
Case School of Applied Science

SECTION K—PHYSIOLOGY AND EXPERIMENTAL MEDICINE

SUMMARY OF THE PROCEEDINGS

There were three meetings of the section during convocation week.

The first session was convened on Thursday, December 27, at 2:15 p.m., at the College of Physicians and Surgeons, in the presence of an unusually large audience. At this meeting the officers for the year 1907–8 were elected; the retiring chairman, Professor William T. Sedgwick, delivered the annual address; and a symposium was held on the subject of 'Protozoa as Factors in Disease.'

The second session was held on Friday, December 28, at 10 a.m., at the Rockefeller Institute for Medical Research, in affiliation with the Society of American Bacteriologists. Twelve papers were presented.

The third session was held on Saturday, December 29, at 10 a.m., at the Rockefeller Institute for Medical Research, in affiliation with the American Physiological Society. Sixteen communications were offered.

EXECUTIVE PROCEEDINGS

The following officers were elected for 1907–8:

Vice-president and chairman of the Section—Ludvig Hektoen.

Secretary—William J. Gies.

Sectional committee—Simon Flexner, vice-president, 1906–'07; Ludvig Hektoen, vice-president, 1907–'08; William J. Gies, secretary, 1905–'08; Charles S. Minot (one year); J. McK. Cattell (two years); Frederick G. Novy (three years); Graham Lusk (four years); Jacques Loeb (five years).

Member of the Council—S. J. Meltzer.

Member of the General Committee—Edward K. Dunham.

SCIENTIFIC PROCEEDINGS

Program of the First Session, December 27, 1906

Vice-presidential address—'The Expansion of Physiology': William T. Sedgwick. (Published in Science, this volume, page 332.)

Symposium on Protozoa as Factors in Disease:

Introductory remarks by the chairman: Simon Flexner.

'The Protozoa from the Standpoint of the General Naturalist': Edmund B. Wilson.

'Some General Principles in connection with Protozoa as Factors in Disease': C. W. Stiles.

'The Protozoan Species': Gary N. Calkins.

'The Morphological Diagnosis of Pathogenic Protozoa': James Ewing.

'Immunity against Trypanosomes': F. G. Novy.

General discussion by William H. Welch, Henry B. Ward and James Carroll.

The Protozoa from the Standpoint of the General Naturalist: Edmund B. Wilson.

The zoologist who is asked to open a discussion on pathogenic protozoa with a few remarks on these animals as they appear from the standpoint of the general naturalist must approach his task with somewhat mixed feelings. There is hardly a field in zoology more interesting or fuller of suggestion; but neither has any other been oftener traversed or more widely exploited. Indeed the subject has been so often discussed, its importance is so well understood, that I am tempted to begin and end with the celebrated remark of Colonel Ingham's double that "there has been so much said, and on the whole so well said, that I will not further occupy the time." And yet there are certain aspects of the subject to which one may again and again return without loss of interest, which are a perennial spring of new ideas and new research. First and foremost among these is the fundamental analogy pointed out by Virchow and Haeckel between the animal body and an organized social state. The conception that the multicellular body is a 'cell-state'—a community of cooperating elementary organisms that are individually comparable to protozoa—made a deep and lasting mark on all morphology, physiology and pathology. Apparently there is no end to the fruits that it has produced, continues to yield, and seems likely to bring forth hereafter. The conception is no doubt an inadequate one. There are some, perhaps many, biological processes that can not adequately or profitably be considered from this point of view alone. Especially in the field of growth and development there are processes that are better treated as the action of a single and indivisible physiological unit than as a resultant of cooperating cell-activities. But, whatever its limitation may be, the conception of the 'cell state' remains one of the most brilliant, interesting and fruitful of the fundamental generalizations of biology. It is part of the air we breathe in every biological laboratory from the day we first sit down to the microscope, and in one way or other it pervades the whole tissue of our work. As such it needs no analysis at my hands.

But there are certain applications of this conception to some of the broader questions of our science on which for a moment I may appropriately dwell. For instance, our conception of heredity, on which the whole modern theory of evolution turns, has been profoundly affected by the phenomena of reproduction in the protozoa; and the same is true of the whole constellation of problems relating to sexuality, the duration of life, old age, and the renewal of vitality by fertilization, all of which are in close relation to the problems of heredity. A great number of the modern researches on these questions can be recognized as fruits of the celebrated comparison, drawn by Bütschli thirty years ago, between the life-cycle of a protozoan race and that of a multicellular animal. Fertilization of the egg is analogous to the conjugation of the protozoa; cleavage and development to the successive divisions of the ex-conjugants and their descendants; maturity, decline and old age in the multicellular organism to the physiological balance and the ensuing gradual failure of vitality after prolonged vegetative reproduction in the protozoa; the stimulus to development given by fertilization is comparable to the renewal of vitality that follows conjugation in the protozoa. This comparison has directly or indirectly stimulated a multitude of important and interesting researches on the fertilization of the egg, on artificial parthenogenesis, on the chemical renewal of vitality in the protozoa, on the stages of growth and decline, on the causes of old age and natural death, and many others. There is not one of these questions that can be adequately considered apart from the phenomena exhibited by the protozoa. It is my impression that we are more likely to solve these problems, in so far as they can be solved, by studies on the protozoa than by investigation on the higher forms. Some of the researches in this field have definitely brought into the arena of scientific experiment and discussion the old dream of the alchemists of the artificial prolongation of life, perhaps of human life. If this is no longer a fantastic vision of pseudo-science but has won a place among the legitimate subjects of scientific inquiry, it is in no small measure owing to investigations on the protozoa.

If we turn for a moment to the study of heredity and evolution, here again it was in considerable measure a comparison of the life histories of protozoa with those of metazoa that necessitated a readjustment of many views that had long been held, almost without question, as applicable to the higher forms of life. Considerations on reproduction and the duration of life in protozoa formed the starting point of Weismann's inquiries on heredity that resulted in a total denial by him of the inheritance of acquired characters in higher organisms —a supposed factor that formed an important part of Darwin's general theory and was the very corner-stone of Herbert Spencer's philosophy of organic nature. Whether Weismann was right remains to be seen. Naturalists are still in disagreement as to whether acquired characters are inherited or not. But there can be no doubt that Nussbaum and Weismann altered our whole point of view by fixing attention on the fact that heredity is effected in metazoa, as in protozoa, by cell-division; that just as protozoa inherit the characteristics of the mother cell because directly derived from it by division, so the body of the higher animal inherits not from the body of the parent but from the egg, because derived from it by division. This simple fact, while it places the basis of heredity in its true light, enormously increases the difficulty of accepting the inheritance of acquired characteristics of the body. That such an inheritance of acquired characters is difficult to conceive does not, of course, prove that it is not a fact; but the arguments urged by Weismann were of such force as to demonstrate the imperative need of a reexamination of the whole question. The illuminating view of heredity brought forward by Nussbaum and Weismann, primarily suggested, I repeat, by the mode of reproduction in protozoa, has made a deep mark on biological research, and has led to reinvestigation of many conclusions that had naïvely been taken for granted, with no realizing sense of the formidable difficulties that they involved, or the weakness of the evidence on which they had been based. Thus the study of the protozoa has had its effect on every part of biology that is concerned with questions of historical descent; and I do not think it forced to say that these creatures may have something to teach us in every department of thought into which evolutionary considerations enter.

It was Weismann's view that the protozoa are endowed with never-ending life. From the physical point of view, this conclusion was not altogether well founded, at least in the sense in which he meant it; but metaphorically speaking it was true. They certainly possess a vitality that seems without limit as a power to animate scientific research, to invade new fields of discovery. Their latest achievement is an irruption into the domain of pathology, and the inroads they have already made are sufficiently attested by the choice of the subject of this discussion. I feel myself here on treacherous ground and will venture no prediction as to whither the new subject of medical protozoology may lead us. But evidently the bacteria must look to their laurels with so formidable a competitor in the field. It is within the bounds of possibility that the discovery of the causal relation between protozoa and disease may one day rank with the greatest of those that general biology owes to the study of these simple animals.

Some General Principles in connection with Protozoa as Factors in Disease: C. W. Stiles.

The speaker first directed attention to the difficulties of classification and the necessity for extensive academic work on protozoology independent of the subject of applied protozoology. Because of incomplete knowledge of the protozoa, we must expect to meet with extensive differences of opinion in reference to numerous species or alleged species, for some years to come. These differences in interpretation are at present inevitable and should be accepted as having a right to existence; they do not necessarily indicate any special prejudice or unreasonableness on the part of contending authors. Zoologists are at present not in a position to concisely define the protozoa as a systematic unit, nor can they yet define sharply the different groups from one another. We must, therefore, have patience at present and receive with open minds, although with some reserve, many interpretations that are published. He referred to the two biologic rules he had enunciated in 1901, to the effect that diseases which are conveyed mechanically by arthropods (as facultative carriers) may be due to either plant or to animal parasites, but chiefly to the former, while diseases which are conveyed biologically by arthropods (as obligatory carriers) are, so far as known, due to animal parasites. In discussing the relation of Rocky Mountain fever, African tick fever and chicken spirillosis, to these same rules, he held that we must await further study in the case of Rocky Mountain spotted fever, while in the case of tick fever and chicken spirillosis it has not yet been proved that the ticks are biological (obligatory) carriers. In chicken spirillosis the disease has, according to literature, been conveyed through feeding the feces of a sick animal to a well animal, hence in nature, this disease does not seem biologically dependent on the ticks. As for African fever, there are certain indications, though as yet no proof, that this can be conveyed as a wound disease.

Passing to yellow fever, the speaker thought that despite certain opinions to the contrary, the present indications remain in favor of a protozoal rather than a protophytic origin.

The Protozoan Species: Gary N. Calkins.

Formerly it was the custom to base protozoan species on the structure of the single cell. This made it very easy to make new species, for any novice with a microscope might see known organisms new to him and describe them as new species thus burdening the literature with names and pseudonyms. On this former basis there were so few points of structure to base species upon that they were difficult to accurately define.

A species in zoology is an abstract conception of a group of animals based upon similarity in structure, habits and mode of life and power of reproducing amongst themselves offspring identical with themselves.

In the single individual of a species we usually recognize more or less clearly defined variations in vitality and sequence of changes which we designate as youth, adolescence and old age, the whole constituting a life history.

In the protozoan species, according to the old custom in taxonomy, these criteria of species can not be maintained. The individual, in the first place, has no life history to speak of; it is formed by division of a cell; it lives only a few hours as an independent cell and then disappears, its substance going into two or many new cells. There is no indication of the changes in vitality characteristic of individuals of metazoa, that is, no trace of periods of youth, adolescence and old age while natural death of the individual is unknown. Furthermore, in such individuals the criterion of offspring which exactly resemble the parents is not observed, for cells may be formed in the course of the several divisions which do not at all resemble the parent cell and which seen independently would warrant interpretation as a new species. Thus in *Tetramitus* or *Cercomonas* the perfectly rigid contour of the ordinary 'individual' is very different from the amœboid 'individual' which is ultimately formed. On the basis of such variable 'individuals' the protozoan species must be of questionable taxonomic value.

A new basis for the conception of protozoan species was given by Schaudinn in 1900. Always interested more particularly in the life history of protozoa, he gave from time to time more or less complete accounts of the life cycle of different forms, *e. g., Calcituba, Leydenia, Polystomella, Paramœba, Trichosphœrium,* etc., while in this year he founded *Coccidium schubergi* on the basis of the complete life cycle and gave us a model which later students of the group have tried to follow.

The new method of taxonomic research which Schaudinn started has resulted in a far more profound knowledge of protozoan species. A number of supposedly different varieties, species and even genera have been found to be only stages in some life cycle. For example, microsphæric and macrosphæric shells of foraminifera are now known to be only stages in the life history of the individual foraminiferon, and in my own experience some of the commonest forms of microscopic life are found to be curiously related. Thus an organism which formerly any student of the protozoa would have described as a species of the genus *Pelomyxa,* is found to be only a stage in the life history of *Amœba;* and the supposedly different species of *Paramecium*—*caudatum* and *aurelia*—are found by continuous culture in their natural habitat to be one and the same species.

Other examples might be given to show how the 'individual' in the old sense varies from time to time and thus becomes a most unstable subject of protozoan species. For this reason, I have urged that the old idea of the protozoan 'individual' be discarded and the life cycle substituted, and I would recognize as the individual, not the single cell but the entire aggregate of cells that are formed from the time of one conjugation up to natural death of the protoplasm resulting from it, or until syngamous union of that protoplasm with similar substance from another individual.

In such an individual we recognize periods of varying vitality which have the same sequence as in metazoa, and we can find characteristic features which indicate the period of youth, of adolescence or old age as in a metazoon. The variations in vitality may be represented by a more or less regular curve in which these periods are clearly marked out.

It is particularly important that species limits should be clearly defined among the pathogenic protozoa. Here as yet, however, there are very few that are based upon the full life cycle, most of them indeed are on the basis of a few individual cells. For example, Woodcock in his review of the hæmoflagellates enumerates no less than forty-eight species of *Trypanosoma* and adds two additional ones as questionable. I do not wish to be too severe, but I imagine it would be nearer the truth had the account been of two species and forty-eight questionable ones. In this group species are based for the most part on unmeasurable differences in structure; often indeed no such differences even can be made out—but species are defined on strictly physiological characters, such as life in different hosts, inability to live on certain culture media, different reactions towards immune sera, etc., none of which are sufficiently characteristic of species, although they may well indicate specific differences.

In very few cases has the life history of the trypanosome been made out, but the work of Schaudinn, Keysselitz and Prowazek indicates that we have to do with digenetic forms, while Novy's conclusions are the reverse. It seems to me that the culture method can not be relied on absolutely in the testing of protozoan species; to cultivate parasitic protozoa on media is to cultivate them in one phase only of their life history, and since the most important phases have never been seen in the cultivated forms it is probable that the period of youth of the individuals is the best for cultivation. Inability to live in the same medium is no test of a good species. There are many species of protozoa which live both in salt and in fresh water; either if transferred suddenly to the other medium would die, but either could be gradually trained to live in the other medium. So it is, as I believe, with *Trypanosoma*, and the multiplication of species here is, to my mind, only evidence of our ignorance of their life history.

What is true of *Trypanosoma* is even more evident in the case of *Spirochæta*. Here, every month or so, some student of the group describes a new species of *Spirochæta* before we have found out even whether the genus to which it is ascribed belongs to the Schizomycetes or to the Mastigophora. Again the variations in structure of the various spirochætes are so great that if they were all true we should have to make new generic names to hold them. *S. obermeieri*, for example, has the chromatoid granules of a bacillus, it reproduces by transverse division, and is, in its morphological characters much more like the spirilla type than like the flagellate forms. *S. plicatilis*, on the other hand, has longitudinal division, a periplast membrane and nucleus similar to that of the male trypanosome, according to Prowazek. *S. pallida*, finally, has flagella, and reproduces by longitudinal division according to Schaudinn, and has a single nucleus according to Schaudinn, Herzheimer, Krzyztalowicz, Siedlecki and Forrest, and trypanosome phases according to Siedlecki.

Out of this heterogeneous collection it would seem to me that *S. obermeieri* might well be a spirillum while the rest that I have mentioned may be *Spirochæta*. Whatever they are, it is quite evident that we are here in territory which lies between the two divisions of schizomycetes and mastigophora and to tell whether a given *Spirochæta* is a plant or an animal is only an academic matter after all. What we really want to know is the life history of the individual and the form changes through which it passes. When these are ascertained the genera, families, orders and classes, or kingdoms, will take care of themselves.

The Morphological Diagnosis of Pathogenic Protozoa: JAMES EWING.

A review of protozoan morphology shows that all these organisms at some stages of development assume very characteristic forms which leave no doubt that they belong to a living structure. This is especially true of the recognized pathogenic protozoa of man and the higher animals. Hence some zoologists have taken the ground that when a protozoon is encountered under the microscope its characteristic forms can be recognized at a glance. Nevertheless, there has been a constant endeavor to force the acceptance, as protozoa, of various specific intracellular bodies found in cancer, rabies, the exanthemata, and certain related diseases of lower animals, although the structure of these bodies is not clearly protozoan. The danger of accepting these doubtful bodies as protozoa lies in the fact that not only in these diseases, but in others of known bacterial origin (diphtheria, glanders), there are peculiar intracellular degenerative products, more or less specific of each disease and organ, all of which closely resemble some forms of protozoa. Hence in the absence of entirely characteristic trophozoites, sporocysts, or spores, it is unsafe to regard any of the bodies as protozoa. The morphological study of the cell inclusions in cancer, variola, scarlatina, measles, rabies, clavellé, contagious epithelioma of birds, etc., has not led to a definite result in any of them.

In the absence of fully characteristic protozoan forms, the circumstantial or collateral evidence becomes of decisive importance. This collateral evidence in cancer is practically conclusive against the protozoan nature of the disease, and of the various cancer parasites. In the exanthemata it does not seem favorable to the protozoan nature of the bodies described as parasites, nor to the protozoan theory of the origin of these diseases. In rabies there are somewhat more definite indications pointing to the protozoan nature of the disease and of the Negri bodies, but this evidence is still inconclusive. The study of diseases of unknown etiology exclusively from the protozoon standpoint is to be deprecated. There are some grounds for supposing that the above diseases, except cancer, may be due to invisible but not necessarily sub-microscopic organisms or even to peculiar actions of well-known bacteria. The virus in the exanthemata, and probably in rabies, is perhaps connected with the specific cellular products of these diseases, but is itself of a nature not understood, and yet unparalleled among known protozoa.

Immunity against Trypanosomes: F. G. Novy.

It is an established fact that rats which have recovered from an infection with *T. Lewisi* are immune to subsequent inoculation with that species of flagellate. The same holds true for cattle, sheep, goats, etc., that have recovered from the infection caused by the pathogenic trypanosomes, such as nagana, surra, dourine, etc. This condition of active immunity is seemingly possible only in those species of animals that are relatively insusceptible, for with really susceptible species the infection is always fatal.

Heretofore all experiments on artificial immunity against trypanosomes have been made on animals that have recovered from the effects of the parasite which has been living and multiplying in the blood-vessels of that animal. Now that cultures of some of these organisms, as for example *T. lewisi* of the rat and *T. brucei* of nagana, are possible it was desirable to ascertain whether or not they could be used to immunize against the virulent organisms. It may be said, in passing, that cultures of both of these trypanosomes, even after they have passed through a hundred generations or subcultures in the course of two years, do not become attenuated by such prolonged consecutive passage but readily infect susceptible animals.

We have shown, however, that cultures of *T. brucei* can be attenuated by exposure for about two days at 34° C. By repeated injections of cultures thus treated, attempts have been made to immunize rats and guinea pigs against *T. brucei*, but thus far these have been but partially successful. That is to say, there has been at most a survival of a few days of the treated as compared with the untreated animals. The failure to immunize with such cultures is attributable in part to the excessive susceptibility, of the animals employed, to infection with *T. brucei*, and in part to the existence of a negative phase following the injections. It is desirable to repeat these experiments with less susceptible animals.

In view of the fact that rats invariably recover, some soon, others late, from infection with *T. lewisi* and the further fact that rich cultures of this organism are readily obtainable, it is evident that this species is well adapted for studies on immunity. Up to the present time it has not been satisfactorily shown that trypanosomes elaborate toxins or that they confer immunity by means of soluble or intracellular products. The latter problem was approached by means of plasmolyzed cultures. To effect solution of the trypanosomal cells the cultures were taken up in distilled water and dialyzed in collodium sacs. Usually after one or two hours of such dialysis in distilled water the trypanosomes completely disappear and the intracellular matter apparently passes into solution.

By means of such plasmolyzed cultures it has been shown that rats which receive three or more injections, on alternate days, on subsequent inoculation with a minimal infective dose of fresh trypanosomal blood from a rat do not become infected, whereas controls are positive. With such solutions it is possible to hyperimmunize rats so that 0.5 c.c. of the immune rat blood protects against a simultaneous and separate injection of the infective blood.

Protection is seemingly obtained against *T. lewisi* by simultaneous and separate injection of the infective blood and plasmolyzed culture, followed twenty-four hours later by a second injection of the latter. Repeated injections of too large a quantity of the plasmolyzed culture and at too short an interval leads to a negative phase, the presence of which is indicated by the unusually early appearance of trypanosomes in the blood after inoculation with the virus.

Inasmuch as it may be said that the plasmolyzed material does not represent a true solution, a series of experiments were made with the filtered (Berkefeld) plasmolyzed liquid. While these experiments go to show that immunity can probably be induced by such filtered soluble products they are not as decisive as they should be and for that reason will have to be repeated. The chief reason for this uncertain result is the rather frequent failure of the control rats to develop infection. Although young rats (50–80 grams) were used to guard against previous infection with trypanosomes it is certain that a large percentage of the rats, as purchased on the market, have acquired an immunity against *T. lewisi*. That the immunity encountered is really acquired and not natural is shown by the fact that we have many times isolated *T. lewisi*, by means of the cultivation method, from rats which on repeated examination were found to be free from parasites and hence were supposed to be normal.

Program of the Second Session, December 28, 1906

(Joint meeting of Section K and the Society of American Bacteriologists.)

On the Biology of Diplococcus intracellularis: Simon Flexner.

The Stability of Tetanus Toxin: M. J. Rosenau.

Some Observations on the Blood of Horses: J. J. Kinyoun.

The Alleged Rôle of Intestinal Worms as Inoculating Agents in Typhoid Fever: C. W. Stiles.

The Absorption of the Third Serum Component: W. H. Manwaring.

The So-called Physical Chemistry of Hemolytic Serum: W. H. Manwaring.

On the Chemical Inactivation and Regeneration of Complement: Hideyo Noguchi.

On the Electric Charge carried by Toxins, Antitoxins and Agglutinins: C. W. Field.

An Improved Technic for Tuberculo-opsonic Preparations: A. P. Ohlmacher.

Some Suggestions concerning the Terminology of Opsonic Theory and Practise: A. P. Ohlmacher.

The Generic Characters of the Coccaceæ: C. E. A. Winslow and Miss A. F. Rogers.

Actinomyces of the Oral Cavity: D. H. Bergey.

Program of the Third Session, December 29, 1906

(Joint meeting of Section K and the American Physiological Society.)

The Functions of the Ear of the Dancing Mouse: R. M. Yerkes.

The Effect of Section of One Vagus upon the Secondary Peristalsis of the Esophagus: S. J. Meltzer and John Auer.

On the Alleged Adaptation of the Salivary Glands to Diet: F. P. Underhill and L. B. Mendel.

Adaptation of Saliva to Diet: C. H. Neilson.

The Effect of Phosphorus Starvation on Aspergillus niger: Waldemar Koch and H. S. Reed.

New Chemical Facts about Tendon and Compound Proteins: William J. Gies.

A Further Study of Peptolysis: W. N. Berg and William J. Gies.

The Action of Blood Serum and Tissue Extracts on the Coagulation of the Blood: Leo Loeb.

Some Observations on the Esophagus after Bilateral Vagotomy: W. B. Cannon.

Concerning the Pharmacological Action of Salicylic Acid: L. B. Stookey and M. Morris.

A Nuclein Metabolism Experiment on a Dog with an Eck Fistula: P. A. Levene and J. E. Sweet.

Protein Analysis: P. A. Levene, W. A. Beatty, D. R. MacLaurin and C. H. Ruiller.

A Demonstration of Normal Gastric Peristalsis in the Rabbit: John Auer.

Peristalsis of the Rabbit's Cecum (with demonstration): John Auer and S. J. Meltzer.

Preservation of Blood Vessels in Cold Storage: Alexis Carrel.

Demonstration of Failure of Regeneration of the Cervical Ganglion Twenty-six Months after its Removal: S. J. Meltzer.

William J. Gies,
Secretary

SCIENTIFIC BOOKS

THE TERRACES OF THE MARYLAND COASTAL PLAIN

Maryland Geological Survey. Pleiocene and Pleistocene. Baltimore, 1906. Pp. 291, plates and maps.

The high standard of investigation and publication reached in the previous reports of the Maryland Geological Survey under the direction of Professor W. B. Clark is maintained in the recent volume on the 'Pleiocene and Pleistocene of the Coastal Plain'; the later physical history of the district being treated by G. B. Shattuck, recently of Johns Hopkins University, now professor of geology at Vassar College, while its paleontology is discussed by Clark, Lucas, Hollick and other experts. The chief physical results of this elaborate study, already outlined by Shattuck six years ago ('The Pleistocene Problem of the North Atlantic Coastal Plain,' Johns Hopkins University Circulars, No. 152, 1901), concern the marine terraces that have been carved on the slopes of the previously dissected coastal plain, and are presented in the first half of the report under such headings as general stratigraphic relations, physiography of each terrace, structure of each terrace, method of interpretation, resulting inferences as to geological history, and summary. The chapters on paleontology include a general account of the Pleistocene fauna by Clark, and flora by Hollick; then a discussion of systematic paleontology under ten headings from mammalia to spermatophyta by various authors, and a special chapter on the elephants of the Pleistocene by Lucas. So detailed a treatment of Pleistocene paleontology is novel in American geological reports.

The geological history of the region to which this review is directed, may be briefly summarized as follows: The series of marine strata, chiefly Cretaceous and Tertiary, which make up the body of the Maryland portion of the Atlantic coastal plain, was uplifted and perhaps nearly baseleveled by subaerial erosion, in time to receive, when again submerged, a broad sheet of Lafayette (Pleiocene) gravels and sands. Another uplift again exposed the coastal plain, thus renovated, to prolonged erosion under the leadership of the Potomac, Susquehanna (Chesapeake), and other large rivers, the main valleys thus gaining an expression of advanced maturity, with depth and breadth perhaps not far short of the dimensions which they now possess. Next came a strong depression; the mature valleys were drowned and a well-defined shore-cliff or scarp was cut on their slopes (the stage of development reached by the shore line being inferred by the reviewer to have been advanced youth or early maturity). Sunderland is the name given to this scarp and to the associated marine terrace deposits then formed. A series of short-lived partial uplifts and slight depressions followed; each uplift permitting a renewal of dissection on the emerged slopes; each depression sufficing to produce a new scarp with a submarine terrace. Three sets of scarps and terraces below the Sunderland were thus formed; the Wicómico, the Talbot and the Recent; all, like the Sunderland, contouring around the frayed-out slopes of the dissected coastal plain, and now standing at altitudes of (about) 200, 100, 40 and zero feet.

This interpretation has two chief bases. First, a critical study of the features along the present shore line; second, a comparison of these features with similar features at higher levels. The work has every appearance of being carefully done. It will be interesting to note how far the conclusions are supported by new studies, either of a more detailed character in Maryland, or of a broader character in neighboring states.

An understanding of the development of the present shore line is evidently of fundamental importance. It is shown to have followed a moderate submergence of the previously dissected coastal plain. The very irregular initial shore line thus produced has been systematically modified by the development of long, off-shore sand reefs on the most exposed ocean front, while scarps have been cut on the headlands of the less exposed bays, and short sand reefs have frequently been thrown across the smaller reentrants within which deltas and marshes have accumulated. At the same time a submarine terrace of gravels, sands and clays is believed to have been formed for a certain distance off shore. At various points where the headland scarps are cut back, the neighboring sand reefs have been forced to retreat at about the same rate; and thus the inner border of the stratified terrace deposits, containing marine fossils, is believed to be locally superposed upon the fresh-water or brackish-water sands and clays of the delta and marsh reentrants. The base-line of the scarps, the surface of the delta plains, and the inner border of the marine terrace are all closely at the same level. The scarps rise sharply above their base line, and gain a height dependent on the altitude of the headland in which they were cut; the terraces slope very gently waterward, their layers resting unconformably on the submerged and eroded land-surface, and their composition and thickness presumably varying with local changes of depth, waves and currents.

It is the occurrence, at various altitudes above sea-level, of features similar to those of the present shore line, but now more or less wasted by erosion, that has led Shattuck to his ingenious interpretation of the Sunderland, Wicómico and Talbot terraces. He traversed the coastal plain in all directions, but not until it occurred to him to explain its details of form and structure by means of marine action at several levels was it possible to bring order out of confusion. The scarps, especially the higher earlier ones, are dulled by weathering; and the terrace fronts are dissected by retrogressive streams, especially where undercut by the next lower scarp. Nevertheless they are still traceable. It is significant that the correlation of the different parts of each terrace is usually not based on paleontological evidence, for most of the deposits do not bear fossils; nor on diversity of composition, for the materials of the successive terraces are all much alike in their variable nature; but chiefly on the continuity and similarity of surface form and deposits, when traced horizontally in various districts; and on the systematic sequence of forms and deposits, when followed down from higher to lower levels. The plane, marked by the inner border of each terrace, must have been horizontal when formed, and is now faintly inclined to the southeast, as a result of slight inequality in later elevatory movements. The Talbot terrace, next preceding the present, has still a practically level inner border; the Sunderland terrace, having felt all the later changes of level, summarizes their inequality in a seaward slope of about three feet to a mile. Much more distinct than these extremely faint slopes is the manifest though still gentle descent of each terrace as it departs from its inner border at the scarp base towards the larger valleys with which it is related.

The maps that are bound in with the report give it excellent illustration. First is a general map of eastern Maryland (1,500,000), showing all the formations here described; and from this it appears clearly that, if the Lafayette and Sunderland ever existed (as they probably did) on the eastern peninsula of Maryland and Delaware—the 'Eastern Shore'—they were removed by successive attacks of subaerial and marine erosion, thus allowing the Wicómico terrace to spead over the axial upland of the peninsula as a broad plain, as yet but little dissected over its medial area. It is only on the group of dissected peninsulas between the Chesapeake and Potomac that the whole series of formations can be found, descending step-like from Lafayette on the uplands to Recent at the shore line. The inferred relations of land and water at successive epochs are even more clearly shown on a series of four smaller maps (20 miles to an inch), illustrating the supposed areas of submergence in Lafayette and later times. A final map indicates the inferred emergence of the land and the extended rivers of the Talbot-Recent interval of uplift. Numerous plates give views of the terrace deposits and of the scarps, recent and abandoned.

A brief review of certain points, on which this excellent report is not altogether clear at first reading, may now be presented.

1. As to the stage of erosion of the Cretaceous-Tertiary coastal plain, that had been reached when depression occurred and the deposition of the Lafayette commenced. It is said that "there was a long interval of erosion before deposition of the Lafayette beds began" (p. 78), and that "the Lafayette was developed as a plain surface sloping gently toward the surrounding waters" (p. 123); but the stage of erosion reached in pre-Lafayette time is not explicitly stated in physiographic

terms. However, the smoothness of the Lafayette plain, where it is still preserved, taken with the moderate thickness of its deposits (50 or 100 feet), would indicate that great advance had been made toward base-leveling by pre-Lafayette subaerial erosion; and that whatever residual relief then survived on the Cretaceous-Tertiary coastal plain area was effectually abraded by the advancing Lafayette sea. This conclusion evidently postulates the marine origin of the Lafayette, an origin generally accepted and not here brought into question.

2. The stage of erosion reached in the Lafayette-Sunderland interval of uplift. A comparison of statements in different chapters of the report and an examination of the relation between the several terrace scarps and the main valleys lead the reviewer to infer that the uplift of the renovated, or Lafayette-covered coastal plain was sufficient in amount and duration to allow its erosion by normal, river-led processes in the Lafayette-Sunderland interval to a stage of advanced maturity. The valleys thus formed seem to have compared well in width and depth, as stated above, with those of the Potomac and Chesapeake of the present day. The text of the report is, however, not immediately clear on this important point; and this is the more to be regretted, because a definite physiographic picture of the district at the beginning of the strong depression which culminated in the erosion of the Sunderland scarp is an essential basis for a clear understanding of the development of the several terraces. The difficulty here seems to be that the problem of the Lafayette-Sunderland interval is not completely stated on an early page, and then referred to, when occasion again arises, in the terms first adopted for its description; but that it is stated partially in different chapters, in various connections, and in diverse terminology, through all of which the reader must pursue his inquiry before he can acquire the writer's point of view. For example, after reading (as quoted above) that "there was a long interval of erosion before deposition of the Lafayette beds began," the following lines state that 'another long period of erosion occurred' before the Sunderland terrace was formed (p. 78). This would naturally give the impression that the Lafayette-Sunderland erosion interval was of great importance, for the pre-Lafayette erosion went far towards base-leveling the coastal plain of earlier uplift. On the other hand, in the final summary, the 'elevation and erosion' between the Lafayette and Sunderland formations is stated in precisely the same terms as the 'elevation and erosion' between each pair of the succeeding formations (p. 137); and inasmuch as the later erosion intervals are well proved to have been of moderate duration (as will be shown below), this would suggest that the Lafayette-Sunderland interval was also of no great length. The reader who glances, as some readers may, over the closing summary of the report before reading the details of the preceding pages, will naturally gain from the summary an impression that all the post-Lafayette erosion intervals were of about the same length. But this impression would have to be changed on further reading. For example, it is stated on an earlier page: "The salient features of the Coastal Plain topography were outlined at this time [Lafayette-Sunderland interval], although it is doubtful if they received their full strength or final touches before the post-Talbot uplift" (p. 123). This would indicate that the interval under discussion was long enough for the attainment of a mature stage of erosion; yet the next earlier sentence states: "After the deposition of the Lafayette formation, the land was raised above ocean-level and subjected to an interval of erosion which was probably of longer duration than the later ones which separated the other surficial deposits [terraces] of the series" (p. 123). Here the phrase, 'probably of longer duration' destroys all the emphasis that might be given to the duration of the Lafayette-Sunderland interval by the previous citation. Again, the statement that the Sunderland formation "extends up into ancient valleys which penetrate it [the Lafayette] as reentrants" (p. 87), coupled with the fact that these reentrants are shown by the general map to stand on the opposite sides of larger valleys,

five or ten miles wide, gives good ground for the inference that advanced maturity of erosion had been reached before Sunderland deposition began; and this inference is confirmed on reading a little later: "It is evident that the valleys of the Potomac, Patuxent and other large rivers as well as that of Chesapeake Bay existed [before Sunderland deposition], since the Sunderland formation, which was deposited when this topography was submerged, slopes toward all these depressions" (p. 123). Yet the next sentence baffles the reader by referring to the pre-Sunderland valleys as 'gorges,' a term which suggests something very different from valleys several miles wide and only a few hundred feet deep, with gentle lateral slopes of semi-consolidated formations, and which implies a relatively brief erosion interval. The reader's mind would surely be more easily made up regarding the writer's idea of the sequence of events, if the important Lafayette-Sunderland erosion interval had been once for all effectively described in technical language in an early chapter, and afterwards referred to when necessary in essentially the same language as that first employed.

The depth of the valleys eroded in the Lafayette-Sunderland interval is believed by Shattuck to have been less than that of the present valleys; but the evidence presented in favor of this conclusion does not seem to be positive. That valleys were present when the Sunderland terrace deposits were formed is shown by the fact that the deposits slope "from the watersheds of the peninsulas of southern Maryland toward Chesapeake Bay on the one hand and the estuaries of the Potomac and Patuxent rivers on the other" (p. 115). But it is concluded that no valley was cut so deep as it is now, because the Sunderland formation "nowhere shows a tendency to develop a thickness sufficient to fill such a valley" (p. 123; see also p. 135). The same is stated to be true of the other terraces (p. 135). It is not altogether safe for one who has but a slight acquaintance with the district under discussion to oppose this conclusion; yet the argument by which it is supported does not appear to be fully convincing. The comparatively brief duration of marine action at the several terrace levels would seem to be compatible with the deposition of most of the marine terrace sediments not far from the shore line, particularly in view of the belief that the scarps were presumably cut and the terraces built for the most part during a time of continued depression. Moreover, if the main valleys had been significantly deepened during the later inter-terrace intervals by normal subaerial agencies under the leadership of the larger rivers, the valleys must have also been greatly widened during the same intervals; and the time required for such widening —a slow process—seems inconsistent with the fairly good preservation of the first-made (Sunderland) scarp. This leads to a consideration of a third topic.

3. The duration of the interterrace intervals. The fact that the weathered Sunderland scarp is now recognizable at all requires that the entire duration of post-Sunderland time can not have been a large part of a physiographic cycle. Such a scarp, a minor feature at the best, would be entirely destroyed by general subaerial erosion in the passage of a coastal plain from the stage of late youth to that of early maturity, or from early maturity to late maturity. Hence only a short duration can be allowed to each of the three inter-terrace intervals of uplift and erosion, as well as to each of the three intervals of scarp-cutting and terrace-depositing. The author recognizes that these intervals are comparatively short, but he does not give sufficient emphasis to the contrast between their shortness and the much greater length of the Lafayette-Sunderland erosion interval. The work done in cutting scarps and depositing terraces is, however, very clearly presented; naturally so, as this is the chief theme of the report. The manner in which the later terrace deposits extend into the reentrants eroded in the margins of the earlier terrace deposits gives good proof that the post-Sunderland movements were oscillations; that is, repeated partial uplifts separated by slight depressions; and not merely pauses in a persistent uplift. The way in which the marine sands have come to overlie the fresh- or brackish-water bay-head clays is

admirably analyzed and illustrated. It is true that by no means all of the deposits which are treated as of marine origin are known to contain marine fossils; yet the localities where marine fossils have been found in them are held to be of demonstrating value for many others. With all these points in mind, one may picture very clearly the rapid succession of short-lived events during the Sunderland depression and the later oscillations. The advancing Sunderland sea presumably shaved off a film from the sinking surface; and the thickness of the film increased to a measure of fifty or more feet when the greatest encroachment of sea on land was made during the pause which closed the depression. In the following partial emergence, the sea presumably withdrew without doing recognizable erosive work —much in the way that the tool of a planing machine backs harmlessly across an iron plate, preparatory to taking off a shaving in its next advance. But the extended streams and rivers took advantage of the withdrawal of the sea and eroded new valleys in the margins of the revealed terraces. The advances of the sea in Wicómico and Talbot times repeated the operations of the Sunderland advance; and the recent advance, now in operation, gives the key to its predecessors.

The paleontological pages of the report show that the terraced coastal plain was a forested region during the time of its oscillations, with many trees like those of to-day; and that mammoths and elephants were among its inhabitants. Nearly all the marine forms are found living along the present coast.

Although this report is elaborated far beyond any previous descriptions of the Maryland coastal plain, there are certain features to which detailed studies may still be profitably directed in the future. There is, of course, a continual watch to be kept for new localities where sections of the various terraces may be exposed, and particularly where fossils may be found. All such discoveries will have high value in testing the correctness of the conclusions now set forth, inasmuch as the capacity of a theory reasonably to account for facts of later discovery is one of its best recommendations. Moreover, there are certain physiographic details to be filled in, which will give increased verisimilitude to the pictures of the Maryland Pleistocene already drawn. These details have regard to the contrasts that may be expected between the shore lines of the exposed ocean front and of the protected bay borders. For example: in Sunderland time, the peninsula of the Eastern Shore was completely submerged; the western border of what is now Chesapeake bay must then have been beaten with breakers from the ocean swell—except in so far as the swell was expended in passing over the shoals of the submerged Eastern Shore. The Sunderland scarp thus formed should have a different outline from the Wicomico, Talbot and Recent scarps, lower down the west Chesapeake slope; for when the latter scarps were formed, Chesapeake bay was enclosed from the ocean by the Eastern Shore peninsula, and its limited waves could not have trimmed a shore line of the same expression as that made by the ocean breakers. Similarly, the Talbot scarp cut in the Wicomico terrace on the eastern side of the Eastern Shore must have been cut by ocean breakers similar in force to those which have in Recent time formed the long, smooth-curved, off-shore sand reef by which Chincoteague and other 'bays' are enclosed along the present ocean front. Hence a similar off-shore sand reef might have been formed in a similar situation during the Talbot epoch. As no such Talbot sand reef is described, the reader is left somewhat in doubt as to whether it is really absent or whether exploration on the farther (eastern) slope of the Eastern Shore was not carried out in sufficient detail to determine it. Again, it is noted that the scarp and terrace of one epoch are occasionally undercut and entirely destroyed by the undercutting of the scarp of the next succeeding epoch; just as might be expected if the general slope of the attacked surface had been somewhat steeper in the later than in the earlier epoch. It will be interesting to learn if independent evidence is eventually found to show that such was the case. Finally, there are certain local features of special significance found at a few points on the present shore line, such as the cuspate forelands seen at Cedar and Cove points on the lower Chesapeake; it will be edifying to learn if similar details are revealed by further study of the earlier shore lines. All these details concerning the shore line features might, it may be noted, have been stated to advantage in a technical terminology, as expressive of the precise quality of the facts as is the technical terminology that is employed without hesitation in the chapters on paleontology; but in the present volume the best technical terminology for the descriptions of shore lines—that suggested by Gulliver—is unfortunately seldom employed.

There is one reflection that Shattuck's close study of a coastal plain suggests to one who is particularly interested in that class of forms as objects of physiographic study; namely, the difficulty of telling the whole truth in a brief statement. It is customary with some physiographers to describe the coastal plain of the Maryland region as having been formerly maturely dissected and recently partly submerged; an effective general picture of its present form and outline is thus suggested in a few words. But the details of its form require a more elaborate statement for their proper presentation. It is a coastal plain which, having been uplifted and effectually baseleveled, was renovated with a wide-spread cover of Lafayette sediments, and then uplifted again, as good as new: in this condition it was dissected to well advanced maturity; then strongly submerged preparatory to an oscillating emergence, which allowed the rapid carving of four scarps and the deposition of four terraces (the last being those of to-day) on its maturely dissected slopes. The first brief statement will serve for use in schools; the second more elaborate account, with appropriate local details, may suffice for use in colleges. The features yet to be more fully investigated offer fitting subjects for field research by graduate students and professional workers.

W. M. Davis

SOCIETIES AND ACADEMIES

THE TEXAS ACADEMY OF SCIENCE

The first meeting of the Texas Academy of Science for the year 1906–'07 was held in the chemical lecture room of the University of Texas on Friday evening, October 26, 1906. The program consisted of the inaugural address of the president, Dr. S. E. Mezes, professor of philosophy in the university and dean of the college of arts, who took for his subject 'What is Matter?'

At the regular meeting, November 24, 1906, Dr. George S. Fraps, of the Agricultural and Mechanical College of Texas, state chemist, discussed 'Food Adulteration' after which the speaker was entertained by the officers of the academy at an informal banquet.

The semi-annual formal meeting was held Tuesday evening, December 26, 1906. Dr. H. Y. Benedict, professor of applied mathematics and astronomy, in the University of Texas, delivered an illustrated lecture on 'The Solar System.'

At the regular meeting of January 26, 1907, Captain T. J. Dickson, chaplain of the 26th Infantry, U. S. A., Fort Sam Houston, San Antonio, by request, presented two illustrated papers: (1) 'Fighting Asiatic Cholera,' (2) 'The First Ascent of Mount Isarog'; both papers dealing with his personal experiences in the Philippine Islands.

The meeting for February–March was held March 7, 1907. Dr. Eugene P. Schoch, adjunct professor of chemistry, University of Texas, gave an experimental exhibition of 'The Transformation of Radium.'

Volume VIII. of the *Transactions of the Academy* has been recently published. Its contents include papers on the following subjects:

'The Æsthetic Element in Scientific Thought,' by Dr. Thomas Montgomery, Jr. This is the presidential address for 1905.

'Paving Brick,' by Thomas U. Taylor, dean of the department of engineering in the University of Texas.

'The Spacial Conception of the Blind,' by Dr. Franz J. Dohmen, honorary lecturer in mathematics in the University of Texas.

'Urogenital Organs of North American Lizards,' by Barney Brooks, professor of chemistry in Coronal Institute.

'The Indebtedness of the German Language to the Latin,' by Dr. Sylvester Primer, professor of Germanic languages in the University of Texas.

The volume concludes with the proceedings of the academy for 1905.

FREDERIC W. SIMONDS,
Secretary

UNIVERSITY OF TEXAS,
April 2, 1907

THE CHEMICAL SOCIETY OF WASHINGTON

THE 173d regular meeting of the Washington Section of the American Chemical Society was held at the Cosmos Club, March 14, 1907.

The question of a sanitary committee was settled by authorizing the president to instruct the committee on communications (O. Schreiner, chairman) "to make arrangements for special meetings of the society, or joint meetings with other organizations where lectures and reports may be presented on general or special phases of sanitation, in order that the society may be kept informed on such matters, and suitable action taken."

W. L. Dubois read a paper on 'Lactose and Butter Fat in Milk Chocolate' in which he showed that lactose could be accurately estimated by polarizing the solution of sugar at 86° after inversion, and butter-fat could be approximately determined by the Reichert-Meissl number of the extracted fat. Dr. A. Seidell presented a paper on the 'Determination of Acetanilid in Headache Powders.' The method suggested was based upon the reaction of bromine with anilin to form anilin tribromide. The sample containing acetanilid is dissolved in dilute hydrochloric acid and the solution boiled. Anilin hydrochloride thus formed is titrated directly with a standard solution of potassium bromate. The free bromine colors the solution yellow at the end of the reaction. W. T. Schaller presented a paper on 'The Chemical Composition of Molybdic Ocher' in which it was shown that the natural molybdic ocher is a hydrous ferric molybdate Fe_2O_3, $3MoO_3$, $7\frac{1}{2}H_2O$, and that the existence of molybdenum trioxide MoO_3 has not been demonstrated.

On April 1, Professor A. Frank, Jr., of Germany, gave an address before the society at the lecture hall of the George Washington University on 'The Utilization of Atmospheric Nitrogen in the Production of Calcium Cyanamid.' The speaker described the gradual steps which had been taken by various investigators during the past century to bring about the fixation of atmospheric nitrogen, and told of the successful accomplishment of this problem by his father working in collaboration with Dr. Caro. The successful issue of this task was made possible only after Professor Moissan had shown the practicability of manufacturing calcium carbide commercially, and after the introduction of the dynamo and electric oven by Siemens and Halske. The process consists essentially in passing a current of air freed from oxygen through calcium carbide heated in an electric oven, calcium cyanamid or lime-nitrogen being thus produced. The speaker also touched upon the fertilizing experiments carried on in Europe to show the value of cyanamid as a fertilizer. Other products are also produced, some being used in the manufacture of gun powder. Specimens of the various products were exhibited.

J. A. LE CLERC,
Secretary

BUREAU OF CHEMISTRY,
WASHINGTON, D. C.

DISCUSSION AND CORRESPONDENCE

THE FIRST SPECIES RULE AS IT AFFECTS GENERA OF NORTH AMERICAN BIRDS

IN my consideration of the application of the 'first species rule' in fixing the types of the genera of North American birds[1] I stated that fourteen changes in the genera of the A.O.U. Check List would result, while the types of several genera would be shifted to congeneric species. Dr. J. A. Allen has stated[2] that my statistics 'greatly underestimate the number of changes' and re-

[1] SCIENCE, XXIV., p. 262, November 2, 1906.

[2] SCIENCE, XXIV., p. 778, December 14, 1906.

cently[3] he has presented a list of twenty-one changes that he claims would be necessary.

It is well known to all that the first species and elimination methods are only used as methods of last resort; and cases where the author in the original publication has indicated a type do not come under the operation of either method. Four of Dr. Allen's alleged changes may thus be cancelled at once.

Spinus Koch, 1816, type *Fringilla spinus* L. by tautonomy.

Zonotrichia Swainson, 1831, types '*leucophrys, pennsylvanica, melodia*' designated by the author. We are bound to pick our type from these three 'typical' species and by either method *leucophrys* is the type and no change is required.

Poœcetes depends upon the last and falls with it.

Cyanurus Swains., 1831, is similarly restricted by the author to tropical species.

Colymbus Linn., 1758, does not come under the first species rule.

Erionetta Coues, 1884, is a monotypic genus and how it can become nameless by the operation of the first species rule I can not conceive. What does happen is that it is replaced by *Somateria,* the type of the latter being *Anas borealis* not by first species rule but because it is the only species definitely quoted.

One other case, *Aix,* I included among those in which the type shifted to a congeneric species, the A.O.U. committee having voted not to divide the genus *Aix.*

Removing these seven cases from Dr. Allen's list we have left fourteen, exactly the number I gave.

In my paper I claimed that fifteen changes would result from consistent elimination. Dr. Allen claims but three changes. I can not of course comment upon his results until the details of his eliminations are published.

In the eliminations of vulturine genera that he has published I called attention to several inconsistencies. One of these he now admits and changes the type of *Sarcorhamphus* from *auricularis* to *gryphus.* In his republication of the *Vultur* case, however, he makes another slip, forgetting that since *gryphus* is now the type of *Sarcorhamphus,* 1806, it must be removed from *Vultur* at that date, leaving *harpyia* or *papa* as the type of *Vultur,* the choice being a nice question of priority.

Sarcorhamphus thus replaces *Vultur* of Dr. Allen's scheme and *Vultur* will replace either *Harpyia* or *Gypagus.*

There seems to be only one alternative, *i. e.,* to remove *gryphus* at the date it became the type of *Sarcorhamphus.* If we do this, however, we must do the same with the other genera: *Aura* will go out at 1816, and *papa* at 1854, the latter being thus the type of *Vultur.*

This is an excellent illustration of the complexity of the elimination method and the opportunities it offers even to experts to fall into errors.

Dr. Allen's comments upon the points of my recent paper do not cause me to alter my statements and further discussion along those lines seems useless. The consideration of Linnæan names and priority of Swainson's papers are quite apart from the main issue.

WITMER STONE

ACADEMY NATURAL SCIENCES,
PHILADELPHIA, April 6

[3] SCIENCE, XXV., p. 552, April 5, 1907.

LACK OF RECUPERATIVE POWER OF ITALIAN WORKMEN

TO THE EDITOR OF SCIENCE: On reading Dr. Meltzer's most interesting paper in SCIENCE for March 29 I was reminded of a remark made to me some four or five years ago in regard to the lack of power of Italian workmen, in Italy, to recover from injuries. My informant had for many years been in charge, as foreman, of large numbers of Italian machinists and laborers (and of some English ones as well) in the Armstrong gun carriage and repair shops at Pozzuoli. In reviewing his impressions of the operatives he said that they were able to do fair days' work but that they were likely to be long ill or even to die from injuries that would not prove serious to an English workman. This he attributed wholly to the less substantial diet of the Italian. It is a noteworthy fact in this connection that the poorest Neapolitans set the subsistence limit at six *soldi, i. e.,* six cents, per day. Meat is rarely eaten by any but well-to-do Neapolitans and the main sources of proteids are bread, spaghetti and beans (*Phaseolus*). It seems to me that in the lack of recuperative power shown by the Neapolitan workmen there is an excellent example of the danger of minimizing the reserve fund of proteids in the system, as suggested by Dr. Meltzer.

JOSEPH Y. BERGEN

CAMBRIDGE, MASS.,
April 12, 1907

SPECIAL ARTICLES

GEOLOGY OF THE SIERRA ALMOLOYA, WITH NOTES ON THE TECTONIC HISTORY OF THE MEXICAN PLATEAU[1]

THE Sierra Almoloya is situated in the southern part of the state of Chihuahua, about 25 miles west of Jimenez, and midway between the latter town and Parral.

This sierra is one of the numerous isolated mountain blocks of northern Mexico, like Santa Rosalia, Naica, Santa Eulalia and others, which rise in solitude from the vast area of surrounding arid plains constituting the great Chihuahua province, between eastern and western sierras, of the Mexican Plateau portion of the northern cordilleran region.

The mountain is a long and narrow range, about ten miles in length, extending in a northeast-southwest direction and averaging less than two miles in width. It is surrounded on every side by a lower area of sloping plain which has an altitude of about 5,000 feet at the mountain base. The total altitude of the mountain above the plain is fifteen hundred feet, the peaks rising to an altitude of 6,500 feet above the sea, as far as could be determined by a careful aneroid study. To the south of the range are several conspicuous outliers of lower altitude as shown upon the map.

The range is dominated by a narrow axial summit ridge, following the northeast-southwest trend of the mountains. Numerous narrow tongue-like salients radiate from the ridge to the plain, and separate by deep arroyos cutting back to the ridge ('capturing' it) and their valleys making great cirques, or amphitheaters between the salients.

Arising from the central ridge are several conspicuous summit peaks. The highest of these, attains an altitude of about 6,500 feet.

Like the other limestone mountains of the Chihuahua province, this sierra reveals the ancient wrinkled and folded structure of the plateau prior to when it was buried in vast beds of rhyolitic and andesitic volcanic ejecta which once covered this whole portion of Mexico, and which is still preserved in the western Sierra Madre, and like the other ranges mentioned the mountain represents the resistance and survival of the hardest, in the destructive atmospheric erosion and degradation of a once higher surface of the great western plateau.

The Sierra Almoloya is, therefore, a destructional or decadent form of a mountain, representing a remnant of the former extent of the rock material upward and laterally which fact is not only testified in the degraded shape of the mountain configuration itself, but by the vast quantities of talus and debris in process of forming on its surface and now filling the surrounding deserts.

Every detail of its relief such as its axial direction, the character of its slopes, the course of its lateral arroyos and other features are conformed to the arrangement or structure of the rock material composing the mountain, such as the lines of stratification, faults and folds, etc., to be later described.

The exposed rocks composing the sierra consist almost entirely of limestones of varying degrees of purity constituting the main mass and country rock of the mountain. Secondarily these are mineral ores and exceptional fragments of igneous rocks, the latter not found in place of origin.

The Limestones.—The mountain mass is composed of stratified limestones of the Comanche series of Lower Cretaceous age largely and mostly of the particular formation known as the Edwards limestone.

These limestones originated as sea muds in the form of chalk and chalk marls, accompanied by horizons of siliceous flint nodules,

[1] Read before the Geological Section of the New York Academy of Sciences, April 1, 1907.

when the Gulf of Mexico occupied this area. They now occur as gray, blue and black strata intensely hard, greatly altered, and in places converted into white crystalline marble and in others into a semi-schist. The individual beds of limestone vary in proportions of lime, silica and clay, and also slightly in color and texture.

These limestones have been greatly tilted and deformed in the various processes of mountain and continent building, so that instead of lying horizontal as they were originally deposited, they are tilted at every conceivable angle. They have also been folded into close folds. These folds are the most complicated I have ever seen in the rocks of this period, constituting what geologically may be termed thrusted overthrows, recumbent anticlines, etc., of the alpine type of structure.

When viewed from the plain from any direction, the mountain appears an elongated dome with the strata dipping conformable with the slopes in every direction from the central summit axis, so that as a whole it is quaquaversal in its character.

The western slopes of the mountain apparently consist of uniform beds of strata dipping towards the plain. On ascending and carefully studying the salients, it was found that the strata were folded directly back of one another in close compact recumbent folds, and that the dip of the folds corresponds with the ascent of the slope. Upon reaching the shoulder below the summit of Picacho the worn-off edges of the outer layers of the overthrown anticline project upward in great vertical strata.

The higher peaks and narrow connecting summit ridges with the exception of Mount Anticline, at the north end of the ranges, are all composed of the truncated vertical strata of the overturn of the recumbent anticlines. Not only are the rock sheets closely folded, but the folded edges are buckled, or bent, so that the buckling of the folds corresponds in its sinuous course to the axis of the mountain. The general strike of the old folds is north 30 east, north 50 east and north and south.

Had it not been for my previous familiarity with every detail of these Cretaceous rocks, as elsewhere studied, I doubt if the true structure would as yet have been ascertained. In fact, it was not until after many days upon the ground that I discovered, by the aid of the layers of flint nodules embedded in the limestone, how to trace the stratification beds in these greatly disturbed and altered strata.

Alteration.—In places near the acute flexures, the limestones are completely metamorphosed into white crystalline marble, or converted into schists—a fact which I have previously noticed in Mexico at Guaynopita, west of Chilpancingo and other localities. This phenomenon is so frequent as to lead to the inquiry if much of the pre-Cretaceous plexus of Mexico has not lost its integrity through the intense metamorphism of the post-Cretaceous mountain movements.

These limestones are also excessively fractured by several systems of joints, faults, etc. The bedding planes, which, when the strata are vertical, are also exceedingly difficult for the layman or even the geologist to distinguish, from the faults and joints.

In this mountain two distinct systems of faulting are discernible. The first system may be known as the north-south and northeast system, the other as the northwest fault system. The north 80° west faults are related to the northwest system in age.

The faulting and folding in north-south and northeast directions is an older structural feature than the other faults mentioned, and is apparently barren of mineralization. Faulting of this kind may be seen on the northeast side of the Picacho and the east side of Placeras and the west side of America.

Many of the north 40° west faults occur in parallel belts across the mountain range and nearly all the mineral outcrops seem to be closely associated with them.

The movements along the northwest faults has been lateral, displacing the older folds.

No igneous rocks outcrop on the Almoloya Mountains, or anywhere within a distance of ten or twelve miles, so far as I could ascertain. Fragments of rolled rhyolite occur in the talus along the west base of the mountain, and pieces of basalt are found in the Julietta

Valley. It is most probable, however, that the mountain is underlain at depth by igneous intrusions.

At the close of the Cretaceous period, at the time of the great Laranude or Rocky Mountain uplift, the sea bottom was uplifted into land, as a part of the Mexican Plateau province, and the rocks were wrinkled and compressed into the marvelous overthrown folds which they now present, having north-east and north-south trends.

This period of folding was that of the whole of the eastern Rocky Mountain region in north-south directions, and the union of the North and South American continents.

During the later epoch, probably the Miocene, the region and its limestone rocks underwent another orogenic movement, and was intruded from below by a vast igneous magma from which came dikes and ejecta of volcanic rocks which are now found throughout the region. Accompanying this epoch the whole structural or tectonic trend of the southern continent changed from a north-south to north 40° west course, and the north 40° west faults and folds of this later orogenic revolution were developed across the older north-south folds and faults, resulting in the development of the quaquaversal structure of northern Mexico, and I have seen it at La Mitra, Villadama and Candela, in the State of Coahuila, the Big and Little San Vincente, mountains crossed by the Rio Grande at Presidio San Vincente; at Guaynopita, Jesus Maria and Santa Rosalia, Mexico; and at Monument Mountain near El Paso, and in the Santragos Chinati and other mountains of Trans-Pews, Texas. The great mineral lodes of Mexico were mostly intruded, as hot vapors, waters and gases up these northwesterly fault zones.

Since or during this second epoch, the plateau as a whole has been uplifted, increasing the erosion, and the surface has worn down at least 2,000 feet. Some of the mineral contents have been washed away; others have concentrated down the fractures enriching the present ore bodies.

As the writer has previously shown, nearly all of the great ore localities of Mexico are associated with faults in this north 40° west direction. These faults are all of late geological origin, and probably the movements of the earth which made them are still going on, as testified by hot springs, in the vicinity.

By plotting the trends of the faults and folds of the Cordilleran region upon a large map, such as cannot be produced here, it certainly appears as if the northern part of the Mexican plateau province from the Colorado plateau southward through the northern tier of the States of Mexico has been the site of the crux of two distinct periods of mountain making movements, as illustrated in the Sierra Almoloya. One of these, and the older, has northerly trends representing the Laramide movements of the typical Rocky Mountain type, and the other having the northwestern trends of the Coast Range type, probably representing a post-Miocene revolution. The hypothesis of the crossing of these two belts of deformation explains many of the hitherto unexplained phenomena of our continent along the International Boundary and in Northern Mexico. It is certainly worthy of further test by field observation. Further study may show that the post-Miocene belt of northwest-southeast movement crossing the Laramide or Rocky Mountain north-south movement may extend from California across Mexico and connect with the Antillean movements of the West Indies.

Robert T. Hill

New York City

THE BIOLOGICAL LABORATORY OF THE U. S. BUREAU OF FISHERIES AT WOODS HOLE, MASS.[1]

Investigators and Assistants.—During the past laboratory season, thirty-two investigators were engaged in the study of various phases of marine biology, this number being somewhat larger than has been recorded for any summer within the last four years. Of these investigators, fourteen received a salary from the bureau, while eighteen are to be classed as volunteers. Thirteen junior assistants were also employed for various duties in the laboratory and in the field, two of whom, from the nature of part of their work, have like-

[1] Report of the work for the summer of 1906.

wise been included among the paid investigators. If we add to the foregoing, a librarian and a clerk, detailed from the Washington office, a collector, permanently employed at the Woods Hole Station, two temporary janitors and a chambermaid, we have forty-nine persons directly or indirectly engaged in the furtherance of scientific research. But the list would be quite incomplete without mention of the crews of the vessels *Fish Hawk, Phalarope* and *Blue Wing,* who of course rendered assistance of a most important kind.

Collecting.—The purchase of almost an entire new fish trap or pound was necessary at the commencement of the season. This was set, as usual, in Buzzards Bay, at a point not far from Woods Hole, and furnished much of the material used by those engaged in the study of fishes and their parasites. Visits were made from time to time to the extensive group of traps at Menemsha Bight, Marthas Vineyard, where material of value was obtained from the fishermen, who have always gladly cooperated with employes of the bureau in obtaining specimens of interest. Frequent trips were likewise made by the collecting vessels to various parts of Buzzards Bay and Vineyard Sound, and occasionally to more distant points. An unusually large amount of drifting *Sargassum* from the Gulf Stream was taken in Vineyard Sound, yielding abundant specimens of the extraordinary fishes, mollusks and crustaceans peculiar to the so-called 'gulf weed.' Finally, systematic shore collecting was carried on at various points, furnishing much material for research purposes as well as records for the biological survey.

Biological Survey.—Definite steps were taken toward bringing this project to a provisional close. No new areas of sea bottom were explored, though dredging was carried on at various points by the *Fish Hawk* for the purpose of verification and supplementation, and for the collection of bottom deposits. These last were submitted to Professor Gilbert van Ingen, of Princeton University, who has undertaken a study of the inorganic constituents of the local sea-floor. This, it is believed, may be of considerable importance in relation to the distribution of the bottom fauna and flora.

There remained considerable unidentified material among the collections of the preceding year, and these were sent to various taxonomic specialists, generally those who had previously done this work. It was found impossible, however, thus to dispose of two groups of great importance numerically, viz., the bryozoa and the amphipods. Accordingly, Dr. R. C. Osburn, of New York City, has undertaken the difficult task of reporting upon the former group; while Dr. Leon J. Cole, of Kingston, R. I., has assumed the equally burdensome task of identifying the amphipods. This latter undertaking is rendered peculiarly thankless, by the recent appearance of a fine monograph on the local amphipods by Professor S. J. Holmes.[1] The determination of our local bryozoa, on the other hand, may be almost regarded as pioneer work. The labors of Messrs. Osburn and Cole have been continued into the present winter, but it is planned that the lists shall be ready in time to allow of their inclusion in the projected report.

As above mentioned, systematic littoral collecting was conducted in a number of different places, along the shores of both Buzzards Bay and Vineyard Sound. It was not the object to keep thorough-going records of all species taken in each locality, as had been done in the case of the dredging stations. But it was thought desirable to extend the known distribution of certain species, for which few exact records are extant. It is to be hoped that ultimately the distribution of the littoral fauna of this region will be plotted out with the same minuteness as has been done for the bottom forms. The shore collecting was under the direct supervision of Messrs. Cole, Osburn and Sumner. The material collected has been sorted out and has been forwarded (or awaits forwarding) to various authorities. Dr. W. G. Van Name, of Springfield, Mass., has already reported upon the compound ascidians taken during

[1] 'The Amphipods of Southern New England,' Bulletin U. S. Bureau of Fisheries for 1904 (published in 1905), Vol. XXIV., pp. 457–529.

the past summer, as well as those of the two preceding summers. The other groups have been (or will be) referred to the authorities who have disposed of the dredging material of previous years; and reports are hoped for in time to be incorporated into the published report of the survey.

It is appropriate that mention should here be made of the loss sustained by the bureau in the death of Lieut. Franklin Swift, U.S.N., for two years commanding officer of the *Fish Hawk,* who died of typhoid fever at Charleston, S. C., on the tenth of November, 1906. Capt. Swift's knowledge of triangulation methods, derived from past experience in the Coast and Geodetic Survey, and from the recent work of the *Albatross* in Monterey Bay, made him an expert in that particular branch of navigation required in systematic dredging operations. The conscientious accuracy of his methods was felt by all of those who witnessed his work at Woods Hole.

Report upon the Work accomplished.—It has been thought desirable to bring the present biological survey to a provisional close, and to publish the results of the work already accomplished. With this in view, the present writer, who has directed the scientific work at the Woods Hole station for the past four summers, has been employed by the bureau to complete these results during the current winter; and a year's leave of absence from college teaching has been granted him for this purpose. A clerk has been detailed, and a scientific assistant appointed, in order to facilitate this undertaking. The work is now being carried out at Woods Hole.

Museum.—The formation of a representative collection of local marine animals was commenced many years ago, though the necessity for such a collection has not always received adequate recognition, and the existing specimens have suffered much from lack of attention. Dr. R. C. Osburn, as acting curator, has recently accomplished much in the way of reorganizing the local museum, and the extensive collecting operations of the past few years have provided much material. The museum now contains a fairly representative set of specimens which have been authoritatively identified by the various experts assisting in the determination of dredging material. It has been decided to fit up two large rooms upon the third floor of the laboratory building for the reception of these specimens. It is planned to make this a reference collection of local forms of life, rather than a public display of curiosities. Any one who has felt the need of identifying an unfamiliar species in the course of his biological investigations will realize the utility of such a museum.

Library.—The library has always proved to be an extremely important adjunct to this laboratory; and its use has not been restricted to investigators here, but has been extended to the whole scientific colony at Woods Hole. The contents of the library have been in part permanent, in part temporary. The permanent collection consists for the most part of government reports, and of separates donated by various authors. In addition to these, however, an extensive collection of standard biological works and periodicals have for a number of seasons past been loaned by the department of anatomy of Brown University. While greatly appreciating the privilege of using these books, it is felt by the Bureau of Fisheries that the Woods Hole laboratory should possess a permanent and fairly adequate working library of its own. It is the bureau's intention, therefore, to begin the purchase of the more necessary works as soon as funds are available for the purpose.

Individual Investigations.—Of the thirty-two investigators, nine represented Harvard University; four, Columbia; three each came from Yale University and the College of the City of New York, and two from Brown. The remainder represented thirteen different institutions in the east, west, north and south.[2]

For the purposes of the present synopsis, the

[2] In this computation each investigator has been accredited to the institution in which he had taught or studied *during the preceding academic year.* Two investigators have been referred to two institutions each. In the ensuing list, on the other hand, that institution has been named *to which* each investigator went at the close of the summer.

subjects of research may be rather arbitrarily classified as follows:

General physiology, reactions to stimuli and behavior	11
Taxonomy	7
Faunal distribution (including survey work)	6
Anatomy and histology	4
Regeneration	3
Embryology (including maturation of the egg)	3
Parasites of fishes	2
Economic, in narrower sense (not including the last)	2
Miscellaneous	5
Total	43

It must here be borne in mind that some investigators were concerned with more than one subject of research; likewise that in a few instances one subject of research has been included under two heads.

Mention may be appropriately made here of the visit of Sir Frederick Nicholson, of Madras, who was engaged in an investigation of American fisheries methods on behalf of the government of India, and who was the guest of the laboratory for several days.

List of Investigators

Carl L. Alsberg, A.M., M.D., instructor in biological chemistry, Harvard Medical School: The chemical composition of the blood of selachians.

Arthur M. Banta, A.M., Austin research fellow, Harvard University: The reactions of marine amphipods to light.

Robert P. Bigelow, Ph.D., instructor in biology, Massachusetts Institute of Technology: The Stomatopoda collected by the *Albatross.*

Wesley R. Coe, Ph.D., assistant professor of comparative anatomy, Yale University: Regeneration in nemerteans.

Leon J. Cole, Ph.D., chief of division of animal breeding and pathology, Agricultural Experiment Station, Kingston, R. I.: Collecting on behalf of biological survey; identification of local amphipods. (Salaried research assistant.)

Edgar D. Congdon, A.M., Austin teaching fellow of Harvard University: Pigment migration in the eyes of crustacea.

Joseph A. Cushman, assistant curator, Boston Society of Natural History: Systematic work upon local sponges, ostracods and foraminifera. (Salaried research assistant.)

Irving A. Field, professor of biology, Westminster College, Westminster, Md.: The food value of some hitherto unused or little used marine animals. (Salaried research assistant.)

Addison Gulick, A.M., graduate student in Harvard University: The histology and function of the osphradium in molluscs.

Chas. W. Hargitt, professor of zoology, Syracuse University: The anthozoa of the Woods Hole region. (Salaried research assistant.)

Geo. T. Hargitt, A.M., teacher of zoology, Syracuse High School, Syracuse, N. Y.: The effect of salt solutions upon regeneration and growth.

Davenport Hooker, student in Yale University, assisted Dr. W. R. Coe.

H. E. Jordan, A.M., fellow in zoology, Princeton University: Cytological studies of echinoderm eggs.

William E. Kellicott, Ph.D., professor of biology, Woman's College, Baltimore: Correlation of external and internal characters in certain fishes.

Beverly W. Kunkel, Ph.D., instructor in biology, Sheffield Scientific School, Yale University: Studies of amphipods and of teleost brains.

Edwin Linton, Ph.D., professor of biology, Washington and Jefferson College, Washington, Pa.: The entozoa of fishes. (Salaried research assistant.)

Jesse F. McClendon, Ph.D., Randolph-Macon College, Ashland, Va.: The development of parasitic copepods. (Salaried research assistant.)

Hanford McCurdy, A.M., Cleveland, Ohio: Hybridization experiments with echinoderms.

Charles V. Morrill, graduate student, Columbia University, assisted in the work of the biological survey; likewise was engaged in the study of regeneration in fishes. (Salaried assistant.)

Max Morse, tutor in natural history, College of the City of New York, assisted in the work of the biological survey; likewise carried on studies upon the reactions of *Gonionemus*. (Salaried assistant.)

Edward Mueller, assistant in American Museum of Natural History, New York: Studies of Hydroids and sea-anemones.

R. C. Mullenix, graduate student, Harvard University: The caudal spinal nerves of elasmobranchs.

Raymond C. Osburn, Ph.D., teacher of zoology in the New York High School of Commerce: Collecting on behalf of the biological survey; identification of local bryozoa. (Salaried research assistant.)

George H. Parker, Ph.D., professor of zoology, Harvard University: The hearing of the weakfish, *Cynoscion regalis*. (Salaried research assistant.)

Herbert R. Sass, M.A., acting assistant in biology, College of Charleston: Factors influencing the distribution of littoral fauna.

George G. Scott, M.A., instructor in natural history; College of the City of New York: Experiments upon the regeneration of the fins of fishes; experiments upon the physiology of the blood of fishes. (Salaried assistant.)

H. D. Senior, M.B., associate in anatomy, Wistar Institute of Anatomy: The development of the blood vascular system of the teleosts.

Michael X. Sullivan, Ph.D., instructor in physiological chemistry, Brown University: The physiology of the digestive tract of elasmobranchs; the rectal gland of elasmobranchs; experiments to determine the effect of coal-tar contamination upon the health of fishes. (Salaried research assistant.)

Francis B. Sumner, Ph.D., director of the laboratory: Biological survey of local waters; the effects upon fishes of variations in the chemical and osmotic properties of the water.

Millett T. Thompson, Ph.D., assistant professor of zoology, Clark University: The masking habit and the food of the spider crabs.

Lloyd P. Upton, graduate student, Brown University: Physiology of nerve and muscle in the dogfish.

Gilbert van Ingen, Ph.D., assistant professor of geology, Princeton University: Examination of bottom deposits. (Salaried research assistant.)

Francis B. Sumner

Fisheries Laboratory, Woods Hole,
December 15, 1906

AMERICAN ASSOCIATION OF MUSEUMS

The second annual meeting of the American Association of Museums will be held in Pittsburg, June 4–6. The headquarters of the association will be at the Hotel Schenley. The first session will be held in the Carnegie Museum on Tuesday, June 4, at ten o'clock. A preliminary program will be issued about May 20. Titles of papers to be presented before the association should be sent as early as possible to Dr. W. J. Holland, Carnegie Museum, Pittsburg. All those interested in the objects of the association are invited to become members and to be present at the meeting. Fees for dues (two dollars for active members, ten dollars for sustaining members) may be sent direct to the treasurer, Dr. W. P. Wilson, the Philadelphia Museums, Philadelphia.

George A. Dorsey,
Secretary

THE SMITHSONIAN INSTITUTION AND THE NATIONAL ACADEMY OF SCIENCES AND THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

In order to bring in closer touch the representative national scientific organizations of the country and the Smithsonian Institution at Washington, and to create a new channel for the diffusion of knowledge, Secretary Charles D. Walcott recently extended to the National Academy of Sciences and to the American Association for the Advancement of Science an invitation to make the Smithsonian Institution their headquarters.

The members of the National Academy of Sciences, Secretary Walcott invited "to deposit their archives, records, etc., in rooms provided for the purpose in the Smithsonian Institution, where the secretary of the academy could have access to them and attend to such business as might be necessary in connection with the affairs of the academy." The secretary of the American Association for the Advancement of Science similarly was offered storage, office rooms and post-office facilities in the institution.

From Mr. Arnold Hague, home secretary of the National Academy of Sciences, the following communication has been received at the institution:

"At a meeting of the National Academy of Sciences held April 17, the secretary of the academy was requested to express to the secretary of the Smithsonian Institution the sincere thanks for the facilities afforded the academy during its sessions in Washington.

"The secretary was also requested to express the thanks of the academy for the room assigned to the academy for the use of its secretary and the keeping of the necessary files and papers, and also for the use of the members of the academy during their visits to Washington."

Dr. L. O. Howard, permanent secretary of the American Association for the Advancement of Science, has sent a letter of acceptance in the following form:

"The council of this association, at its spring meeting held in Washington on April 17, passed a formal and hearty vote of thanks to you for your great courtesy to the association in setting aside for its use storage and office rooms and a post-office box in the Smithsonian Institution."

By the location of the offices of these national scientific societies in the Smithsonian Institution, the institution becomes a clearing-house of national scientific learning. In this manner are forwarded the objects named in the will of James Smithson, the document by the terms of which the institution was founded, 'the increase and diffusion of knowledge among men.'

SCIENTIFIC NOTES AND NEWS

THE city council of Jena has given a plot of land, on which a philogenetic museum will be established in honor of Professor Ernst Heinrich Haeckel.

IN honor of Lord Lister's eightieth birthday the *Münchener medizinische Wochenschrift* has printed a German translation of his articles, containing the rudiments of the antiseptic theory as applied to the treatment of wounds, which began to appear in the *Lancet* of March 16, 1867.

PROFESSOR ADAM POLITZER will this year retire from the active duties of the chair of otology at the University of Vienna, and it is proposed to present to him a gold medal. Those who wish to take part in this testimonial and to secure copies of the medal should address Dr. G. Kaufmann, Vienna, VI., Mariahilferstrasse, 37.

M. DESLANDRES, of the Astrophysical Observatory at Meudon, has been elected president of the Astronomical Society of France.

AMBASSADOR BRYCE formally presented to Professor Ernest W. Brown, of Haverford College, the Royal Astronomical Society gold medal for 1907 (awarded last February), before the opening of the afternoon session of the American Philosophical Society, on Friday, April 19. Ambassador Charlemagne Tower and a large number of other members of the society and their friends were present.

PROFESSOR GEORGE L. MEYLAN, of Columbia University, has been elected president of the American Physical Education Association.

PRESIDENT IRA REMSEN, of the Johns Hopkins University, will make the principal address at the commencement exercises of the Michigan School of Mines.

MR. J. D. BOWMAN, secretary of the Carnegie Foundation for the Advancement of Teaching, has announced that, although the foundation has declined to admit state universities to the accepted list of beneficiaries of the fund, it occasionally grants retiring allowances to men in state institutions who have rendered distinguished academic service. In accordance with that plan the executive committee of the foundation has given retiring allowances to the following: E. Benjamin Andrews, chancellor of the University of Nebraska, who has been a prominent teacher and educator for thirty years. Francis H. Smith, for more than fifty years professor of natural philosophy at the University of Virginia. William V. Folwell, for fifteen years president of the University of Minnesota and now professor of economics in that university. Amos N. Currier, for forty years professor of Latin and Greek in the University of Iowa and now dean of the college of liberal arts at that institution.

It is announced that the Lowell Observatory expedition to the Andes for the purpose of observing the apposition of Mars and the eclipse of the sun in July next will be in charge of Professor D. P. Todd, head of the astronomical department of Amherst College. He will be accompanied by Mrs. Todd, A. G. Illse, mechanician, and E. C. Slipher, of the Lowell Observatory, an expert photographer. The party will sail on May 11 for Colon.

Professor T. A. Jaggar, Jr., of the Massachusetts Institute of Technology, has gone to Alaska, where he will study the seismic and volcanic conditions.

Dr. George B. Gordon, of the Free Museum of Science and Art of the University of Pennsylvania, will this summer make an expedition to Alaska to make ethnological studies and collections.

Dr. Charles S. Prosser, professor of geology in Ohio State University, has a leave of absence during the present year, and has spent considerable time in comparing Ohio material with type or authentic specimens in Cornell and Columbia University museums, in the American Museum of Natural History and in the New York State Museum.

The University of Upsala will commemorate the bicentenary of the birth of Linnæus on May 23 and 24, and on May 25 the exercises will be continued at the Royal Academy of Sciences, Stockholm.

The botanical seminar of the University of Nebraska will celebrate the two hundredth anniversary of the birth of Linné (Linnæus) on the twenty-third of May, instead of the earlier date announced some time ago. The program includes an address by Dr. Roscoe Pound, a paper on 'Prelinnean Botany' by Dr. Clements, one by Dr. Bessey on the 'Position of Linné in Botany,' and one by Dr. Ward on 'Linné as a Zoologist and Physician.' Following the program there will be a convocation of the seminar at which the usual refreshments will be served.

New York University will unveil bronze tablets in honor of eleven Americans in the Hall of Fame on May 30, including memorials of Louis Agassiz and Maria Mitchell.

In memory of Pierre Curie a medallion is about to be placed in his laboratory in the Municipal School of Physics and Chemistry.

Dr. Carl Ludwig Rominger, the veteran paleontologist, and for fourteen years the state geologist of Michigan, died at his home at Ann Arbor, Michigan, on April 22, at the age of 86 years.

Dr. Franz Kjellman, professor of botany at Upsala University since 1883, has died at the age of sixty-one years.

Dr. Albert von Mosetig-Moorhof, professor of surgery in the University of Vienna, was drowned on April 26.

The collection of photographs and autograph letters of the members of the National Academy of Sciences since its organization, made by Dr. Marcus Benjamin, has been purchased by an anonymous donor and presented to the academy.

The tenth Congress of Polish Men of Science and Physicians will be held this year at Lemberg some time between June 16 and July 24. There will be a scientific and medico-hygienic exposition in connection with the congress.

The seventh International Congress of Physiologists will be held this year at Heidelberg, August 13–16, under the presidency of Professor A. Kossel. The previous congresses were held at Basle, in 1889; Liége, in 1892; Berne, in 1895; Cambridge, in 1898; Turin, in 1901, and Brussels, in 1904.

The New York Botanical Garden has arranged its course of spring lectures, to be delivered in the lecture hall of the museum building of the garden, Bronx Park, on Saturday afternoons, at four o'clock, as follows:

April 27—'The Life Story of a Tree,' by Dr. C. Stuart Gager.

May 4—'The Flowers of Trees and Shrubs Growing Wild near New York City,' by Dr. N. L. Britton.

May 11—'Jamaica: Its Flora, Scenery and Recent Disaster,' by Dr. M. A. Howe.

May 18—'Water Lilies and other Aquatic Plants; their Relation to Horticulture,' by Mr. G. V. Nash.

May 25—'The Influence of Vegetation in the Formation of Recent and Ancient Swamps,' by Dr. Arthur Hollick.

June 1—'Some Little-known Edible Fruits of the United States,' by Dr. H. H. Rusby.

In order to provide a method for viewing the collections of the garden under guidance, an aid will leave the front door of the museum building every week day afternoon at three o'clock, to escort all who may wish to accompany him. The routes will be as follows: Monday, Hemlock Forest and Herbaceous Garden; Tuesday, Pinetum; Wednesday, Fruticetum and North Meadows; Thursday, deciduous arboretum, nurseries, propagating houses; Friday, public conservatories; Saturday, museums.

Nature states that the Port Erin Biological Station has never been more fully used by workers in marine biology than during the present Easter vacation. From the last week in March onwards throughout April, systematic collecting at sea and investigations in the laboratory have been actively pursued by as many biologists as can be comfortably accommodated. During the first half of April ten to twelve investigators occupied seats in the laboratory, and about the middle of the month a dozen senior students came in addition. The researchers include Professor B. Moore (biochemistry), Dr. H. Roaf (physiology of crustacea), Mr. J. Pearson (cancer), Mr. R. D. Laurie (biometrics), Mr. W. J. Dakin (Pecten), Professor Herdman, Mr. Wollaston and Mr. Gunn, all from Liverpool University; Professor Hickson, Mr. Chaffers and Mr. Whitnall, from the Victoria University of Manchester; Mr. Unwin, from the University of Leeds; and Mr. Chadwick, the resident naturalist. Plankton collections, both surface and deep, are being taken periodically, at stated localities, over a limited area for statistical purposes, from the steam yacht *Ladybird,* and the usual sea-fish hatching and distribution of larval plaice is in progress.

We learn from the *British Medical Journal* that the medico-historical collections of the 'Empress-Frederick House for Higher Medical Study' were recently opened. Professor Schjerning, Director-general of the Army Medical Department, was present, and the Prussian Ministry of Education, and the Imperial Board of Health sent delegates. Professor E. Hollander, to whose skill in collecting and special knowledge of medical history, the collection owes the greater part of its objects, made the opening speech. He pointed out the value of a historical collection for teaching purposes. It gives students the best insight into medicine and therapeutics of past ages, showing in what manner medical aid was carried out, and what were the notions entertained by doctors and laity as to the nature of disease. The collection was then inspected; it contains originals and copies of ancient apparatus, oil paintings, water colors, engravings, etc., state and municipal rescripts, coins, medals and many miscellaneous objects of interest. A special division of the collection has been set apart for sending on loan to other university towns.

UNIVERSITY AND EDUCATIONAL NEWS

Mr. John D. Rockefeller has given to the University of Chicago land fronting the south side of Midway Plaisance, extending from Madison Avenue to Cottage Grove Avenue. This property is said to have been acquired by Mr. Rockefeller at a cost of $1,500,000 and to be now of considerably greater value.

Williams College will receive $192,000 by the death of E. W. Currier.

It is announced that a new physical laboratory will be erected at Princeton University through gifts whose source is not announced.

By the recent death of Mrs. Eliza Orne Ropes at Salem, Mass., public bequests to various institutions, amounting to $1,500,000, made in trust by Mary Putnam Ropes, who died in 1903, were released. Of these Harvard will receive a bequest to endow the Nathaniel Ropes professorship of political economy. If, after the endowment of the professorship, any surplus remains it will go to the Peabody Museum of Archeology and Ethnology.

Mr. Theodore P. Shonts will provide Drake University of Des Moines with fifty service scholarships of $50 each.

Miss Anna T. Jeanes, of Philadelphia, has created an endowment fund of $1,000,000, the income from which is to be applied toward the maintenance and assistance of elementary schools for negroes in the southern states.

Mrs. Russell Sage has given $75,000 to the Syrian Protestant College in Beirut, Turkey.

Mr. Byron E. Walker has given his paleontological and geological collections and library to the University of Toronto.

The statute of limitations was held to bar the Catholic University of America from recovering $78,000 from John F. Waggaman, said to be due on notes executed to the late Thomas E. Waggaman.

It is said that the Louisiana State University will establish this year a medical department in New Orleans.

The hundredth anniversary of the birth of Ezra Cornell was celebrated at Cornell University on April 26 and 27. On the first day the exercises were held in the Armory, and were presided over by President Schurman; an address on behalf of the trustees by Mr. Andrew Carnegie, who was unable to be present through illness, was read; an address on behalf of the faculty was made by Dr. Andrew D. White, the first president of the university, and addresses were made on behalf of the alumni and students. On April 27 the new agricultural building was dedicated, and addresses were made by Governor Hughes, ex-Governor Batchelder, of New Hampshire, and Professor Liberty H. Bailey, dean of the College of Agriculture.

In connection with the Yale Summer School Professor Gregory, who is one of the directors of the Connecticut Geological Survey, has arranged to take a class of teachers to the various parts of the state, where the most typical formations are to be found, and will lecture in the field to the class.

It is said that the principalship of the University of Toronto has been offered to Dr. Robert A. Falconer, principal of Pine Hill College, Halifax.

Dr. John Scholte Nollen, professor of German in Indiana University, has been elected president of Lake Forest University, Lake Forest, Ill. He takes charge on June 20, 1907.

At the University of Wisconsin, Professor H. L. Russell has been elected dean of the College of Agriculture to succeed Dean W. A. Henry, who has resigned. Otis A. Gage, now of the faculty of Cornell University, has been appointed assistant professor of physics; A. N. Winchell, now of the Montana School of Mines, assistant professor of mineralogy and petrography, and C. H. Hawes, of Cambridge University, lecturer in anthropology. The following promotions have also been made: From associate professor to professor, E. C. Elliott, education; Victor Lenher, chemistry. From assistant professor to associate professor, D. H. Otis, animal nutrition. From instructor to assistant professor, W. F. Dearborn, education; R. H. Denniston, botany; C. J. Davis, hydraulic engineering; J. H. Vosskuehler, machine design; J. G. Moore, horticulture; C. A. Ocock, engineering.

Benjamin L. Miller, A.B. (Kansas), Ph.D. (Johns Hopkins), of the department of geology at Bryn Mawr College, has been appointed professor of geology at Lehigh University.

Dr. H. W. Stuart, of Lake Forest University, has been appointed assistant professor of philosophy, and Dr. George H. Sabine has been appointed instructor in philosophy at Stanford University.

Legh W. Reid, associate professor of mathematics in Haverford College, has been appointed professor of mathematics.

At Cornell University, Mr. R. P. Lay has been appointed instructor in experimental engineering, and Robertson Mathews in machine design.

Dr. Grimbert has been appointed to a newly-established chair of biological chemistry in the School of Pharmacy of the University of Paris.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MAY 10, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ZOOLOGY AT THE NEW YORK MEETING

I

AT the convocation week meetings held in New York, December 27, 1906, to January 2, 1907, Section F of the American Association for the Advancement of Science and the Eastern Branch of the American Society of Zoologists held joint meetings throughout for the reading of papers. In the forenoon of Friday, December 28, there was a joint session of these two societies with Section G, devoted to papers on animal and plant breeding, and in the afternoon of the same day the societies joined with the American Society of Naturalists in a discussion on the biological significance and control of sex, which has been published in full in SCIENCE for March 8. Section F and the Society of Zoologists held separate business sessions, the proceedings of which follow.

SECTION F

A business meeting was held December 27 for the election of officers, and the section was represented in a committee of the council which recommended the appointment by the president of the association of a committee to arrange a suitable memorial of the fiftieth anniversary of the publication of the 'Origin of Species.' The officers for the ensuing year are as follows:

Vice-president and Chairman—E. B. Wilson, New York.

Secretary—C. Judson Herrick, Granville, Ohio.

Member of Council—Herbert Osborn, Columbus.

Member of General Committee—E. L. Rice, Delaware, Ohio.

Sectional Committee—E. B. Wilson, E. G. Conklin, C. Judson Herrick, H. B. Ward (one year), Frank Smith (two years), W. E. Ritter (three years), A. M. Bleile (four years), A. L. Treadwell (five years).

AMERICAN SOCIETY OF ZOOLOGISTS

The fourth annual meeting of the Eastern Branch and the seventeenth annual meeting of the society since its establishment as the American Morphological Society elected the following officers for 1907:

President—Charles B. Davenport.

Vice-president—F. H. Herrick.

Secretary-Treasurer—H. E. Crampton.

Member of Executive Committee (to serve three years)—W. R. Coe.

Eleven persons were elected to membership, making the total membership of the Eastern Branch 135. A grant of $125 was voted toward the expenses of the International Zoological Congress to be held in this country next August.

Professor Crampton tendered his resignation as secretary-treasurer, and after adjournment of the session C. Judson Herrick was appointed by the executive committee to fill the vacancy.

JOINT PROGRAM

The Functions of the Nervous System of the Razor-shell Clam: G. A. Drew, University of Maine.

On the Sense of Sight of Spiders: A. Petrunkevitch, Indiana University.

The Sense of Vision in the Dancing Mouse: Robert M. Yerkes, Harvard University.

That brightness vision is fairly well developed in the dancer is shown by its ability to discriminate blacks, grays and whites. Color vision is extremely poor. There is some indication of the discrimination of red and green and of red and blue, but none whatever of blue and green. All my experimental tests as well as my observations of the habits of the mouse support the conclusion that such visual guidance as is received results from stimulation by brightness differences. There are many reasons for believing that the red end of the spectrum is much lower in brightness value for the mouse than for man. The general behavior of the dancer and the results of form, brightness and color tests show that vision is not very important in the life of the animal.

An Experimental Study of the Image-forming Powers of Various Types of Eyes: Leon J. Cole, Rhode Island Agricultural Experiment Station, Kingston, R. I.

The responses of certain phototropic animals to two areas of light of different size, but of equal intensity, were used as criteria in drawing inferences as to the image-forming powers of their eyes. To one side was a ground-glass, lighted from behind, which gave an evenly illuminated area 41 cm. square. To the other side was practically a point of light; but at the position midway between them, where the experiments were performed, the intensities of the two lights were equal. Eyeless forms (the earthworm was used) turned practically an equal number of times toward each light, showing no power of discriminating between them. Animals with 'direction eyes' were but little better in this respect (*e. g.*, *Bipalium*, *Oniscus*, larva of *Tenebrio*). On the other hand, animals with well-developed 'compound eyes' (*Vanessa*, *Ranatra*) and 'camera eyes' (frogs) discriminated readily, positive animals turning much more often to the large light, and negative animals more often to the small. This discrimination was taken as evidence of image-formation by the eyes. Frogs

(*Acris gryllus*) with the skin covered but eyes exposed reacted like normal frogs; without the use of the eyes their responses corresponded to those of the earthworm.

We have thus a physiological test of the image-forming powers of the eyes, and in these experiments it corroborated in the main inferences which would be drawn from a study of the structure of the eyes in question.

This paper is published in Proc. Am. Acad. Arts and Sciences, Vol. 42, No. 16, pp. 335–417, Jan., 1907.

The Significance of the Grasping Antennæ of Male Harpacticoid Copepods: L. W. Williams, Harvard Medical School.

This paper has been published in Science for February 8.

Further Observations on the Behavior of Tubicolous Annelids: Chas. W. Hargitt, Syracuse University.

Following up the work done on these animals and reported elsewhere, the writer has extended the observations to aspects of behavior other than those already recorded. Three points are concerned in the following observations:

First, a study of behavior under natural conditions of environment. This has been possible in quiet pools near low tide lines. Experiments on *Hydroides dianthus* with shadow stimuli, or light intensity of varying degree, under these conditions have confirmed in all essentials those made last year.

Experiments as to tactile responses showed considerable variations as compared with the former series. This may be attributed to the fact that specimens living under these conditions become more or less inured to similar stimuli from the actions of waves which naturally buffet them almost constantly.

Second, experiments on the relative sensory acuteness of specimens from deep water, about twenty fathoms, compared with those from shallow waters, one to three or four fathoms. In cases tested there was shown a definite preponderance of positive reactions among the latter, and a corresponding preponderance of negative responses in the former.

Third, a comparative study of the aspects of behavior shown in the growth of colonies taken from shore waters, subject to the action of waves, and those from quiet waters of bays, etc., shows an unmistakable variability in the aspects of the tubes, which clearly indicates environmental adaptation. Furthermore, specimens growing in an environment, such as marly bottom, or silt, or other similar condition, show the same evident response of adaptation. On the other hand, specimens growing along shore lines, or on rocky bottoms, show likewise the unmistakable response natural to such condition. Not a single colony among hundreds along the shore lines showed any free and vertical tubes. Likewise specimens dredged from muddy bottoms showed the erect and vertically directed tubes which would bring the animals above the obstructing mud.

Any careful consideration of the facts would hardly fail to convince one that no single factor, such as heliotropism, or geotropism, or any other tropism alone, was adequate for their explanation.

Rhythmical Pulsation in Animals: Alfred G. Mayer, Carnegie Institution of Washington.

Experiments made at the Tortugas Marine Laboratory of the Carnegie Institution upon *Cassiopea, Salpa, Lepas* and the loggerhead turtle give results as follows:

Rhythmical pulsation can be sustained only when a strong stimulus is counteracted by an inhibitor, so that the pulsating organism is maintained at or near the threshold of stimulation in a state analogous to that of unstable equilibrium, thus allowing weak internal stimuli to produce recurrent movement.

In the lower marine animals the NaCl, calcium and potassium of the sea-water combine to form a powerful stimulant, which if unchecked would produce only sustained tetanus, but the magnesium overcomes this effect by its anesthetic (diastolic) influence.

The pulsating organs of terrestrial animals are also stimulated by optimum combinations of NaCl, with potassium and calcium, and this is held in check by a definite proportion of magnesium.

A Ringer's solution resembles this optimum combination of NaCl, calcium and potassium, and is only a stimulant, not an inorganic food. It must be counterbalanced by magnesium in order to enable it to sustain pulsation indefinitely.

In *Cassiopea* any paralyzed strip of subumbrella tissue, cut in the shape of a closed circuit, will remain indefinitely in rhythmical pulsation, if once a contraction wave be started in the circuit. Every time this wave returns through the circuit of tissue to the place whence it started, it is restimulated and sent forth anew, and being thus reinforced at each return it is sustained indefinitely.

In the scyphomedusa, *Cassiopea,* the diffuse nervous or epithelial elements of the sub-umbrella transmit the pulsation stimulus to which the muscles respond by contraction.

The peripheral muscular layer of the wall of the loggerhead turtle's heart is the only part actively concerned in the rhythmical movement, and the internal cavernated mass of the heart's tissue may be removed without checking the pulsation. This peripheral part of the muscular wall of the heart tends to maintain itself in pulsation very much as will circuits made of the sub-umbrella tissue of *Cassiopea.*

The pulsation-stimulus acts solely upon the peripheral muscular layer of the heart's wall, the inner cavernated tissue remaining passive.

The above is a brief review of Publication No. 47 of the Carnegie Institution of Washington, 'Rhythmical Pulsation in Scyphomedusæ,' 1906.

The Interrelation of Sensory Stimulations in Amphioxus: G. H. Parker, Harvard University.

To weak acid solutions and other like mixtures the anterior end of *Amphioxus* was found to be most sensitive, the posterior end less so, and the middle trunk region least sensitive. To the pressure of a camel's hair brush, the middle region was less sensitive than the two ends, which, however, were not distinguishable one from the other by this method of stimulation. To a current of warm water (40° C.) the anterior end was most sensitive, the middle less, and the posterior end least. There were no reactions to a current of cold water (2° C.). To a fine beam of strong sunlight, previously passed through water to eliminate heat, the anterior end including the 'eye spot' was not sensitive, the region immediately behind the 'eye spot' was most sensitive, the posterior region slightly less so and the middle region least so.

The distribution of sensitiveness to light corresponds to the distribution of the pigment cups in the central nervous organ and these cups are without doubt the mechanisms concerned with the reception of light. The distributions of the other classes of sensitiveness are in mutual agreement, and, from the nature of their stimuli, these classes are doubtless represented by integmentary nerve terminals. To what extent these classes are independent may be inferred through the effects of exhaustion. After the tail of *Amphioxus* has been repeatedly stimulated with weak acid, the animal ceases to respond to this stimulus but is still normally sensitive in that part of its body to heat or to mechanical stimulation. In a similar way after exhaustion to mechanical stimulation or to heat stimulation, the particular part of the body experimented upon is still sensitive to the other classes of stimuli. Exhaustion to light stimulation has no effect upon the sensitiveness to the other classes of stimuli. These observations lead to the conclusion that light, heat, mechanical and chemical stimuli are received by physiologically separate mechanisms and that these mechanisms are located in the skin except in the case of light, whose receptive organs are the pigment cups in the central nervous organ.

Analysis of the Cyclical Instincts of Birds: Francis H. Herrick, Western Reserve University.

The behavior of wild birds is primarily determined by a number of commanding instincts of ancient origin. These cardinal instincts are of two kinds, namely: (1) *continuous instincts,* which are needed for the preservation of the individual, such as preying, fear, concealment and flight, and (2) *cyclical instincts,* which are necessary for the maintenance of the race. By cyclical instincts we mean those discontinuous, recurrent impulses which attend the reproductive cycle, and which may be described as parental instincts.

The cyclical or parental instincts as a rule recur with almost clock-like precision, in spring or summer, with repetitions within the breeding season in certain species. They are modified by the continuous instincts, such as fear, and the instinctive behavior as a whole is liable to modification at every point by intelligence. Neglecting such changes for the present, we will briefly analyze the cyclical instincts, reserving details and tabular statements for a fuller presentation.

The reproductive cycle is made up of a series of terms, representing discrete acts or chains of actions which follow in a definite succession. Eight or more terms may be recognized, many of which, such as brooding and feeding the young, are recurrent within the series. The cycle may be graphically represented by a number of tangent circles, each one of which stands for a distinct sphere of influence, or subordinate series of related impulses, named and numbered as follows: (1) Spring migration; (2) courtship and mating (often attended by song); (3) selection of nesting site and building nest (often accompanied by the fighting instinct); (4) egg-laying; (5) incubation—including care of eggs, such as shielding, rolling, cleaning and covering (fear often completely blocked by brooding instinct); (6) care of young in nest, subject to the following analysis: (*a*) feeding young, including capture and treatment of prey, return to nest (pause), call-stimulus, testing reflex response of throat, watching for reflex response (pause); (*b*) inspection of young and nest; (*c*) cleaning young and nest; removal and disposition of excreta; (*d*) incidental care of young and incidental behavior in this and other terms of cycle, such as brooding, shielding or spreading over young whether sitting or erect, bristling and puffing, preening, gaping, stretching and yawning, guarding and fighting; (7) care and incidental education of young when out of the nest; guarding, feeding, play, and other instinctive acts; (8) fall migration. Beginning at 2, 3 or 4, according to circumstances, the cycle may be repeated once or oftener within the season.

The coordinated instinctive responses of the young begin in the sixth term, and are mainly as follows: (6) Initial responses at moment of hatching or shortly after, including grasping movements of limbs, elevation of head, opening of mouth, and the swallowing reflex in response to contact of bill of old bird or of food in deep part of throat (in the altricial species); characteristic actions in muting following feeding, in response to the stimulus of food and the attitude of inspection in adult; call-notes, pecking and gaping, stretching and spreading in response to heat, flapping, fear and flight; (7) calling (teasing), following, crouching and hiding, play, imitation, preying and flight; (8) fall migration.

The formula of the reproductive cycle given above is a composite, which with slight changes will apply to most of our common wild birds. In the most aberrant cases of behavior, where the parental instincts have been reduced to a minimum as in the cow buntings of North America and in some of the megapodes, the cycle ends abruptly with term 4, and in the cowbird there is no attempt to either build a nest or to conceal the eggs.

Some Features in the Behavior of the Starfish: H. S. Jennings, Johns Hopkins University.

The paper gave an analysis of the righting reaction of the starfish, and showed that the animal could, by a systematic course of training, be caused to form a habit of righting itself in a certain definite way.

Movement and Problem-solving in Ophiura brevispina: O. C. Glaser, University of Michigan.

1. *Ophiura brevispina* moves in practically all of the ways possible to a pentaradiate animal.

2. Its behavior in removing obstructions from its arms is not perfected by practise under ordinary conditions.

3. Preyer's conclusion that Ophiurans are intelligent is not substantiated by this study; for not only is it impossible to demonstrate 'resolution' or improvement, by the method that he employed, but the assertion that an animal is intelligent because when stimulated it performs varied movements until some one of these brings about cessation of the stimulus, leads into difficulties, for these animals often perform in instantaneous succession movements that fail for the same reason. *Ophiura*, moreover, hardly ever executes a single movement, but usually a considerable number. Each of these on Preyer's view results in learning, but it is impossible without striking evidence to the contrary, to believe that Ophiurans can learn half a dozen things at the same time. If some of all the movements performed at a certain instant are 'correct,' the case is farther complicated in that some of all the things which the animal learns fall into the category of successes, some into the category of failures.

4. The reason why *Ophiura brevispina* does not improve under ordinary circumstances is probably due to its versatility. This animal can perform a surprising number of movements. Of all these some are better fitted to meet a certain difficulty than others, but a considerable number will serve the purpose. Where the number of solutions to a problem is large, it is not surprising that no particular method of solution should be perfected, viz.: that resolution should not occur.

The Breeding Habits of the Florida Alligator: Albert M. Reese, Syracuse University.

The habits of the alligator were studied during parts of three summers in the Everglades, in the swamps of central Florida, and in the Okefenokee Swamp. The time of laying is the month of June, usually during the second and third weeks. The nests, which are built on the bank near the caves of the alligators, vary considerably in size, and consist of a very compact mass of damp, decaying vegetation. They probably serve more as a means of keeping the eggs moist and at a constant temperature than as a means of heating them. The average number of eggs in a single nest is about thirty, forty-eight being the greatest number found in one nest. The eggs are so closely packed in the nest that it seems hardly possible that the young alligators, on hatching, should be able to dig their way out; it is possible that the female who laid the eggs may hear the noise made by the young before hatching and may dig them out of the nest before they suffocate. The period of incubation is probably about eight weeks, and sometimes is found to have begun before the eggs are laid, so that eggs taken directly from the oviducts may contain well advanced embryos. There is considerable variation in the size of the eggs, the variation in long diameter being greater than that in short diameter. The average long diameter of the four hundred eggs measured was 73.742 mm. The average short diameter was 42.588 mm.

An Electric Wax-cutter for Use in Reconstructions: EDWARD L. MARK, Zoological Laboratory, Harvard University.

The wax-cutter is made by heating a platinum wire about 0.4 mm. in diameter by means of an electric current regulated by a rheostat consisting of ordinary electric lamps of different candle power and arranged in multiple. To give the wire alternating motion parallel to its length, it is stretched in a frame made of a bent steel rod, one portion of which is substituted for the 'needle-bar' of an ordinary household sewing machine. The melted wax is withdrawn through a copper tube—kept hot by passing through a small hot-water tank—attached to a suction pump of the Bunsen type.

The apparatus is fully described and illustrated in a number of the *Proceedings* of the American Academy of Arts and Sciences published in March.

The Microscopic Structure of the Stigmal Plates of the Tick Genus Dermacentor: C. W. STILES, Washington, D. C.

The Circulatory System in Nereis: H. R. LINVILLE, New York.

The general plan of the circulatory system and the circulation in Nereis, as observed in living individuals, is a median dorsal vessel in which the blood flows anteriorly as the result of peristaltic waves of contraction in the wall of the vessel, and a larger median ventral vessel in which blood flows posteriorly without contraction of the wall. Anteriorly the dorsal vessel branches at the cephalic plate into four vessels, and the blood is carried downward and posteriorly through a set of capillaries in the region of the pharynx to the ventral vessel. Posteriorly the last three somites of the trunk have single pairs of blood vessels which carry blood upward into the dorsal vessel. Beginning at the eleventh trunk somite and extending to the fourth somite from the posterior end, there is a complicated arrangement of lateral vessels and capillaries. At a point near the anterior end of each intermediate somite a pair of 'hearts' lying close to the intestine carry blood downward in peristalic waves, to a pair of short vessels which connect with the ventral vessel. A portion of the blood carried by the hearts passes into these short connecting vessels, and then into the ventral vessel or out into another pair of blood-vessels that start from the ventral ends of the short connecting vessels and extend to the nephridia in the ventral rami of the parapodia. The remainder of the blood from the hearts goes into a pair of vessels, which are continuous with the hearts but bend outward and upward and pass through the dorsal musculature into the somite in front, to sets of skin capillaries there on the dorsal rami of the parapodia and on the dorsal surface of the somite. Blood passes through these sets of capillaries, with the blood from the nephridial capillaries, into a pair of vessels which empty blood into the dorsal vessel immediately after the peristaltic wave of contraction in the dorsal vessel has passed the point of connection.

The Relation of Variability to Food Supply as illustrated by the white daisy, Chrysanthemum leucanthemum L. and the yellow perch, Perca flavescens Mitch.: L. B. Walton, Kenyon College.

Notwithstanding the numerous biological problems which have been attacked by means of statistical methods during the last ten years, an absence of evidence concerning the effect of food supply upon the variability of organisms exists. It was with a view toward obtaining data bearing upon this particular problem that the present investigation was undertaken. While the natural environment by no means furnishes conditions for obtaining the best results, it seemed advisable, at least in a preliminary survey of the subject, to adopt such a method.

In the first part of the investigation results were obtained from the ray flowers in two groups of the common white daisy (*Chrysanthemum leucanthemum* L.), 500 heads growing on rich soil (group A) and 500 heads growing on poor soil (group B) were examined. The specimens were collected on the same day and from localities approximately one mile apart. While the mode (33) and the mean (28.786) were much greater in specimens growing on rich soil (cf. Ludwig, Tower, Shull, etc.) than in those on poor soil (21) (25.632), the index of variability in each group was approximately the same taking into consideration the probable error.

In the second part of the investigation results were obtained from the number of pore-bearing scales in the lateral line of two groups of yellow perch (*Perca flavescens* Mitch.) obtained in Lake Erie. The one group (group A) was procured from a locality (cove in Sandusky Bay) where there was every reason to believe that the food supply approached the maximum, while the other (group B) was collected from the rocky shores of an island some ten miles distant where the food supply apparently approached the minimum. Again the index of variability showed no decided difference when the probable error was considered.

In connection with the ray flowers of the daisy it is of interest to note that specimens from rich soil exhibited a tendency toward an even number of ray flowers, while those from the poor soil had a tendency toward an odd number of ray flowers. This however may be a coincidence, although taken into consideration with the differences, a somewhat remarkable one. No decided tendency toward the Fibonacci series was apparent.

The computations were made by the ordinary method, checked by logarithms and a Burrough's adding machine. There are a number of possible errors minimizing the value of the results. These, together with the literature bearing upon the subject will be considered in the final paper.

While the above results suggest that food supply does not materially affect variability, it is evident that work upon a larger number of specimens, as well as carefully controlled experimental investigations, where the effects of different groups of stimuli may be segregated, will be necessary before any final conclusions may be drawn.

A Study in Variation, Geographical Distribution and Mutation in Snails of the Genus Partula from Tahiti: H. E. CRAMPTON, Columbia University.

In presenting the more important results of a recent study in the field of terrestrial pulmonates of the island of Tahiti, belonging to the genus *Partula,* it was shown that different valleys contain forms that on account of their more or less complete isolation have come to differ in correlation with their geographical proximity or remoteness. The vital conditions that limit the snails of this island to their particular stations are dryness peripherally, where the valleys debouch upon the coastal alluvial plain, and lower temperature centrally. Only rarely may stragglers pass from one region to another.

Evidence was adduced showing that 'mutations' have arisen at various recent times, the observations of Garrett and Mayer, taken in connection with the results of the writer, making it certain that at least three forms have thus originated, at dates that may be determined with substantial accuracy. It was furthermore shown, in corroboration of Mayer's contention, that the environmental conditions can not be regarded as the factors that have produced the several specific and varietal differentia exhibited by the Tahitian snails.

On a Case of Reversion induced by Cross-Breeding and its Fixation: W. E. CASTLE, Harvard University.

This paper has been published in SCIENCE of January 25.

Reversion: C. B. DAVENPORT, Cold Spring Harbor, Long Island.

Observations on the Habits of Salt Marsh Mosquitoes: JOHN B. SMITH, New Brunswick, N. J.

During the summer of 1906 a close watch was maintained on the development of mosquitoes on the salt marsh area near Elizabeth, New Jersey. In all ten distinct broods developed, the first observed April 19 and coming to maturity May 2; the tenth observed October 12 and coming to maturity soon after. Larvæ were found however until November 30 after even heavy frosts. Broods I., II. and III. were mostly *Culex cantator*; in brood IV., *C. cantator* and *C. sollicitans* were almost equal; brood V. had 80 per cent. *cantator,* and after that *C. sollicitans* was in the majority in all cases. The latest larvæ, however, were *cantator.* There was a great difference in the habits of the various broods: I., II., IV. and VI. were migrants and left the marshes in great numbers; the others remained on the marshes or did not get very far inland. There seemed to be a relation between numbers and migrations, the heavy broods migrating most and farthest.

An Undescribed Species of Noctuid Moth from New York City: HENRY BIRD, Rye, New York. Illustrated by box specimens showing larvæ and habits.

New Evidence from Primitive Sharks on the Origin of the Limbs of Vertebrates: RAYMOND C. OSBURN, Columbia University.

Embryological studies on *Heterodontus japonicus* Dumeril, a cestraciont, and on *Chlamydoselachus anguineus* Garman, a notidanid shark, show that there is a deep-seated, primary similarity between the paired and unpaired fins embracing all the structures of the fins—skeleton, muscles, nerves, blood supply and ceratotrichia. The arguments recently advanced by the gill-arch theorists for the branchial origin of the paired limbs (viz., abortive muscle-buds, fusion of muscle-buds, collector nerves, discrepancy between muscle and fin rays, and fin-migration) are all disposed of by showing that these conditions may exist in any fin, unpaired as well as paired. The pectoral girdle is proved to be not serially homologous with the gill arches. In Heterodontus the pectoral girdle is shifted forward during development toward the gill region instead of away from it as the gill-arch theory assumes. The attachment of the trapezius muscle, assumed by the adherents of the gill-arch theory to be 'an old relic of a former branchial musculature supplying the shoulder girdle,' is shown instead to be secondary. The pelvic arch has primarily no dorsal prominence homologous with the scapular portion of the pectoral arch, but, on the other hand, the pectoral arch passes through a stage similar to the pelvic arch when only the ventral portion is present. Hence neither of them is to be regarded as a modified gill arch. In all fins the condensation or thickening of the mesenchyme, from which the skeleton is later differentiated, begins always in the fin-fold in contact with the ectoderm and extends inward, and is thus of external origin in contrast to that of the gill arches which arises next to the endoderm of the pharynx. In the paired and unpaired fins the sequence of development of the various structures is identical. The above facts show such striking similarity between the paired and unpaired fins, in the development of all structures, and such contrast with the gills, as to strongly support the fin-fold theory of Thacher, Balfour and Mivart.

On the Structure, Development and Relationship of Blastoidocrinus (Billings 1859): George H. Hudson, Plattsburgh, Clinton Co., N. Y.

The only known species of the genus was described by Billings in Cam. Org. Rem., Decade IV. (1859). F. A. Bather in Part III. of the 'Treatise on Zoology' edited by E. Ray Lancaster gives it a family of its own and has placed it with *Asteroblastus* under Grade A, *Protoblastoidea* Bather (1899). The elaborate hydrosphere folds would cut it out of this grade, however, and while it has an ambulacral system like that of the Edrioasteroidea its brachioles would alone exclude it from that class. The present paper is based on a very perfect specimen (fragments only have been heretofore known), and on fragments and some thousand single and very perfect plates from specimens of different ages. The paper presents new and remarkable elements in Echinoderm structure, some from internal structures displayed by a section from the perfect specimen, and the development of many structures from five areas of 'primary meristem,' one each at the distal ends of the rays. Cystid, blastoid edrioasteroid and crinoid characters were briefly mentioned. The form has been made the type of a new order Parablastoidea. A more complete description is published in N. Y. State Museum Bulletin 107, p. 97.

Notes on the Periodical Literature of the Smaller Domesticated Animals: C. B. Davenport, Cold Spring Harbor, N. Y.

There is a mass of current periodical literature on the domesticated animals that is not taken cognizance of in the zoological bibliographies nor in those of agriculture. Consequently they are unknown to most zoologists. With the revival of scientific interest in breeding this literature becomes of great importance because it tells where stock is to be obtained and because it contains suggestive data on the factors of evolution. Taken together this mass of periodical literature constitutes a history of the current evolution of domesticated animals of the most detailed and intensive sort.

Origin of the Sperm-center in the Fertilization of Cerebratulus lacteus: Naohide Yatsu, Columbia University.

Since the discovery that a cytaster with a centriole may be formed *de novo,* the question has been raised as to whether the sperm-center may not be a cytaster produced by a stimulus exerted by the spermatozoon. This idea has led to the formulation of the chemical 'theory' of fertilization. It has, therefore, become important to examine the origin of the sperm-center. In studying the spermatozoon of *Cerebratulus lacteus* a definite granule was found in the middle piece, which takes a strong hæmatoxylin stain. After the entrance of the spermatozoon into the egg the middle piece swells up into a vesicle, and faint rays appear centering in the granule in the middle piece. The granule or centriole then escapes from the vesicle and becomes the center of the growing aster. At this moment there is no centroplasm or centrosome around it, the rays reaching the central granule. Later the central ends of the rays become obscure and the centroplasm is accumulated around the centriole. Then the centriole in the centrosome divides into two. But the daughter centrioles soon lose their centrosome and become naked. From this observation three conclusions may be drawn:

1. The centriole is actually introduced into the egg by the spermatozoon, excluding the possibility that the centriole in the sperm-aster arises from the egg.

2. The centrosome is derived from the egg substance.

3. The centrosome is not a permanent organ of the cell as has been thought by some cytologists.

A Graphic Method of Correlating Fish Environment and Distribution: Albert H. Wright, Cornell University.

It is some years since ornithologists saw the advantages of some graphic means of representing complex bird waves and their coincident relation to physical conditions. In ichthyology, a schematic method whereby fish distribution and environment can be correlated would seem of material aid.

The study of a stream and its fishes involves the consideration of factors so numerous and so diverse and accumulates such a mass of data, that one is almost driven, perforce, to adopt some graphic method to make results appear quickly and clearly.

The chart described, presents the range of each species in the stream and the important physical conditions which obtain, namely: the nature and the configuration of the bottom; the depth at any given point; the surface of the water; the drop in the stream and the current conditions; the altitudes of the mouth, source and all intermediate points; the distances in miles; woodlands, swamps, falls, mill-ponds, dams in use and remains of dams, small and important tributaries and whether from the right or left side, bridges for landmarks, canals, stream across the divide, the width at any place, at every mile point valley cross-sections in which the geologic formations may be shown, etc.

Venation of the Wings of Paleozoic Dragon-flies: E. H. Sellards, University of Florida.

The paper relates specifically to the dragon-flies of the Permian and is a summary of results published in detail elsewhere. The structural characters of especial interest are found in the disposition of the veins of the radio-median area, and particularly in the position of the radial sector, which is observed in the case of a large and well preserved specimen, to cross the first two branches of the media as in the case of modern dragon-flies. The other main veins of this area are also in essential agreement with the veins of the same area in modern forms. Such differences as occur are believed to be of less than ordinal value. The order Protodonata, established by Handlirsch, is not accepted; the Protodonates being regarded as a suborder of the Odonata.

Note on the Origin of the Mesoderm of the Polyclad, Planocera inquilina Wh.: Frank M. Surface, University of Pennsylvania.

According to Arnold Lang (1884) the mesoderm of the polyclads arises from the whole of the second and third quartets of micromeres. It had been long suspected that Lang was in error, but the subject was not investigated until 1898 when E. B. Wilson published some observations on a species of *Leptoplana*. He found that all the first three quartets contribute to the formation of ectoderm, while the mesoderm arises by budding in from cells of the second quartet. This mesoderm thus corresponds to the 'larval' mesoderm of annelids and molluscs. Wilson, however, found no evidence of mesoderm arising from members of the fourth quartet and thus in this one important particular the early development of the polyclads differed from the above-mentioned groups.

In working over the cell lineage of *Planocera inquilina* it has been definitely determined that in this species, mesoderm arises from the *posterior cell of the fourth quartet, i. e., 4d,* just as it does in annelids and molluscs. At the stage of about forty cells, *4d* buds into the interior a single large cell which later divides into a right and left moiety from which the mesodermal bands arise. Some of the mesoderm, however, arises from cells of the second quartet as described by Wilson.

Land Planarians in the United States: L. B. Walton, Kenyon College.

Leidy, at a meeting of the Philadelphia Academy of Science, August 12, 1851, presented a paper in which he described the first and only species of land planarian (excluding *Placocephalus kewensis,* an introduced form living in hot houses) which has thus far been found in the United States. To this he gave the name *Planaria sylvatica.* The five specimens he obtained were collected under flower pots, boxes, etc., in gardens at Philadelphia, and under pieces of bark, and old logs in the woods bordering Wissahicon Creek. On October 7 of the same year, after a more critical study of the specimens, he proposed a new genus for their reception, the name thus becoming *Rhynchodemus sylvaticus.* At a meeting of the society on August 24, 1858, he again referred to the subject stating that since 1851 he had found one specimen in the western part of Pennsylvania on Broad Top Mountain (August, 1857) as well as several specimens at Newport (July, 1858). Since this time no further observations concerning the collection of additional land planarians in the United States have appeared.

Consequently the occurrence of two distinct species of *Rhynchodemus* at Gambier, Ohio, is of considerable interest. The first form which may prove identical with the examples procured by Leidy at Philadelphia, was found on the partially decayed stem of a Virginia creeper, July 9, 1904, near Bexley Hall. Five specimens were obtained, while additional representatives have been found at the same place each succeeding summer. During November of the past year a single specimen was also taken under a stone in a meadow some three miles south of the preceding locality. The specimens mentioned agree closely with the description given by Leidy as well as with his drawing of the Philadelphia forms published in Girard's paper on planarians (*Ann. sc. Nat. Zool.,* 7 ser., pp. 145–310, 1894). The length is greater however, while the anterior part is constricted as figured by Girard for the Newport specimens. It is evident that two and possibly three species were confused by Leidy under the one name. His material was given to Girard and thus far it has been impossible to locate it.

The second form occurring at Gambier is considerably smaller than the preceding one as well as much lighter in color. Only two examples have been found, both under stones in damp woods. It seems quite distinct from any of those taken by Leidy as well as the species tabulated by von Graff in his excellent monograph.

Some papers in preparation dealing with the anatomical structure of the forms will probably make clear their systematic position. Furthermore, it appears evident that land planarians are widely distributed over the United States and that by reason of their similarity to young snails, they have often been overlooked by collectors.

Some Little-known Shark Brains, with Suggestions as to Methods: BURT G. WILDER, Cornell University.

This paper continues that of which an abstract was printed in SCIENCE for May 26, 1905. Now first, so far as I know, are shown the brains of *Heterodontus* (*Cestracion*) and *Pristiophorus.* With the former the cerebrum and cerebellum resemble those of the 'acanth' (*Squalus acanthias*), indicating an antiquity little if any greater. Notwithstanding certain ectal resemblances of the two dentirostral genera, *Pristis,* the 'saw-ray' and *Pristiophorus,* the 'saw-shark,' their brains differ markedly, the latter's being the more primitive. Their inclusion within the same family or even the same division would seem to me an error less only in degree than would be their combination with *Xiphias, Polyodon* and *Psephurus* as 'Rostrata,' or than was Günther's association of Ganoids and Selachians as 'Palæichthyes,' aptly characterized by Gill as a 'piece of scientific gaucherie.' Upon encephalic grounds I think *Pristiophorus* and *Scymnorhinus* should be excluded from the Squalidæ, and *Sphyrna* from the Carchariidæ. The brain of each selachian genus is, I think, recognizable, but I am less certain as to family forms. The Notidanoid or Diplospondylous type is well marked, and includes *Scymnorhinus.* At present the rays can not be distinguished from the sharks in any such simple way as, *e. g.,* the Anura may be from the Urodela by the secondary fusion of the olfactory bulbs. Perhaps, in no shark is the prosocele so nearly obliterated as it seems to be in all rays. In no ray do the cerebral protrusions remain unconjoined as in some sharks; but, paradoxically, in no ray is there, as in several sharks, so nearly a complete obliteration of the evidence of their primary independence. Under 'methods' may be enumerated: (1) The need of well-preserved brains of all species; (2) maintaining the natural contours, especially of thinner parts, by injecting the preservative into the cavities; (3) making solid injections of the cavities; (4) exposing brains with a 'shoe-knife,' obliquely shortened; (5) exploring with the 'syringotome' or canaliculus knife; (6) the use of sheets of uniform size, say 35 x 45 cm., upon which, in a manner permitting change, are drawn outlines of the animal and of its characteristic parts, especially the brain; such sheets may be arranged and rearranged upon the wall so as to facilitate research and exposition to small classes.

The Primary Septa in the Rugose Corals: CLARENCE E. GORDON, Amherst Mass.

The number of these septa is still in dispute. The assertion of Professor Duerden that there are six protosepta is contradicted by other evidence of an important nature. Professor Duerden's argument is not conclusive because he has not yet shown in what way a tetrameral plan of growth shown in the early condition of certain individuals can be in all cases the result of imperfect silicification. The possibility of acceleration is not denied and the very species that he studied show strong evidences of acceleration in development. Hence the uncertainty of reasoning from sections, about which there must always be a good deal of doubt.

Early Stages in Streptelasma and Allied Genera: Thomas C. Brown, Columbia University.

This paper considers an argument concerning the original tetrameral arrangement of the septæ in *Streptelasma* and allied genera of the rugose corals and describes the protheca as observed in these genera.

Fission in the Hydroid Corymorpha: Harry Beal Torrey, University of California.

Spontaneous fission occurs occasionally in Corymorpha, across heteromorphic stems. The latter are geotropic, both ends turning upward, and take the form of U's, attached at the loop, with free vertical limbs. The distal limb is the longer, with the larger and earlier hydranth.

The fission plane is first defined by a constriction in the loop of the U. The stem attenuates in this region, owing to the migration of proximal and distal limbs away from each other. The cœnosarcal canals are obliterated and the circulation characteristic of the stem ceases. This may be the cause of the disintegration which may appear here. Frustules arise on both sides of the constriction before fission is completed.

The new polyps, proximal and distal, are normal in all respects. Morphallaxis plays an important rôle in their complete development.

Variation during the Life-Cycle of Infusoria in its Bearings on the Determination of Species: Lorande Loss Woodruff, Williams College.

The data summarized, derived from the culture of various infusoria through long periods, suggest that it is customary to regard the structure most frequently observed in 'wild' Infusoria as too constant in character, and to overlook the fact that modifications occur throughout the life-cycle which are in no sense abnormal, and which must be taken into account in the determination of species.

1. *Oxytricha fallax,* for example, shows a variation of over 250 per cent. in length, and over 300 per cent. in width during the life-cycle.

2. The shape of the animal changes greatly at different periods of the life-cycle.

3. The form of the macronucleus alters exceedingly, not only toward the end of the life-cycle but also at periods of the highest reproductive activity. Peculiarities in shape are sometimes transmitted from generation to generation. During a cycle extending over about a year, the macronucleus was 'normal' in form for less than three months.

4. The number of micronuclei varies during the life-cycle. There is apparently a tendency toward numerical reduction during the early part of the cycle, and reduplication during the latter part.

5. The quantitative relation of cytoplasmic and nuclear material changes during the life-cycle. This is brought about by both cytoplasmic, and macronuclear and micronuclear changes.

6. The proportionate length of the ciliary apparatus varies, the cilia being longer during high reproductive periods of

the life-cycle. This is probably due to a reduction in size of the body unaccompanied by a proportionate reduction of the ciliary apparatus.

7. The general activity of the organism varies greatly. During periods of high division-rate, motion is exceedingly rapid, and in a general way may be said to diminish with the rate of division.

8. Stimuli produce different effects at different periods of the life-cycle. K_2HPO_4, for example, accelerated the division-rate of *Gastrostyla steinii* during the early part and retarded it toward the end of the life-cycle.

Notes on the Life History of the Nematode Hæmonchus contortus: B. H. RANSOM, Bureau of Animal Industry, U. S. Department of Agriculture.

Hæmonchus contortus, a nematode worm of the family Strongylidæ, which lives parasitic in the fourth stomach of ruminants is responsible for great losses among sheep in the United States, especially among lambs. The lambs become infected, through the medium of the pasture, from the adult sheep of the flock. Full grown sheep may be heavily infested and show little or no evidence of disease. Recent studies in the zoological laboratory of the Bureau of Animal Industry have brought out a number of interesting facts in the life history of *H. contortus.* The eggs of the parasite hatch out in the droppings of the host, and the embryos develop to the final embryonic stage, known as the ensheathed stage, in a period of time varying according to temperature, from three to four days at 95° F., to three to four weeks at 50° F. Ensheathed embryos crawl up perpendicular surfaces whenever the air is saturated with moisture, and by such migrations gain positions on grass blades when they are likely to be picked up by grazing animals. Ensheathed embryos when swallowed by a sheep develop to maturity in two to three weeks, but eggs and embryos which have not reached the ensheathed stage are not infectious when swallowed. The eggs and newly hatched embryos are quickly killed by freezing or drying, but ensheathed embryos suffer no apparent injury from freezing and may live in a dried condition for at least thirty-five days. When the temperature is 40° F., or below, the eggs remain dormant, and a small percent may retain their vitality for as long as two months, but none survived three months in the experiments tried. Embryos have been kept alive in cultures at a temperature of about 70° F. for as long as six months. Enclosures previously pastured by infested sheep were still infectious after remaining empty for seven months, from November to June. It is suggested that lambs may be kept from becoming infected with the parasite and the flock in course of time freed from infection by moving the flock from one pasture to another before the embryos have time to develop to the ensheathed stage in which they crawl upon the grass, the time being determined by means of cultures of feces from infested animals. Each time the flock is moved a culture is made by placing a small quantity of feces with sufficient water to make a thick paste in a wide-mouthed corked bottle and the culture exposed to out-door temperature. When ensheathed embryos begin to crawl up the inner surface of the bottle where they may readily be seen with the aid of a hand lens, the sheep are moved to another enclosure.

On the Place of Origin and Method of Distribution of Taste Buds in Ameiurus melas: F. L. LANDACRE, Ohio State University.

Taste buds appear simultaneously in the extreme anterior portion of the oral cavity (ectoderm) and on the endoderm of the first three gill arches. Buds always spread posteriorly from these places of origin by discontinuous groups. Those of the pharynx spread back into the œsophagus and are continuous with the buds on the last gill arch. Those of the anterior oral cavity spread back in the mouth by discontinuous groups until they reach the area occupied by the pharyngeal buds and they also spread back on the outer surface of the body by discontinuous groups until they reach the posterior portions of the body.

No buds spread from the pharyngeal group to the outer surface of the body. The first taste buds to appear on the outer surface are continuous with those just inside the lips. All the remaining buds appear in discontinuous groups determined partly by the distribution of the rami of the V. and VII. nerves, but not entirely so. There are six well defined groups of buds on the outer surface of the body and two in the anterior oral cavity distinct from the dorsal and ventral lip buds.

The appearance of buds in the oral and cutaneous areas in detached groups spreading from anterior to posterior seems to indicate the order in which specialized communis fibers reach the surface through rami of the V. and VII. nerves. A comparison of the rami bearing fibers in *Ameiurus* with other types shows a very great degree of variability in the geniculate ganglion of the VII. nerve as to the number of rami through which it may send communis fibers and as to the time at which it sends them in *Ameiurus*. The functional needs of the organism, such as changes in the methods of seeking and locating food, seem to determine the direction of spreading and also to be more important factors in determining the manner of appearance (*i. e.*, in detached groups) than the mere anatomical arrangement of trunks and rami of the nerves, so that the discontinuous groups may be designated as functional groups.

The Central Reflex Connections of Cutaneous Taste Buds in the Codfish and the Catfish. An Illustration of Functional Adaptation in the Nervous System: C. Judson Herrick, Denison University.

The taste buds which occur in the outer skin of siluroid and gadoid fishes have been thoroughly studied anatomically and physiologically, their innervation worked out and their central reflex connections compared with those of the tactile nerves from the same cutaneous areas. The peripheral gustatory and tactile nerves of the cod and the catfish are the same in principle, with the exception of the location of the most sensitive areas used in the locating of food. This area is on the barblets of the catfish, but on the filiform pelvic fins of the gadoids, particularly the smaller forms, like the hake and tom cod. Correlated with this difference is a striking difference in the course of the secondary gustatory tracts for nerves coming from the cutaneous taste buds. In the catfish the facial gustatory center has migrated forward for ease of correlation with the tactile and motor centers of the barblets, jaws, etc., and there is a broad connection between this facial lobe and the general tactile center in the funicular nuclei, whence a common motor reflex path serves to put both sense organs into relations with the motor centers. In the cod there has been no forward migration of the facial lobe, because the tactile nerves from the most sensitive area come in behind the vagal lobes by way of the spinal nerves. And the secondary gustatory path from the terminal nucleus of the cutaneous taste buds does not connect with the tactile correlation center, but passes directly to the motor centers. This short-circuiting of the reflex path from cutaneous taste buds is also an adaptation to the different and more active movements made by the cod in feeding.

C. Judson Herrick,
Secretary

(*To be continued*)

SCIENTIFIC BOOKS

Second Report of the Wellcome Research Laboratories at the Gordon Memorial College, Khartoum. Andrew Balfour, M.D., etc., Director. Department of Education, Sudan Government, Khartoum 1906. Royal 8vo. 255 pp., 21 plates, 106 figures.

The functions of the Wellcome Research Laboratories founded by private munificence are thus expressed in the language of the foundation:

(*a*) To promote technical education; (*b*) to promote the study, bacteriologically and physiologically, of tropical disorders, especially the infective diseases of both man and beast peculiar to the Sudan, and to render assistance to the officers of health, and to the clinics of the civil and military hospitals; (*c*) to aid experimental investigations in poisoning cases by the detection and experimental determination of toxic agents, particularly the obscure potent substances employed by the natives; (*d*) to carry out such chemical and bacteriological tests in connection with water, food stuffs, and health and sanitary matters as may be found desirable; (*e*) to promote the study of disorders and pests which attack food and textile producing and other economic plant life in the Sudan; (*f*) to undertake the testing and assaying of agricultural, mineral, and other substances of practical interest in the industrial development of the Sudan.

The first report of these laboratories covered the history of its work up to January, 1904; the second, now before us, brings the record down to the early part of 1906. The director, Dr. Andrew Balfour, assisted by a staff of five or six scientists, has achieved a piece of work that from every standpoint deserves the highest praise. The difficulties of scientific work in a region so far removed from supplies and necessities, to say nothing of conveniences, one where "native helpers have proved to be only broken reeds," "not to be trusted beyond the bottle washing stage," can not easily be over-estimated. Despite this the field covered both in territory and in topics investigated, is so broad and the results presented in the report so extensive, that only the most important can be noted here.

F. V. Theobald, the consulting entomologist, has written a fine chapter on the mosquitoes, as well as others on human, animal and vegetal pests. E. E. Austen, of the British Museum, London, has contributed also a valuable chapter on blood-sucking diptera from the Anglo-Egyptian Sudan.

The work reported by the director himself is full of interest. It begins with a record of mosquito work in Khartoum and the Anglo-Egyptian Sudan. By the persistent work of the mosquito brigade anophelines have been practically abolished and the town kept in a fairly satisfactory condition, one vastly different from that which used to obtain. "At Khartoum the subject is complicated by the presence of mosquito-carrying steamers, boats and barges. Were it not for these, success would have crowned the efforts at extinction." For about $350 in 1905 Khartoum was kept free from malaria, and to a very large extent also from the annoyance which usually adds so much to the discomforts of life in the tropics; a trivial expense for such immunity.

Of other biting insects the distribution of *Glossina morsitans,* the carrier of trypanosomiasis in animals has been found to be somewhat general in the southern Sudan, and *G. palpalis,* the vector of the human trypanosome, has been positively identified from the extreme southern limits of the country. Valuable data are given on the habits of other biting insects, including the Congo floor maggot, and the true jigger, or Chigoe, not heretofore reported from the Sudan. Some records of ticks and an extended discussion of *Aphis sorghi* and of locust swarms, and their destructive work as well as of their parasites, are worthy of note.

A hæmogregarine from the jerboa, or desert rat, which was the first to be found in mammals, is described in detail and well illustrated. It is similar to one since reported by Captain Christophers in India. The free motile stage was observed only three times, but cysts and the merozoites were regularly found in the liver and kidney. Interesting forms from the jerboa flea which were at first regarded as developmental stages of this hæmogregarine were found on further investigation to be in reality parasites of the flea itself. Other forms from small mites (*Dermanyssus*) which infest the jerboa may prove to be the developmental stages sought. Another very interesting series of observations concerns a leucocytozoon of mammals, obtained from the blood of a Norway rat, although it could not be found in numerous examinations of the blood from many Egyptian rats.

Trypanosomiasis in the Anglo-Egyptian Sudan received careful attention. In the region south of the tenth parallel of latitude it certainly exists to a very considerable extent, affecting donkeys, horses, mules and possibly camels. This is not the species, *Trypanosoma nanum*, found in cattle. Of the latter disease the report discusses at length its symptoms and post mortem findings, as well as its morphology and inoculation experiments. Similar though more extended records are given for an undetermined species from mules which is probably identical with *T. dimorphum*.

One very interesting chapter embraces the report of the traveling pathologist and naturalist, Sheffield Neave, who spent four months in the field in southern Sudan. His chief effort was to locate the natural source of infection with the tropical blood parasites. In all he made 750 blood films, from 55 human subjects, 118 other mammals, 69 birds, 33 fish, 6 amphibia, and 18 sheep ticks. Trypanosomes were found in man, the mule, four fish, and two birds. Filariæ were found in five birds, *Halteridium* in eight, and a new *Hæmamœba* in one. All of these forms are described in detail. Many insects and a few plants were collected on the trip, and numerous data recorded regarding the tribes with which he came in contact.

The chemist of the Wellcome Research Laboratories gives a valuable chapter on a multitude of subjects from the chemical composition of Nile waters to the use of asbestos in ancient burial, and new forms of chemical apparatus. It would be impossible even to cite all, but the extended study of gum arabic, its varieties, occurrence, uses, grading, determination of viscosity, etc., and the careful examinations of Nile waters are investigations of fundamental importance.

The work is well printed and splendidly illustrated. One hundred and six fine text figures and twenty-one full-page superb colored plates add greatly to the scientific value of the report.

Such rich results reflect great credit upon the director and his staff and furnish a most ample justification of the generosity and foresight of the founder. Institutions more favorably outfitted and conveniently located may well be jealous of their laurels when such reports as this appear.

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A 1671 ENGLISH VERSION OF NICOLAUS STENO'S DE SOLIDO INTRA SOLIDUM NATURALITER CONTENTO, BY H. O.

The recent reprinting of Steno's classic 'Prodromus' in Germany has revived the interest in this seventeenth century anatomist and geologist and in the few scarce editions of his remarkable treatise. His famous tract which appeared in Latin at Florence in 1669 was, according to Professor von Zittel, reprinted in Leyden in 1679 and a French translation was brought out by Elié de Beaumont in 1832, but neither von Zittel nor the booklists to which I have had access make mention of an English translation of the book. Just recently there fell into my hands an evidently little known English version printed in London in 1671, with the following title-page:

The | Prodromus | to a | Dissertation | Concerning | Solids Naturally Con- | tained within Solids. | Laying a Foundation for the Ren- | dering a Rational Accompt both of | the *Frame* and the Several *Changes* of | the Masse of the Earth, as also | of the various *Productions* in the same. | By Nicolaus Steno. | English'd by H. O. | London, | Printed by *F. Winter*, and are [*sic*] to be Sold | by *Moses Pitt* at the *White-Hart* in | *Little Brittain*, 1671.

There are sixteen pages of preface with the title-page, and 112 pages of text and one plate; the size of the printed part of the page measures 2.75 inches wide by 5.5 inches high.

Why has this translation been overlooked and who was H. O.?

I am not aware that any writer on the history of geology specifically refers to having seen or read this translation. The copy in the writer's possession is bound up as a separately paged tract at the end of a small volume of the celebrated Robert Boyle's 'Essays of Effluvium,' etc., containing also his 'Essay about the Origine and Virtue of Gems' of 1672. A general title-page gives reference to Steno's work. This title-page is dated 1673. All of the contained tracts appear to have been separately printed at different dates between 1671 and 1673, at which last date they were brought out in the form above described.

The translation appears to have passed out of sight in the same century for John Ray, elected to the Royal Society, 1667, who rewrote his now curious 'Three Physico-Theological Discourses' in 1693, twenty-two years after the H. O. translation appeared, does not mention either the original Prodromus or this translation of it. Had he known either work probably Ray would not have quoted in his second edition (pages 156–157) Steno's earlier 'Description of a Shark's Head' to the neglect of the most important scientific contribution to the discussion of the origin of fossil shells and geological structures which was extant in his time. It is difficult to account for Ray's reticence unless by reason of his living outside of London. But the publication of the H. O. translation of Steno's 'Prodromus' as an appendix to Boyle's prolix essays was from the start likely then as now to bury the work out of the sight of any writer on geological subjects.

As for H. O., the translator, he reveals himself in a preface of six pages entitled 'The Interpreter to the Reader' as having recently received a copy of the original Latin work from Italy, as meeting and hearing a declaration from 'the excellent Robert Boyle,' as being familiar with his opinions and writings, and as well with 'Mr. Robert Hook,' his occupation in the 'rebuilding of the city of London, and his attendance on the R. Society,' from which account it is to be inferred that H. O. also was much about the Royal Society, and his dealings with Boyle who was one of the founders of that institution strengthens this opinion. We know that during these years Henry Oldenburg[1] was secretary of the Royal Society. It is further known that Boyle was in the habit of employing persons to translate works from one language into another at his expense. Oldenburg's initials attached to this translation, his conversation with Boyle concerning the latter's Essay on Gems, of which interview he states that Boyle "before he would see or hear anything of that Prodromus of Steno, did upon occasion declare to the author of that English version the sum and substance of what is deduced at large [regarding gems] in this tract," and the consociation of Boyle as founder and Oldenburg as first secretary of the Royal Society at this time and of H. O.'s translation with Boyle's 'Essays' make it highly probable that Henry Oldenburg (c. 1626–1678) englished Steno's 'Prodromus.'

In the interests of a wider acquaintance of many English-speaking students with the path-breakers of modern geology and paleontology the H. O. version of Steno's 'Prodromus' might deservedly be reprinted.

J. B. WOODWORTH

GEOLOGICAL LABORATORY,
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SOCIETIES AND ACADEMIES

THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE

THE twentieth meeting of the Society for Experimental Biology and Medicine was held in the Rockefeller Institute for Medical Research, on Wednesday evening, February 20. The president, Simon Flexner, was in the chair.

Members present — Adler, Burton-Opitz, Calkins, Carrel, Conklin, Emerson, Ewing, Field, Flexner, Foster, Gibson, Gies, Lee, Levene, Levin, Mandel (J. A.), Meltzer,

[1] See Encyclopedia Brittanica, 9th ed., Vol. 17, page 439, Vol. 22, page 401, and index volume.

Meyer, Murlin, Noguchi, Opie, Salant, Wolf, Yatsu.

Member elected—C. Ward Crampton.

Abstracts of Original Communications[1]

Experimental Studies on Nuclear and Cell Division: E. G. Conklin.

Extensive experiments were made on the segmenting eggs of *Crepidula plana.* These experiments included a study of the influence on nuclear and cell division of hypertonic and hypotonic sea water, of ether, alcohol, etc., of the lack of oxygen, of the electric current, and of pressure and shaking. Many important conclusions were reported and numerous drawings shown.

Heterotransplantation of Blood Vessels: Alexis Carrel.

The author's method consisted of removing a segment of the abdominal aorta of a cat, and of reestablishing the circulation in the lower part of the aorta by interposing a segment of the jugular or carotid of a dog and suturing it to the cut ends of the aorta. It was found that a segment of a dog carotid which had been transplanted in a cat could act as artery for seventy-eight days at least.

Transplantation of the Kidney with Implantation of the Renal Vessels in the Aorta and Vena Cava: Alexis Carrel.

The transplantation of the kidney with implantation of the renal vessels in the aorta and vena cava consists of extirpating from an animal a kidney with its vessels, together with a segment of the aorta and vena cava; also of transplanting the kidney into the abdomen of another animal and suturing the edges of the patches to the edges of suitable openings made in the walls of the aorta and vena cava. The author used this method mainly on cats and obtained excellent results from the standpoint of restoration of the circulation. Of seven animals operated on, six remained in good condition. The seventh died of intestinal intussusception four days after the operation.

[1] The abstracts presented in this account of the proceedings have been greatly condensed from abstracts prepared by the authors themselves. The latter abstracts of the communications may be found in Number 3 of Volume IV. of the society's proceedings.

Secondary Peristalsis of the Esophagus—a Demonstration on a Dog with a Permanent Esophageal Fistula: S. J. Meltzer.

Injections of indifferent solutions or of air directly into the esophagus cause there a regular peristaltic movement. This latter form of peristaltic movement, which for the sake of brevity the author terms *secondary peristalsis,* differs essentially from *primary peristalsis,* that which follows deglutition, through the nervous mechanism by which it is controlled. The secondary peristalsis requires the presence of some sort of a bolus within the esophagus, and presupposes the integrity of the latter; whereas the primary peristalsis requires neither a bolus nor the integrity of the esophagus. Even if a large section of the latter is removed, the peristalsis appears in the lower segment in due time after each deglutition as long as the vagus nerves remain intact.

The author demonstrated both forms of peristalsis in a dog with a permanent fistula in the upper half of the cervical esophagus.

Peristaltic Movements of the Rabbit's Cecum and their Inhibition, with demonstration: S. J. Meltzer and John Auer.

When a well-fed rabbit is fastened on its back on a holder and the hair of the abdomen is removed, as a rule movements of the cecum can be seen sooner or later. The movements are well marked and characteristic in their appearance, and leave no doubt as to the organ in which they take place. As a rule, especially in well-fed rabbits, the movements begin in the colon and travel towards the small gut, that is, they are antiperistaltic in character. But frequently at the end of an antiperistalsis, after only a short interval, the wave returns and runs from the small gut towards the colon; in other words, the antiperistalsis is often followed by a peristaltic wave. The constriction is preceded by a bulging which is more marked than the former. The degree of the constriction (and bulging) is variable. Weaker waves sometimes do not finish the course. A complete course of a wave in one direction lasts from thirty to fifty seconds.

The average rate of the movements is about one per minute, but the rhythm is far from being regular. Various influences suppress cecal peristalsis. Ether applied through the nose stops the movements but they return in about a minute after the ether is removed. Pain, struggle and fright stop the movements; but they soon return again. The most striking effect, however, is the one caused by opening the abdomen: the peristaltic movements as a rule disappear completely and permanently.

The authors found that stimulation of the cecum by exposing it to abnormal conditions is capable of inhibiting its movements directly. Laparotomy abolishes the movements of the cecum by direct inhibition, assisted probably also by reflex inhibition. Cecal peristalsis ceases after cutting both vagi. Stimulation of the peripheral end of one vagus causes a tetanic contraction of the entire cecum, especially after destruction of the cord. Some of the above mentioned facts were demonstrated on an animal with destroyed cord.

Deglutition through an Esophagus Partly Deprived of its Muscularis, with demonstration: S. J. MELTZER.

The author demonstrated a dog drinking milk in perfectly normal manner against gravity from a bowl on the floor, although a large section of the path of deglutition was deprived of all muscle fibers. The author stated that he had completely removed the muscularis from the entire cervical esophagus of a number of dogs. On the day after the operation they drank milk and water like normal dogs. In these cases there were no muscle fibers for quite a long distance to do the slow work of pushing the liquids into the thoracic esophagus. They were apparently squirted through the cervical esophagus by a muscular force located anteriorly to the esophagus. That this force is not due to the constrictors of the pharynx was demonstrated by another experiment. In one dog, besides the removal of the esophageal muscularis, the middle and lower constrictors of the pharynx were cut and completely put out of function. This dog, also, drank without any difficulty the day after the operation. The throwing force is apparently exercised by the muscles of the mouth and tongue.

The function of deglutition is provided with a mechanism for a rapid squirting down of appropriate materials. As to which of the mechanisms comes into play in any specific case depends upon the nature of the material which is swallowed.

Immunity Against Trypanosomes: F. G. NOVY. (See proceedings of Section K of the American Association for the Advancement of Science, this volume, p. 693.)

On Secondary Transplantation of a Sarcoma of the Rat: SIMON FLEXNER and J. W. JOBLING.

The results of this series of experiments[2] show that secondary inoculation succeeds in a high percentage of the rats in which no visible metastases can be seen, and in which visible metastases, in the lungs chiefly, are present. These facts bear upon the view expressed by Sticker, that a primary tumor protects the body from the development of a secondary tumor until the period of metastasis arrives, and upon Ehrlich's negative results in secondary transplantations of a rapidly growing mouse carcinoma. The sarcoma studied by the authors is characterized by its infiltrative growth, but it increases far less rapidly than the most active of Ehrlich's tumors, and reaches, in relation to the size of the rat, no such large size as the latter does in proportion to the size of the mouse.

On Certain Chemical Complementary Substances: HIDEYO NOGUCHI.

A comparative study of complement and extract lysins under the same conditions, with numerous important results.

Effects of Experimental Injuries of the Pancreas: ISAAC LEVIN.

The author's results lead to the conclusion that the injuries of the pancreas that produce the gravest effect on the organism are those which cause the most serious interference with the circulation of that organ. To pro-

[2] Reference to the previous series was made in SCIENCE, 1906, XXIV., p. 766.

duce a fatal disease it does not suffice to interfere partly with the free secretion of the pancreatic juice into the intestines as in the first series of experiments, or to injure some of the parenchyma and at the same time allow the juice to secrete into the peritoneal cavity, as in the second series. The interference with the circulation must be such as to produce a lesion of the whole organ, so that not only will the organism be deprived of the normal function of the pancreatic cells, as after extirpation of the organ, but also every cell will become diseased and begin to act abnormally and injuriously to the organism.

The Pathology of Function: an experimental laboratory course: HAVEN EMERSON.

An outline of experimental procedures comprising a laboratory course at Columbia University, on some common disorders of function and the physiological methods of detecting them.

The Influence of Alcohol on the Composition of Urine: F. C. HINKEL and WILLIAM SALANT.

The data obtained by the authors are illustrated by the appended summary of results of a long experiment:

TABLE SHOWING THE INFLUENCE OF ALCOHOL (50 C.C. OF 50% OR 70% DAILY) ON THE COMPOSITION OF DOG URINE

Average Daily Output in Grams

	Fore period 6 days	Alcohol periods 50% 6 days	70% 7 days	After period 10 days
Total nitrogen ..	5.5856	4.9066	5.2846	5.2590
Total sulphur ...	0.3368	0.2553	0.2978	
Neutral sulphur .	0.0917	0.1035	0.1402	
Inorganic sulphur	0.2081	0.1334	0.1442	0.2187
Ethereal sulphur	0.0371	0.0185	0.0133	0.0067
P_2O_5	0.8016	0.5526	0.5730	0.6959
Chlorides	0.3872	0.3000	0.3210	0.3631

Spirochæta microgyrata (Löw.) *and Mouse Tumors:* GARY N. CALKINS.

The author described a tumor taken from the right fore leg of a female mouse. A piece of the tumor weighing about 1½ gram was ground up with normal salt solution (3 c.c. of solution per gram of tumor material) and this was injected under the skin of the neck in twelve white mice. The remainder was fixed in 10-per-cent. formalin and in Zenker's fluid. One tumor has appeared in the inoculated mice. Dr. Ewing described the tumor from sections as an adenoma with glandular characters of the thyroid. Necrotic areas are few in number and very small; mitotic figures are rare.

Sections of the tumor put through the Levaditi silver nitrate method reveal the presence of *Spirochæta microgyrata.* The spirochæte is not widely distributed, but may be found at various points in the tumor mass, especially in the few small vacuolar areas. It has the characters of the species described by Löwenthal in 1905 in a case of human ulcerated carcinoma.

On the Competency of the Venous Valves and the Venous Flow in Relation to Changes in Intra-abdominal Pressure: RUSSELL BURTON-OPITZ.

In these experiments on dogs, the author measured blood flow in a femoral vein by means of his new recording stromuhr[3] and suddenly raised intra-abdominal tension either by pressure with the hands upon the external surface of the abdomen, or by inflation of the cavity with air.

In both cases a retardation of the venous inflow was noticed, the degree of the slowing of the blood-stream being in accordance with the increase in the intra-abdominal pressure.

A more abrupt and decisive slowing of the blood stream occurred when pressure was exerted with the hands. It then became possible at times to produce not only a stoppage of the flow, but also a slight backward movement, such as can be accounted for by the stretching of the venous valves.

On Vaso-motor Nerves in the Pulmonary Circuit: RUSSELL BURTON-OPITZ.

Various carefully devised experimental procedures failed to reveal vaso-motor influences in the pulmonary circuit.

The Effect of Salicylic Acid upon Autolysis: L. B. STOOKEY.

The liver, kidney, spleen and muscle taken from dogs which had received subcutaneously

[3] This volume, p. 422.

doses of sodium salicylate (0.1 gram, in 1-per-cent. solution, per kilo of body weight) daily during a period of ten days, showed rates of autolysis greater than those observed in organs taken from normal dogs.

On the Synthesis of Protein through the Action of Trypsin: ALONZO ENGLEBERT TAYLOR.

The author subjected to tryptolysis 400 grams of protamin sulfate obtained from the spermatozoa of the striped bass. The resultant products, freed from sulfate and concentrated to the point of saturation of the solution containing them, were treated with 300 c.c. of a glycerol extract of livers of large soft-shelled California clams, which contain a strong, stable, tryptic enzyme. The mixture was treated with toluol and set aside in a sealed flask. This solution, which was clear at the beginning, gradually became opalescent, then cloudy and finally a white precipitate settled out. This mixture was found to contain a large quantity of protamin. The author presented numerous chemical facts in support of his conclusion that protamin was regenerated in this mixture from its non-protein hydrolytic products.

A Method for Separating Leucin from Amino-valerianic Acid: P. A. LEVENE.

Separation of leucin from amino-valerianic acid was accomplished by means of lead acetate and ammonia. A basic lead salt of leucin, insoluble in hot water, was formed. From a mixture containing 52.53 per cent. of C and 9.39 per cent. of H, by the use of these reagents, a substance was obtained which had 54.55 per cent. of C and 9.90 per cent. of H. On reprecipitation it acquired the composition: C = 54.70 per cent.; H = 10.09 per cent. Leucin contains 54.89 per cent. of C and 10.01 per cent. of H.

WILLIAM J. GIES,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 427th meeting was held March 9, 1907, with President Stejneger in the chair. The following communications were presented:

J. W. Gidley: 'A New Horned Rodent from the Miocene of Kansas.' Illustrated with lantern slides.

W. H. Osgood: 'Notes on European Zoological Gardens.'

C. L. Pollard: 'Dictionaries in their Relation to Biology.'

THE 428th meeting was held March 23, 1907, President Stejneger in the chair. Dr. C. W. Stiles read a paper on 'A Reexamination of the Type of *Filaria restiformis*, an Alleged Parasite of Man.' He exhibited the original specimen of *Filaria restiformis*, described by Leidy in 1880. A reexamination of this type has developed the fact that the worm is not a *Filaria*, but a member of the family Mermithidæ. It was certainly not a parasite in the genito-urinary apparatus of man as originally described and, in all probability, was not a parasite of man at all. The original material is in the Army Medical Museum, Washington, D. C. As it can not be determined generically, Dr. Stiles has proposed to place it in the collective group *Agamomermis* as *A. restiformis*. A more detailed account together with drawings of the original specimen will appear later.

Mr. Lyster H. Dewey read a paper on 'The Zapupe Fiber Plant of Eastern Mexico.' Zapupe is a name applied to two species of Agave cultivated in the states of Tamaulipas and Vera Cruz, Mexico, and also to the fibers produced by these plants. Many large plantations comprising a total of more than 4,000,000 zapupe plants have been set out during the past four years. Although these plants have been called Tamaulipas henequen and Huasteca henequen, they are quite distinct from the henequen plants of Yucatan. Both have rigid straight leaves 1 to 2 m. long, but they are narrower, thinner and more numerous than henequen, *Agave rigida elongata* (Jacobi) Baker, or sisal, *A. rigida sisalana* Engelm. Both have small marginal up-curved spines. One, called 'Zapupe verde,' has light green leaves and very sharp terminal spines grooved at the base. The other, called 'Zapupe azul,' has bluish glaucous leaves and terminal spines, usually with an irregularly diamond-shaped flattened area on the face, but without groove or channel. Zapupe azul, in habit, form of leaf and spines seems to agree perfectly with Tequila azul, *Agave tequilana* Web., cultivated extensively in the region of Tequila, Jalisco, for the production of 'Tequila wine.' The plant is not used for the production of liquor in eastern Mexico. It is said to have been introduced in that region and its origin seems uncertain. Zapupe verde has long been cultivated for fiber by the Indians of Tautoyuca, Vera Cruz. The zapupe fiber morales made by these Indians are among the finest to be found in Mexico. This plant may be *Agave angustifolia* Haw. which has been referred somewhat doubtfully as a synonym of *A. rigida.*

The fibers of both species of zapupe are very similar in character. They belong to the sisal group among the hard fibers used for twines and cordage. They are finer, and more flexible than either Yucatan or Bahama sisal, approaching the better grades of Bahama sisal in general character. In a test for tensile strength they compare favorably with the better grades of sisal.

M. C. Marsh,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

THE FIRST SPECIES RULE

I have read with a great deal of interest all that has been more recently published in Science on this topic, because the adoption of and strict adherence to the rule making the 'first species' the generic type will make about as many changes in the nomenclature of the lepidopterous family in which I am especially interested as can well be crowded into it. It will bring up names that have dropped out of use for fifty years and it will completely change the conceptions of a large number of genera that have been in common use for nearly or quite as long a period.

I was particularly interested in the essay by J. A. Allen in the April 5 number of Science and especially in the following, on p. 548:

> Of course, an author often states that certain species are referred to a given genus provisionally, or are given as doubtfully belonging to it. In all such cases the rules of our standard codes prohibit the taking of any such doubtfully referred species as the type of a genus.

Ordinarily when an author characterizes a genus he has some definite idea that represents his genus—a combination of structures which, taken together, make his generic conception. Whenever there is any change in this association by extension or limitation the genus as first proposed is no longer in existence. As limited or enlarged the association of species represents the conception of the person that limits or extends.

In 1890, in a revision of the species listed under *Agrotis* in our catalogues, I proposed the name *Rhynchagrotis* for an assemblage most prominently characterized by a palpal structure that bore a resemblance to a short snout or beak. There were other characters as well and the combination of those characters made up my genus for which no type was designated.

Among the species referred to this new association was *Agrotis chardynii* (*gilvipennis* Grt.), an oddity in our fauna, standing by itself and differing markedly from all our other forms. It did not really agree with my definition of *Rhynchagrotis* and so I stated; my reason for placing it there being that I believed it would prove to be properly referable to an exotic genus to which I did not care to risk making a synonym.

In accordance with my usual practise in revisional work I prepared a table of species, and for convenience in tabular arrangement I usually separate the oddities first. Thus, *chardynii* being the only one of our species with yellow secondaries was the first to be excluded in the synoptic arrangement, and the list of species described under *Rhynchagrotis* begins with that name.

Recently, Sir George Hampson, in his monumental catalogue of the Phalænæ in the British Museum, treated the Agrotids in his Volume IV., and as his basis for generic combinations did not coincide with mine, there were some shifts. Among others my association under *Rhynchagrotis* was broken up, and of all species in the world *chardynii,* which I felt sure could not remain in it, has now become the 'type,' because it happened to head the list. The name still is *Rhynchagrotis* Smith; but any student who attempts to identify the Smith genus as it stands now, from the Smith description as it was written, will inevitably fail to understand how Smith could have written up such an inapplicable set of characters for his genus. My genus no longer has any existence, though the name proposed by me remains to represent a set of characters specified by Hampson.

It does seem to me as if, when an author has recorded a given set of characters as representing his conception of a genus, any arbitrary rule that limits his generic term to any species or set of species that does not include that combination is both illogical and unscientific. It seems like holding to the letter to avoid an inquiry into the spirit of truth.

JOHN B. SMITH

NEW BRUNSWICK, N. J.,
April 22, 1907

THE ANTHROPOLOGICAL EXHIBITS IN THE AMERICAN MUSEUM OF NATURAL HISTORY

TO THE EDITOR OF SCIENCE: The communication in your issue of April 12, by Dr. Dorsey on 'The Anthropological Exhibits in the American Museum of Natural History' suggests the important question: For whom should the public exhibits in such museums be designed? Should they be for scientists, for college students or for the general public?

The needs of these three groups are so very different that it is quite evident that the same style of exhibit can not be satisfactory to all. If designed for the public the exhibit should come within its comprehension and should lead in a definite manner to a general appreciation of some of the more important features of the subject; for it is to be presumed that the public will see little beyond that which is prepared for them. If they are to obtain definite ideas it is best that the exhibit aim to impart a limited number of fundamentals rather than lose itself in a multitude of details. In other words, effectiveness is dependent on concentration in aim and in limiting the number of objects shown. It is unavoidable that such an exhibit should partake somewhat of the character of a text-book illustrated by specimens, though it is probably advisable to disguise as far as possible the mechanism of this; for people like better to think they are discovering facts and principles than that these are forced upon them. However, if any considerable portion of the public is to be guided aright it is necessary that the text-book character of the labels shall be at least pronounced enough to be discernible to the trained specialist and consequently to be offensively kindergartenish to him if he imagines that the exhibit was made for him.

An exhibit designed for students having had the advantages of text-book and oral instruction would needs be more advanced, less explanatory, and with a greater wealth of detail.

For an advanced specialist an exhibit of all the material in the museum, each specimen accompanied by its field label, would probably be as satisfactory an arrangement as could be made in exhibition cases; but I am very certain that most anthropologists, like mammalogists and ornithologists, would prefer to have the specimens in trays in storage cases where they could be handled and minutely examined.

The exhibits in our museums twenty or twenty-five years ago were largely of a character that reached no class of people as they should be reached; but catered principally to naturalists. Those were the days when the exhibit expressed what the official occasionally put into words: "The public be d———." Within a very few years it seems to have come to most museums that they were on the wrong track; that their exhibits were not conducive to the best use of the specimens by naturalists and that they utterly failed to reach the public. The keeping open to the public of the halls of a large museum is a matter of great expense, justifiable only on the ground of public instruction, and quite uncalled for if the exhibits are not intended for them.

Most museums are supported to a considerable extent by their communities and therefore the taxpayer has a right to demand that something be done for him; and every fair-minded museum director will see to it that he receives considerate treatment.

If it is conceded that an exhibit for the specialist is of practically no educational value to the public—is to it primarily a collection of meaningless curios—then it is patent that museums drawing largely on public funds can not honestly adopt such an arrangement, but must follow one that will give a fair equivalent for value received.

Museums that may be privately endowed so as to be independent of public funds may, of course, adopt any method they choose; but it would appear to be a foolish waste of energy and money to throw open to the general public a specialists' museum.

The college student is not here considered because his own institution makes special provision for him.

Looking from the standpoint of popular education at the ethnological exhibits of the American Museum, I have been greatly pleased to note the decided advance that the last few years has shown in the matter of arrangement. I can imagine a man of average education, with no special knowledge of the Eskimo or the Plains Indian, viewing those exhibits for an hour and coming away with a fair general idea of the peoples represented, such as he could not possibly have acquired in many hours under former conditions. This man will represent more than ninety per cent. of the visitors to the museum. He it is who is paying a considerable proportion of the expenses of the museum and now is getting his dues. At the same time I presume that the specialist can be amply cared for in this department, as I know to be the case in certain zoological branches.

I hope that there will be no backward step to the condition of storage exhibition with, to the public, its meaningless repetition of specimens that have little or no information to convey.

Henry L. Ward
Public Museum, Milwaukee,
April 15, 1907

MAGAZINE SCIENCE

The science in the magazines is not always bad. Much of it is not only most illuminating to the non-specialist mind (including in the term all those who are specialists in some one subject, but whose college knowledge of all other subjects is wholly antiquated), but is also of the highest order of authority. For instance, the March number of the *Century* contains a very important article by Professor Hugo de Vries on the work of Luther Burbank, and in the May number of the same magazine there appeared one by Professor Stratton on railroad signaling in connection with color-blindness. But the article on color in the number for April belongs to the class of the antiquated and the non-scientific to a degree that has become, fortunately, most unusual. Criticism of an article like this is not worth while, but one can indicate its character by a few quotations. We are told that "two tuning forks of discordant rates of vibration, set in action close together, will make no sound" (as if vibrations of exactly opposite phase were the only ones that give discordant notes); and that "it is possible that the harmonies of color waves may some day be reduced to mathematical tabulation." The writer believes in the 'capacity of brain cells to note rhythmic variations' of various degrees of speed; he affirms that "the brain receives impressions in the form of waves of vibration," and also that "two kinds of light waves are emitted from all objects, color waves and white waves."

After this one is not surprised to find that he thinks there are red-blind individuals who see green, and green-blind individuals who see red, and that, in fact, all the knowledge about color that has been gained in the last twenty years or so is *terra incognita* to him. It seems a pity that three full pages of bright colored illustration should be wasted in propagating error. And this is an article which the New York *Evening Post* took the trouble, upon two separate occasions, to praise! It is said that the *Youth's Companion* employs a reputable scientist whose sole duty is to see that no patently false science, or other matter of fact, appears in its columns. It would be wise if less modest journals followed the same plan.

Christine Ladd Franklin
Johns Hopkins University

SPECIAL ARTICLES

THE ETHER FREEZING MICROTOME, IN BOTANICAL TECHNIQUE

THE following description of a freezing microtome, and its advantages, is published in the hope that others will derive such benefits and conveniences as I have for some time enjoyed through its use. The microtome is exceedingly simple, and so easily manipulated that an inexperienced person may quickly learn to operate it successfully. It is very rapid in its work, allowing of sections even in less time than is sometimes required for free-hand sections, and does very efficient work. In some kinds of material the sections are superior to those obtained by the paraffine method. It is useful in dealing with a great variety of objects, and is cheap enough to be within the reach of all.

The freezing method of embedding in section work is well known to all biologists, but owing to the use of faulty or cumbersome apparatus, or to the application toward inappropriate ends, it has not become as general in use as it should be. Osterhout[1] found the freezing method absolutely necessary in the anatomical study of the red sea-weeds. No other freezing apparatus with which I am acquainted combines the good features of the machine described below, and I would especially call the attention of all scientists who have used or are using freezing microtomes to these features. The freezing device is exceedingly simple and effective. The knife carriage is also very simple, is accurate in its work, and although very rigid allows of sectioning on any part of the knife, and at almost any angle, vertical or horizontal with little or no delay in adjustment. This latter feature is especially valuable.

It should, first of all, be explained that it is not the intention of the writer to recommend this method as a substitute for the paraffine method. Messrs. Hill and Gardiner, however,[2] have developed this freezing microtome technique in the examination of 'connecting threads' of pine tissues to a high degree of proficiency, and it is to be hoped that their results will be more widely applied. It is not, however, in the study of cytological problems that I have found the technique of most use—though remarkable results have been obtained by Hill and Gardiner—but it is in its use in other lines that it has proved of very great value. The method is capable of application in so many ways, and for such a great variety of purposes that an enumeration of these is deemed advisable. It is chiefly useful for a great deal of work where paraffine sectioning is too slow, and where free-hand sections are difficult to obtain; for example, in sections of certain rust pustules. A great deal of time can often be saved by preliminary sectioning with the freezing microtome before using the paraffine method, in order to determine the condition of the material about to be used. The freezing method can be so developed as to give sections as quickly as, or even in less time than, the free-hand method. The sections, moreover, can be produced in much greater number and are far superior in thinness and uniformity, and in certainty of success. Material can be frozen in eight seconds with an apparatus in good order, so that the embedding can be accomplished in less time than is required for the insertion of the same in pith for free-hand work.

Every mycologist knows how difficult it is to get sections of most fleshy fungi and how almost impossible sections of sporophores of the Tremellineæ and teleutospore clusters of *Gymnosporangium* are, yet these are cut with greatest ease by the freezing method. It is possible to cut a gelatinous sorus of *Gymnosporangium macropus* from tip to bottom together with the hard wood from which the sorus arises, and to preserve it all intact. This seems to me impossible by the free-hand method, and the dehydration in the paraffine method leaves the material in an unsuitable condition for section work. Again, objects of large size can be cut easily, such as small phalloid 'eggs,' entire caps of small agarics, small earth stars, etc., which, if cut at all by the free-hand method would give uneven sections, unless the manipulator is a person of extraordinary skill. I have found

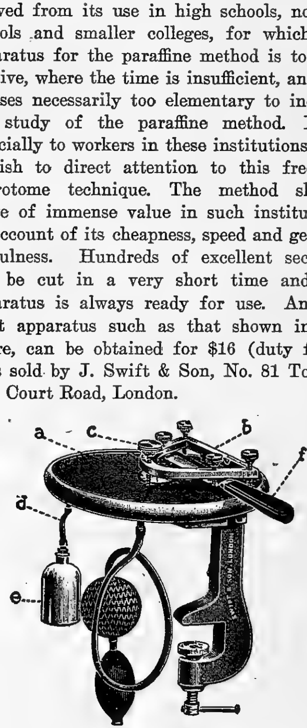
[1] *Bot. Gaz.*, **21**: 195, 1896.

[2] See *Philosophical Transactions of the Royal Society of London*, Series B, Vol. 194, pp. 83–125.

the method of great use in sectioning rust pustules, from both fresh and dried material. It is also of very great value in the study of hard seeds, such as the grains of cereals. The paraffine method, at least as usually practised, is not successful in producing sections of the starchy grains of cereals. Dr. A. Mann, of the Department of Agriculture, however, informs me that he has succeeded by very long infiltration of both xylol and of paraffine in obtaining very satisfactory sections of mature barley grains. By the freezing method excellent thin sections of whole grains (even of large corn) can be cut without difficulty. On the other hand, the method is very useful in cutting hard materials, as small blocks of wood (soaked in water) grass stems, etc. This is made possible by the firmness of the mount, and by the direct and unswerving approach of the knife, which avoids the turning action of the blade, so common in the cutting of free-hand sections from such material. In dealing with such material, moreover, by the paraffine method, dehydration makes the wood so hard that the knife is very easily ruined. In the freezing method, the blocks, on account of their water-soaked condition, are less apt to injure the knife. Again, large soft berries, as cranberries, are easily sectioned whole, and for rapid sectioning of leaves the method is particularly useful. Osterhout's work (*l. c.*) has demonstrated its usefulness in the treatment of red sea-weeds. The method is thus seen to have a wide range of usefulness.

The freezing microtome method should find a place in every botanical laboratory, first as a supplementary aid to the paraffine method, and second, as a quick method of securing sections for ordinary morphological and anatomical work. It sometimes happens that an instructor can obtain only a small amount of material, which may be extremely rare, and by this method he can usually obtain enough excellent sections for a large class in a very short space of time, without the delay necessary for paraffine work, and without the waste of material which is sure to attend the work of sectioning when done by the students. Again, in elementary classes, microtome work of a difficult nature is sometimes necessary, and the students may be unable to obtain satisfactory free-hand sections. In mycological and pathological laboratories the method is extremely useful. It has, moreover, been found of very practical value in medical pathological laboratories, where it is used in rapid histological work.

Perhaps the greatest general good could be derived from its use in high schools, normal schools and smaller colleges, for which the apparatus for the paraffine method is too expensive, where the time is insufficient, and the courses necessarily too elementary to include the study of the paraffine method. It is especially to workers in these institutions that I wish to direct attention to this freezing microtome technique. The method should prove of immense value in such institutions on account of its cheapness, speed and general usefulness. Hundreds of excellent sections can be cut in a very short time and the apparatus is always ready for use. An efficient apparatus such as that shown in the figure, can be obtained for $16 (duty free). It is sold by J. Swift & Son, No. 81 Tottenham Court Road, London.



The apparatus is shown in the accompanying figure. The material to be cut is first placed in a ten per cent. gum-arabic solution. It may be soaked in this for one to twenty-four hours, according to the size of the material. It can, when necessary, be kept in the gum arabic for only a few minutes or even a few seconds, though the longer soaking will give a better infiltration. It is then oriented in a drop of ten-per-cent. gum arabic placed on the small brass plate (*a*) in the center of the table, and a spray of ether is atomized against the corrugated under surface of this plate, producing the necessary low temperature. The atomizer is usually worked by hand with a double bulb, but I have found a compressed-air tank (such as physicians use) a very great convenience, as such a tank allows of the unhindered use of both hands in the microtome work. It also admits of more rapid freezing. I have been able with this apparatus to freeze material in eight seconds on a warm summer's days in Washington, D. C. A foot pump may also be used in place of the double bulb. The knife (*f*) is carried by a shoe (*b*) and is held in place by adjustable screws. The shoe is supported by three bone-tipped adjustable screws, the forward one of which (*c*) is used to set the knife after each stroke in preparation for the next one. The screws rest on a plate-glass top, which covers the table around the central brass plate. The smoothness of motion is facilitated by oil placed on the plate. The atomizer is of the ordinary type. The intake is shown at (*d*), and the ether bottle at (*e*). The ether should be of good quality (that used in medicine for anesthesia) in order to obtain the best results.

The gum arabic may be kept in stoppered bottles, and can be preserved from mold and bacterial attacks by adding a few crystals of carbolic acid or thymol. The sections after cutting can be handled in the ordinary way with section lifters or with small sieve nets of cloth or other substance. The latter method is very useful if the sections are to be transferred to stains and afterwards washed. Very delicate sections may also be handled by means of a loop of fine platinum or brass wire. The sections are caught up in the water drop and are easily transferred to other dishes or to a slide without the injury which is liable to occur in handling with ordinary section lifters. The sections may be mounted in glycerine or glycerine jelly, and can then be permanently mounted, without having touched alcohol if water stains are used. As Hill and Gardiner point out, the dehydration of sections in alcohol may leave protoplasmic structures in a condition very different from the normal. Of course, the effect of freezing is also one which must be taken into account, though this is seldom, if ever, a serious factor in the morphological and anatomical work for which this method is here recommended. Sections for ordinary anatomical work can be cut from fresh material, or from dry material after soaking in water. The material may also be killed by the ordinary methods, preferably without the use of alcohol, and may then be washed in water in the usual way and preserved indefinitely in a concentrated thymol solution. Such material can be prepared for the knife simply by washing carefully in water.

I wish to acknowledge that my acquaintanceship with the possibilities of the microtome described above was made in the Cambridge (England) botanical laboratory, and I am indebted to Mr. A. W. Hill, of Cambridge University, for many courtesies and favors in my observations and study of this method.

E. M. FREEMAN
U. S. DEPARTMENT OF AGRICULTURE,
WASHINGTON, D. C.

ASTRONOMICAL NOTES

THE YALE PARALLAXES

Transactions of the Astronomical Observatory of Yale University.—Dr. W. L. Elkin, director of the Yale Observatory, undertook, in 1884, by means of the heliometer, the determination of the parallaxes of the ten stars of the first magnitude in the northern sky. This work was carried out with rare ability and success during the following ten years; but before the completion of this work, it was decided to extend the research by undertaking a survey of all rapidly moving stars not previously attempted, with a view to singling out those which are near enough to show a measurable parallax. This work has been carried on during the last thirteen years, and the results have been recently published as Volume II., Part 1, of the Observatory Transactions, under the title, 'Parallax Investigations on 163 stars mainly of Large Proper-

Motion.' The greater part of the observations were made by Dr. F. L. Chase, assistant astronomer, and a smaller number by Mr. M. F. Smith, assistant, and Dr. W. L. Elkin, the director.

It was thought at first that a small number of observations at each of two successive epochs of maximum parallactic effect would certainly show if the parallax amounted to as much as 0″.20, and give some indication of a value as small as 0″.10. Later the plan was extended to include two more epochs taken in reverse order. The results were made to depend wholly on measures of distance, and in general two comparison stars were selected on opposite sides of the star whose parallax was sought.

Systematic personal error, due to the direction of the stars, or to differences in color or brightness, was avoided by the use of a reversing prism eye-piece. Also, gauze screens were used to equalize the brightness of the stars to within half a magnitude. For each star in most cases twelve complete observations were made. Every precaution to eliminate known sources of error was employed, so that the authors have good reason to believe the results to be free from systematic error, except, perhaps, one due to color. Such an error seems to be theoretically possible. In order to ascertain whether an error due to this cause is appreciable in actual observations, Dr. Chase made a series of observations on five highly colored stars. The results appear to indicate, that there is a discernible color effect, which is in accordance with theory; but the errors involved are so small that even in extreme cases, they come within the probable errors, never amounting to more than 0″.03.

All the observations were given equal weight. The effect of poor definition seems to have been inappreciable.

A reliable estimate of the systematic errors of the stars in general is obtainable by a comparison of the Yale parallaxes with those deduced by other able observers. A table of twelve such stars is given from which the average difference between the Yale values and the others amounts to 0″.036, from which, assuming equal accuracy for both results, the total probable error of each is ± 0″.017.

The authors do not claim great precision for the individual results, but attach importance to the mean values of various groups which they formed. With the addition of the ten stars of the first magnitude previously determined by Elkin, the number of stars considered is 173, from which five groups were formed. In each group the stars are arranged as indicated in the following mean results.

TABLE I

Results arranged according to Proper Motion

P. M.	Par.	No. Stars.	Mean Magn.
0″.14	+0″.019	21	3.8
0.49	+0.032	39	6.3
0.59	+0.059	45	6.7
0.77	+0.039	46	6.5
1.50	+0.109	22	6.2

TABLE II

Results arranged according to Stellar Magnitude

Mean Magn.	Par.	No. Stars.	P. M.
0.8	+0″.095	10	0″.61
3.8	+0.066	29	0.53
5.6	+0.056	33	0.63·
6.7	+0.045	34	0.73
7.6	+0.017	31	0.68
8.3	+0.047	36	0.80

TABLE III

Results arranged according to Size of Parallax

Par.	No. Stars.	P. M.	Magn.
—0″.110	7	0″.55	7.3
—0.025	29	0.52	6.4
+0.031	66	0.62	6.8
+0.097	44	0.79	6.1
+0.159	17	0.97	6.3

TABLE IV

Results arranged in order of Right Ascension

R. A.	Par.	No. Stars.	Magn.
h h 0– 3	+0″.074	2.2	7.1
3– 6	+0.056	2.0	5.5
6– 9	+0.047	1.8	6.2
9–12	+0.021	2.2	6.2
12–15	+0.046	1.7	6.4
15–18	+0.078	2.3	6.4
18–21	+0.039	3.0	5.4
21–24	+0.030	2.1	6.3

TABLE V

Results arranged according to Stellar Spectra

Class.		Par.	No. Stars.	Magn.	P. M.
Type I.	A	+0″.065	13	4.0	0″.42
Type II.	E	+0.068	12	6.4	0.71
	G	+0.125	4	4.0	0.69
	I	+0.030	5	5.5	0.67
	F	+0.079	30	4.7	0.66
	L	+0.040	1	4.1	0.11
	H	+0.023	24	6.5	0.65
	K	+0.054	5	1.9	0.88
Type III.	M	+0.007	3	2.1	0.22
	Q	+0.040	2	2.9	0.02

Table I. shows that there is, as might be expected, a distinct relation between parallax and proper motion. Not only are there striking individual exceptions to this law, however, but the group having a mean proper motion of 0″.77, with a mean parallax of +0″.039, destroys the continuity of the series.

In Table II. may be traced some relation between magnitude and parallax. This comparison would have great interest, had the selection of the stars been differently made. As it is, only the first group of ten stars were chosen with reference to their brightness, while all the rest were selected because of large proper motion, that is, in a general way, because of their nearness. Only a hint can therefore be obtained as to the real relation between the magnitudes and parallaxes of the stars as a whole. The table shows that in general bright stars are nearer than faint ones, though even this obvious truth is apparently refuted by the last two groups, which make stars of mean magnitude 8.3 much nearer than those of magnitude 7.6. It must not be inferred that the actual selection was unwise. Any other selection than that employed would probably have led for the most part to negative results. The authors made the best of an extremely difficult problem, perhaps the most difficult in the whole realm of observational astronomy.

Of Table III. the authors say: "This table may also serve to indicate the number of spurious parallaxes belonging to the work. If, according to Newcomb, we regard all the negative results as due to errors of observation, and likewise an equal number of positive values to balance these, it would seem that all seventeen of the group with parallaxes between +0″.14 and +0″.20 are real, 38 of those from +0″.07 to +0″.13 and 35 of those under +0″.06. Thus there are 90 stars of the entire list of 163, for which there is considerable presumption that the parallax values found are actual."

Tables IV. and V. appear to lead to results of small importance so far as distribution is concerned.

Finally, a summary is given for the different groups, except for Table III., of the average total stellar velocity relative to the sun, and of the luminosity relative to the sun. In this summary the greater luminosity of the brighter stars is strikingly shown.

Too high praise can hardly be given to these parallax investigations, carried on during so many years. Yet the results, though of great value in themselves, do not encourage the hope that by similar heliometer observations we shall ever gain a knowledge of the distances of any large number of stars, especially of those most distant. It is doubtful if we have, at the present time, any mode of research sufficiently refined to determine the parallaxes of the most distant members of our sidereal system. The quantities involved are too small. By more powerful instruments, especially by photographic telescopes of great focal length, it may be possible to determine smaller values than those yet found. The relation between proper motion and parallax offers a hopeful means for the determination of mean values, but this method has limitations. The relation between magnitude and distance is as yet uncertain. Indeed, the solution of the most difficult parts of the problem calls for some new means of research far more powerful than any known at the present time.

S. I. BAILEY

HARVARD COLLEGE OBSERVATORY

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE PLATTSBURG MEETING OF THE SECTION OF GEOLOGY AND GEOGRAPHY

SECTION E will hold a summer field meeting, July 3–10, 1907, in New York State in the region between Lake Champlain and the Adirondacks.

Dr. Lane, the retiring vice-president, will deliver his presidential address during the meeting. Several informal talks will be given on subjects germane to the field excursions, during the half hours after lunches. Papers may be expected from Professors Fairchild, Davis, Kemp, Cushing, Woodworth, Hudson and others.

Dr. John M. Clarke will give a paper on 'Lake Champlain' including an account of the work of the state in the Champlain Valley which is classic ground in the history of American geology. The region is the field of three great wars and many bloody encounters before them and the records of these events are locked up in some measure in the place names of the country. The section owes much to Dr. Clarke for his careful planning of the meeting so that visiting geologists will see as much as possible of this extremely interesting region.

The program of excursions is as follows:

Wednesday, July 3.—Preliminary trip. Preceding the first day of the regular trips Professor Woodworth will conduct those who desire to visit 'The Gulf' at Corey Hill, Canada, to that locality, passing over the marine beaches along the international boundary. Persons intending to take this trip should reach Mooers the night before. This party will join the other members at West Chazy for the Altoona excursion. As this trip must be made in wagons, the number is limited to twenty-five persons.

Thursday, July 4.—Trip to Altoona, Mooers Junction. Plattsburg to West Chazy by train; drive to Cobblestone Hill; Altoona spillway on Potsdam sandstone; to Altoona delta; thence down the Big Chazy to study the fossil shore lines, to Mooers, where the party will spend the night. Only twenty-five persons can be accommodated in the wagons; others may walk four and one half miles from West Chazy to Cobblestone Hill and the Altoona spillway. Guide, Professor Woodworth.

Friday, July 5.—From Mooers Junction to Chazy. The Chazy limestone in its varying aspects and its faults. If this trip is to be made it will be necessary to start in the morning, as there is no other train between the two places. Return to Plattsburg in the evening, to Hotel Champlain or the Catholic Summer School. Guides, Professor Cushing and Dr. Ruedemann.

Saturday and Sunday, July 6 and 7.—By steam-launch to Crab Island, Valcour Island and Valcour shore. Extensive displays of Paleozoic sediments with interesting structural features. Return Saturday evening to spend Sunday at Bluff Point, at Hotel Champlain, or the Catholic Summer School. Interesting exposures of the Trenton faulted down against the Chezy and of monchiquite dikes in the limestone, within easy walk of the hotel. Guides, Professor Cushing, Dr. Ruedemann and Professor Hudson.

Monday, July 8.—Plattsburg to Lyon Mountain by rail. The entire day to be given up to the examination of the magnetite mines. It may be possible to make stops at interesting localities: Dannemora, Cadyville, but this will depend upon convenience. Return to Plattsburg. Guide, Mr. Newland.

Tuesday, July 9.—Keeseville; Anorthosite and Potsdam conglomerate; north slope of Trembleau Mountain, Marine delta, and higher lake shores; thence to Ausable chasm cutting the Potsdam sandstone since the retirement of Hochelogan Sea. Return in the evening to Plattsburg. Guides, Professors Woodworth and Cushing.

Wednesday, July 10.—Plattsburg to Port Henry and Mineville iron mines, or to Ticonderoga and the graphite beds. It is not likely that both trips could be made on the same day. Each one will choose which excursion he will take. The Port Henry and Mineville trip will be under the guidance of Mr. Newland, the Ticonderoga trip under the guidance of Professor Kemp.

Thursday, July 11.—If any of those who attend the meeting would like to spend another day in visiting points of interest on Lake George on the way home, Professor Kemp has kindly consented to act as guide for this excursion.

There will be a winter meeting of the American Association in Chicago, when there will be abundant opportunity for members of the section to present papers. The Plattsburg meeting is one primarily for field excursions.

The Hotel Champlain, charmingly situated overlooking the lake, will accommodate 300 to 400 persons. The rate will be about four dollars a day.

The Champlain Assembly, incorporated as the 'Catholic Summer School of America,' has invited the members of Section E, through its director, Mr. John B. Riley, to be its guests during the meeting. Rooms may be secured at one dollar a day in the buildings of the Champlain Assembly. Members may take their meals at the Champlain Club; breakfast, lunch, or supper, fifty cents; dinner, seventy-five cents. The grounds of the Champlain Assembly are three miles south of Plattsburg and less than half a mile from the Hotel Champlain. Both may be reached from Plattsburg by steam-train or trolley.

A circular will be sent about June 15 to those who plan to attend the meeting. This will give information in regard to railroad rates. The summer excursion rates will doubtless make it possible to secure round-trip tickets for a little more than one and one third single fare.

The sectional committee of Section E extends a cordial invitation to all members of the Geological Society of America and the Association of American Geographers to attend the Plattsburg meeting.

F. P. GULLIVER,
Secretary Section E

NORWICH, CONN.,
April 25, 1907

THE LEICESTER MEETING OF THE BRITISH ASSOCIATION[1]

THE British Association is assured of a hearty welcome to Leicester for its seventy-seventh annual meeting to be held there from July 31 to August 7, under the presidency of Sir David Gill, K.C.B., F.R.S. Leicester is a place of great antiquity, few towns in England having a longer history of uninterrupted activity. Its Roman remains include the 'Jewry Wall,' a remarkable example of brick-work, and some mosaic pavement *in situ*. The geological features of the district are comprehensive, the Charnwood Forest, with its rocks providing many a geological puzzle, being within a few miles of the town. Botanists, too, have a happy hunting-ground there. The local committees and sub-committees are working hard to insure the success of their efforts, and great interest is being shown on all sides in the visit of the association to Leicester. A guarantee fund of more than 3,300*l*. has been raised towards the necessary expenses of the welcome, and this without any public appeal being made. No less than eleven amounts of 100*l*. and upwards are included in this sum.

A call has been made on all the principal halls and public buildings throughout the town for general and sectional use, and it is believed that the arrangements when completed will be most satisfactory in every way. The greatest difficulty the executive committee have had to meet has been the fact that Leicester possesses no town hall or public building large enough for the purposes of the holding of the usual conversazione and general reception of the large number of members and guests anticipated. An ingenious suggestion, however, on the part of the chairman of the executive committee (Mr. Alfred Colson), which has met with the full approval of all concerned, promises to overcome all obstacles, and even to make the proposed conversazione additionally attractive on account of the unique way in which it will be housed. The intention is to utilize the whole of the present museum buildings, including the art gallery and mayoral reception rooms, for the use of which permission has been granted, and to erect on the four sides of the grass square adjoining a loggia or corridor constructed entirely of timber, 25 feet in width, forming a covered promenade about 500 feet in length. The four outer sides will be closed, but the inner sides, overlooking the grass-plot, will be open, and so constructed as to be easily beautified with floral decorations. Internally the loggia will be draped with incombustible material and fitted with electric light and suitable furniture. Besides answering for the reception to be given by the Leicester Literary and Philo-

[1] From *Nature*.

sophical Society, the structure and grounds, with a military band in attendance, will make a convenient general rendezvous throughout the week.

A further edition of a very interesting work, 'Glimpses of Ancient Leicester,' by a local author, Mrs. Fielding Johnson, is being issued in connection with this meeting, and a handbook by another Leicester lady, Mrs. Nuttall, will be provided. The latter book will contain chapters on subjects of scientific interest prepared by various experts specially for the use of visitors.

Excursions are being arranged to many points of interest in the district, and the Mayor, Alderman Sir Edward Wood, J.P., will issue invitations to an evening fête in the Abbey Park. Sir Samuel Faire, J.P., will give a garden-party, and it may be taken for granted that the social side of the meeting will be well provided for. The comfort and enjoyment of all attending the meeting will not be overlooked, while the objects of the existence and visit of the association will throughout the week have the first consideration and thought.

THE AMERICAN MUSEUM OF NATURAL HISTORY

THE American Museum of Natural History, New York, will hereafter be open to the public free of charge at all times. Mr. Morris K. Jesup, president, has addressed the following letter to Dr. Hermon C. Bumpus, the director of the museum:

From the time of the founding of the American Museum of Natural History—now nearly forty years ago—certain days of each week have been reserved for 'members,' pupils of the public schools, special students and artists, the public being admitted on these days (Mondays and Tuesdays) only on the payment of an admission fee. Although this is almost a universal custom, I am convinced that its continuation by the American Museum is now of doubtful expediency.

This reservation really amounts to the closing of our doors to the public for approximately one-third of the time, and while, for reasons of economy, it may have been necessary during our early history, we should not forget that the American Museum is a municipal institution, it is primarily for the public, and any regulation that interferes with the general enjoyment of its privileges is contrary to the spirit of its founders and opposed to the wishes of its supporters.

The growth of the museum during the last few years has made it possible to accommodate large bodies of school children without interfering materially with the use of the exhibition halls by adults. Special students are now given laboratory facilities well removed from visitors, and artists have long found every encouragement for their work in private rooms or in portions of the gallery temporarily partitioned off for their use.

It is true that free admission to the museum on Mondays and Tuesdays has long been enjoyed as one of the many 'privileges of membership,' but I thoroughly believe that those who have contributed and are contributing towards the support of this institution are not actuated by selfish motives; they contribute because they believe in the work that the museum is doing and because they derive pleasure from being associated with it.

I wish therefore—and in this I believe I have the support of the entire board of trustees—that until further notice arrangements be made to have the American Museum of Natural History open daily and its exhibition halls absolutely free to all.

In forwarding a copy of this letter to members of the museum, the director says:

The enclosed letter of President Jesup will meet the hearty approval of all interested in the welfare and development of public educational institutions.

When the American Museum was relatively small, and the exhibition halls were consequently often overcrowded, there were valid reasons for restricting the attendance on certain days, but with the recent growth and the provision of special facilities for students and others specially interested, these reasons no longer exist. The attendance is constantly growing, and it is a matter of common remark that those visiting the museum are seriously interested. They are orderly, intelligent and earnest. The museum is not a resort for the idle. The location is such as generally to require the expenditure of considerable time in travel on the part of the visitor, and although the admission fee has been small, it is nevertheless more than many can afford, and hundreds, even thousands, have been turned away thereby.

It is thought that this action on the part of President Jesup is in the line of progress, and it will doubtless add materially to the already large number who are identified with the museum as members or patrons, since it is in accord with the prevailing ideas that underlie all educational work, viz., that sources of information should not be surrounded by barriers, and that agencies of instruction should be kept constantly active.

Moreover, it should not be forgotten that there are now many privileges extended to members. They receive the *Journal* and, on request, copies of all 'Guide Leaflets.' The member's ticket admits to the evening lectures, which are arranged exclusively for members and their friends, and to the various laboratories, workrooms and other portions of the building not open to the public. The appointment of an instructor, Mrs. Roesler, makes it possible for the guests of members to be received at the museum and shown about the building; or members may leave their children with Mrs. Roesler for a few hours for instruction in the various exhibition halls.

SECOND ANNUAL MEETING OF THE AMERICAN ASSOCIATION OF MUSEUMS

THE second annual meeting of the American Association of Museums will be held at the Carnegie Institute in Pittsburg June 4–6.

Owing to the unavoidable absence of Dr. Hermon C. Bumpus, who has sailed for Europe, the duty of making all arrangements has devolved upon Dr. W. J. Holland, director of the Carnegie Museum, who has associated with himself Mr. John W. Beatty, the director of the department of fine arts of the Carnegie Institute. Those who propose to attend the meeting of the association are requested to communicate directly with Dr. Holland, who requests more particularly that all who intend to present papers at the meeting will inform him at the earliest possible date of the titles of the papers which they intend to present, so that arrangements may be properly made for the program of the meeting.

Hotel accommodations in the city of Pittsburg are ample, and arrangements have been made with three of the leading hotels, which offer the following rates:

Hotel Schenley—European Plan

Single room, one person	$2 00
Single room, one person, with bath	3 00

For two persons occupying the same room as a special concession, the same rates will be charged as for single occupants.

Hotel Lincoln—European Plan

Single room, one person	$1 50
Single room, one person, with bath	2 00

Double room from $3.00 up.
Double room, with bath, from $5.00 up.

Fort Pitt Hotel—European Plan

Single room	$1 50 to $3 50
Single room, with bath	2 50 to 3 50
Double room	2 00 to 3 00
Double room, with bath	3 50 to 5 00

The Schenley Hotel, which is very near to the Carnegie Institute, will be made hotel headquarters.

Those who attend the meeting, the sessions of which will begin on the morning of Tuesday, June 4, at 10 A.M., will register in the library of the Carnegie Museum.

It is hoped that not only those who are at present members of the association, but all who are interested in museums and their work will identify themselves with the association. Under the terms of the constitution it is provided that,

Persons actively engaged in the work of Museums may become Active Members on the payment of two dollars per annum, and may become Active Members for Life upon payment of thirty dollars at any one time.

Persons not actively engaged in the work of Museums, contributing five dollars per annum, may become Associate Members.

Each Museum paying not less than ten dollars a year shall be a Sustaining Member of the Association, and through its chief executive officer, or a properly accredited representative, shall be entitled to cast a vote on all matters coming before the Association.

The association already enrolls in its membership nearly two hundred individuals and museums.

The magnificent new buildings of the Carnegie Institute, the home of the Carnegie Museum and the Carnegie Art Gallery, which were dedicated on April 11, will furnish an inspiration to those who attend the meeting. The annual international exhibit of paintings, which will not close until the middle of June, will still be on exhibition. This is regarded by art critics as the finest and most representative display of pictures which has been seen in this country since the time of the Exposition at St. Louis. It is hoped that all museums of art as well as of science will be largely represented at the gathering, and every effort will be made to make the occasion one of enjoyment as well as of instruction to those who attend the sessions.

All communications relating to arrangements should be addressed to Dr. W. J. Holland, the director of the Carnegie Museum.

THE CARNEGIE INSTITUTION AND A DEPARTMENT OF ANTHROPOLOGY

The following letter urging the establishment of a Department of Anthropology for the purpose of investigating the problems of anthropology in South America has been signed by practically all students of anthropology in the United States:

April 3, 1907

To Dr. R. S. Woodward,

President of the Carnegie Institution of Washington, D. C.

Sir:—The undersigned were appointed by the American Anthropological Association, the Archeological Institute of America, the American Folk-Lore Society, the Anthropological Society of Washington, the American Ethnological Society, and Section H of the American Association for the Advancement of Science—the six societies of the United States entirely or mainly anthropological in scope—to discuss the subject of the most important researches that should be undertaken for the furtherance of anthropological science, and to outline a plan of research of such importance as to be worthy of the consideration of the trustees of the Carnegie Institution of Washington.

The committee believes that the isolation of the continent of South America from the great land masses of the old world in recent geological times makes the study of man's appearance on the continent and the development there of the numerous tribes, languages and cultures in early times, a problem the solution of which would be of supreme importance to anthropological science.

In such a research the study of the racial and cultural development of the peoples of this continent and particularly of the contact of this remote area with other parts of the world would be of fundamental importance.

Since it is not likely that any government will take up such an international investigation, and as it is impossible for any of the existing societies and institutions devoted to anthropological research to engage in so extensive an undertaking, the committee respectfully submits the following resolutions to the Carnegie Institution of Washington:

Resolved, That the trustees of the Carnegie Institution of Washington be respectfully requested to establish an anthropological department for the purpose of investigating the problem of the anthropology of South America, with special reference to the lines of contact between the early inhabitants of that continent and other continental areas.

Resolved, That should such a department be established its work should be based on the following four lines of investigation:

1. The antiquity of man in South America with special reference to the discoveries made in the Pampean formations. This work should be in charge of a competent geologist who should make a critical study of the strata in which the human remains have been found for which great antiquity is claimed. Associated with the geologist should be a trained archeologist who should make archeological investigations in the region of the alleged discoveries.

2. While historically no relation has been traced between the cultures of the more advanced tribes of the Andean Highlands and those of Central America, there is a general resemblance in fundamental types which seems to indicate that either a very early connection between North America and South America existed or that the later cultures grew up on the basis of an older type common to both continents. This investigation would require painstaking archeological researches extending from Mexico southward into the most southern regions to which the influence of Andean culture extended. The investigation of the ethnical relation between South America and North America would require particularly an exhaustive study of the early remains extend-

ing from Colombia northward through Central America, toward southern Mexico, to be correlated with the investigations now being carried on in Middle America.

3. Another line of connection between South America and North America probably extended over the Antillean Islands toward the Atlantic coast of the North American continent. The investigations of explorers have demonstrated that Caribbean and Arowak influences extended from southern Brazil northward to the eastern coast of the Gulf of Mexico; and North American archeology makes us suspect the existence of an earlier connection, which may have extended between South America and the southern and central portions of the United States. In this research is involved an investigation of the many scattered and isolated tribes inhabiting the Amazon valley and neighboring regions.

4. While the indications of North and South American contact are fairly definite on some lines, we have much vaguer indications of foreign influence on the Pacific coast of South America, where certain traits of culture, as well as physical appearance, suggest possible contact with the Polynesian Islands. Notwithstanding the vagueness of the indications, this question is theoretically of fundamental importance. Equally uncertain are the indications of relation with the old world on the Atlantic side, but the possibility of contact by way of the Atlantic Islands to Northwest Africa may be considered.

Resolved, That to take up the four lines of research here outlined, an annual appropriation of not less than twenty thousand dollars would be required; and the extension of the work, which would necessarily follow, would make it advisable that an anthropological department, charged with the investigation of the particular problem of the ethnical relation of South America to other continents, should have a continuous appropriation of not less than forty thousand dollars, and that its work should not be limited to a definite number of years, because even now, in the imperfect state of our knowledge, we can see that the solution of the problem will require many distinct and important lines of research. The work should therefore be continued as long as results of importance are secured in the various lines of research. Respectfully submitted,

(Signed) F. W. Putnam, *Chairman,*
for the Archeological Institute of America.

Roland B. Dixon,
for the American Folk-Lore Society.

W. H. Holmes,
for the Anthropological Society of Washington.

A. L. Kroeber,
for the American Anthropological Association.

Franz Boas, *Secretary,*
for the American Ethnological Society, and for Section H of the American Association for the Advancement of Science.

SCIENTIFIC NOTES AND NEWS

Dr. Francis Galton has been appointed to deliver the Herbert Spencer Lecture for 1907, at Oxford, and proposes to lecture this term on 'Probability the Foundation of Eugenics.'

Oxford University has conferred its doctorate of science on Dr. A. Graham Bell.

McGill University has conferred its doctorate of laws on Professor Ernest Rutherford, who is leaving McGill to accept a chair at Manchester, and the doctorate of science on Dr. H. M. Ami, of the Canadian Geological Survey.

Professors E. C. Pickering, of the Harvard College Observatory, H. Poincaré, of Paris; W. Ostwald, of Leipzig, and Ramón y Cajal, of Madrid, have been elected members of the Royal Irish Academy.

Professor Albrecht Penck, professor of physiography at Berlin, and Professor Max Noether, professor of mathematics at Erlangen, have been elected foreign members of the Academy of Sciences at Copenhagen.

Professor Wilhelm Roux, professor of anatomy at Halle, has been elected a corresponding member of the Biological Society of Paris.

Professor J. Wiesner, professor of botany at the University of Vienna, has been made an honorary doctor of applied science by the Vienna Technical Institute.

THE University of Bologna has conferred an honorary doctorate of philosophy on Professor Augusto Righi, the physicist, on the occasion of the twenty-fifth anniversary of his doctorate.

DR. H. C. VOGEL, of the Astrophysical Observatory at Potsdam, has been awarded the Maximilian order for art and science of the Bavarian government.

DR. GEORGE F. KUNZ, of New York City, has received the decoration of Knight of the Legion of Honor of the French Government in recognition of his scientific work.

PROFESSOR THEODORE W. RICHARDS, of Harvard University, began his lectures at the University of Berlin on May 4.

DR. W. C. FARABEE, who is in direction of an anthropological expedition from Harvard University, has left Arequipa for explorations among the Indian tribes at the headwaters of the Amazon.

PROFESSOR HENRY E. CRAMPTON, of Columbia University, has left New York this week for a second visit to the Island of Tahiti, where he will spend four months in the study of certain terrestrial molluscs.

DR. AND MRS. VAUGHAN CORNISH, who were at Kingston at the time of the recent earthquake, sailed on May 4 for Jamaica to study the physical effects of the seismic shock and the problem of reconstruction. Dr. Cornish will give an account of his experiences to the British Association and the Royal Geographical Society.

CAPT. J. FRANCIS LEBARON will return to the United States in May and resume his practise as a consulting engineer. Capt. LeBaron has been two years in Eastern Nicaragua engaged in a study of the water powers and water supplies.

THE class day address to the graduating class of the Michigan College of Mines was delivered by Dr. Ira Remsen, president of the Johns Hopkins University, on May 3, 1907.

PRESIDENT C. S. HOWE, of the Case School of Applied Science, will give the commencement address at the Massachusetts Agricultural College, from which he graduated in 1878.

PROFESSOR JAMES F. KEMP, head of the department of geology of Columbia University, has been appointed non-resident lecturer in economic geology next year at the Massachusetts Institute of Technology. He will deliver a course of twenty lectures.

DR. E. E. BROWN, United States Commissioner of Education, is to deliver five lectures on the historical development of Connecticut education at the Yale Summer School.

THE Croonian lecture of the Royal Society was delivered by Professor J. B. Farmer, F.R.S., on April 25, 'On the Essential Constituents of the Nucleus and their Relation to the Organization of the Individual.'

THE Cambridge Historical Society will celebrate the birth of Louis Agassiz on May 27. Brief addresses will be made by President Eliot, Professor A. Lawrence Lowell, Professor W. H. Niles, of the Massachusetts Institute of Technology, and others. Letters will be read from surviving pupils of Agassiz, who are unable to be present.

CHARLES H. HINTON, examiner in the Patent Office and known for his publications in mathematics and logic, died suddenly in Washington, on April 30. Mr. Hinton was born in London; graduated from Oxford University, and was sixty-three years old at the time of his death.

M. AIMÉ LAUSSÉDAT, member of the Paris Academy of Sciences and formerly director of the Conservatory of Arts and Trades, has died at the age of eighty-seven years.

DR. N. WAGENER, emeritus professor of zoology at the University of St. Petersburg, has died at the age of seventy-seven years.

DR. HARMER, the superintendent of the museum of zoology of Cambridge University, announces the receipt of a cast of a skeleton of *Diprotodon Australis*, presented by Dr. E. C. Stirling F.R.S., director of the South Australian Museum at Adelaide. Dr. Harmer also records the gift of a valuable consignment of some nine skeletons and forty skulls and skins of mammals, mostly antelopes, from tropical Africa, presented by Mr. C. B. C. Storey, M.A., of Clare College.

THE Baltimore *Sun* is responsible for the original announcement that "Sir William Ramsay had succeeded in accomplishing what no other chemist has ever been able to do—the segregation of one element from another and the production of copper by the synthetic or combination process from the elements sodium, lithium and potassium. A combination of these elements, when treated with radium vapor, gives as a product copper sulphate, which is readily 'broken down' into copper." This nonsense has been published with head-lines on the first page by leading newspapers throughout the country. The Boston *Transcript* publishes an editorial article indicating that it was a breach of confidence for President Remsen to make known the private communication of Sir William Ramsay!

THE department of mammalogy of the American Museum of Natural History has recently acquired by purchase a collection of mammals from China. The series includes 106 specimens, mostly of species the size of a hare or larger, of which 43 are from the Island of Hainan and 63 from the interior of China, near the foot of the Taipashiang Mountains. The latter are all new to the collection, and the Hainan specimens do not duplicate the material previously received from that island.

WE learn from *Nature* that a conference on the teaching of hygiene and temperance in the universities and schools of the British Empire was held in London on April 23. Lord Strathcona presided at the morning session and Sir John Gorst occupied the chair at the afternoon meeting. Sir Victor Horsley, F.R.S., in an address on the method of introducing hygiene and temperance into secondary schools and universities, suggested that an essential reform within the Board of Education is that there shall be such advice given to the Minister of Education as will enable him to grasp the principles of scientific education. It is the business of the state to see that the code and curriculum of education are arranged on a scientific and common-sense basis, and this will necessarily include the hygiene of common life and instruction in temperance. Sir Victor Horsley contended that we shall not make any headway unless we have expert advice at headquarters. It is clear that the whole system of education requires revision from a medico-scientific standpoint. The following resolutions were unanimously adopted: (1) "That this conference has heard with great satisfaction that instruction in hygiene and temperance is systematically given in the elementary schools of the colonies of the empire, and that there is strong evidence of the value of this teaching. While cordially acknowledging what has been already accomplished in the United Kingdom by certain educational bodies, this conference urges upon all local authorities the necessity of providing that the teaching of hygiene and temperance shall form an essential part of the whole curriculum of education of all children." (2) "This conference is of opinion that to meet adequately the responsibilities of the state towards school children, it is essential that a medical department should be instituted in the Board of Education."

GAS has been discovered in ten counties of the one hundred and five in the State of Kansas. Its history and distribution in the Kansas-Indian Territory field are so closely connected with those of oil as to be almost inseparable. About the year 1860 the numerous shallow oil wells drilled to depths of a few hundred feet in southeastern Kansas yielded traces of natural gas as well as of oil. Twenty years later, gas in small quantities was found in a number of places near Independence. The first good gas well in the vicinity of Neodesha, which is now a center of production of gas as well as of oil, was drilled in 1893. The present production of gas in the Independence quadrangle is enormous. The value of the quantity now annually consumed in the quadrangle alone is estimated to be about $800,000. More gas sands than oil sands are encountered in the drilling of individual wells. This oil may be found above the gas or below it. The gas is believed to come mainly from depths of 1,800 to 2,300 feet. Thus far most of the gas has been put to local use. It furnishes the light, fuel and power of practically all the cities and most of the farm communities and is extensively used for fuel in drilling and pumping. It also supplies the city of Parsons and its numerous industrial plants east of the quadrangle. All this, however, forms but a small percentage of the quantity consumed and to be consumed by the manufacturing industries which have grown out of this natural commodity. Of these industries the most important are those producing brick, tile, pottery, glass, cement, zinc and lead.

UNIVERSITY AND EDUCATIONAL NEWS

A bill is now before the legislature in which provision is made for the erection at the University of Wisconsin of men's dormitories, commons and union, and additional dormitory accommodation for women.

Mrs. William Thaw has given $50,000 to the Westminster University of Denver.

The Cavendish Laboratory Extension Syndicate, Cambridge University, has proposed plans for the new laboratory running along Free School-lane, which will cost between £7,000 and £8,300. Towards defraying the cost of this building there is available Lord Rayleigh's gift of £5,000 out of the Nobel prize, and Professor Thomson is able to find £2,000 from the laboratory funds.

Dr. A. Ross Hill, of the University of Missouri, has been elected professor of the philosophy of education at Cornell University, and will become dean of the faculty of arts and sciences in succession to Professor Walter F. Willcox.

Promotions in the scientific departments of the University of Chicago have been made as follows: Heinrich Maschke, to a professorship in mathematics; Frank R. Lillie, to a professorship in zoology; Robert R. Bensley, to a professorship in anatomy; Edwin O. Jordan, to a professorship in pathology and bacteriology; Leonard E. Dickson, to an associate professorship in mathematics; Charles R. Mann, to an associate professorship in physics; Robert A. Millikan, to an associate professorship in physics; Henry G. Gale, to an assistant professorship in physics; Lauder W. Jones, to an assistant professorship in chemistry; William L. Tower, to an assistant professorship in zoology; Charles J. Chamberlain, to an assistant professorship in botany; Henry C. Cowles, to an assistant professorship in botany; Howard T. Ricketts, to an assistant professorship in pathology and bacteriology; Norman M. Harris, to an assistant professorship in pathology and bacteriology.

At Cornell University Hermann Diedrichs has been advanced to a professorship of experimental engineering and Dr. Ernest Albee to a professorship of philosophy.

Dr. Thomas L. Watson, professor of geology in the Virginia Polytechnic Institute, has accepted the professorship of economic geology in the University of Virginia.

Dr. Albert Ernest Jenks has been promoted to the position of professor of anthropology in the University of Minnesota.

Mr. Gregory D. Walcott, Ph.D. (Columbia), of Blackburn College, has been elected professor of philosophy in Hamline University.

At the University of Wisconsin promotions from assistants to instructors have been made as follows: Lawrence Martin, geology; G. M. Reed, botany; Margaret Schaffner, political science; James Milward, horticulture; Conrad Hoffman, agricultural bacteriology; O. L. Kowalke, chemical engineering; F. W. Lawrence, hydraulic engineering. New assistants were appointed as follows: Hally D. M. Jolivette, botany; H. B. Sanford, electrical engineering; K. O. Burrer, electrical engineering; L. B. Aldrich, J. H. Baker, D. S. Dye, W. E. Forsythe, O. H. Gaarden, H. J. Plagge, W. F. Steve, all physics; Matthew Michels, butter and cheese scoring; A. B. Sutherland, philosophy.

Dr. N. Ach, docent for psychology at Marburg, has been called to the chair of philosophy at Marburg.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MAY 17, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE GEOLOGICAL SOCIETY OF AMERICA[1]

THE New York meeting was the largest in the history of the society, the number of fellows and fellows-elect in attendance being 133. The sessions began Thursday afternoon at Columbia University with the business meeting and the reading of memorials and were continued through Friday and Saturday at the American Museum of Natural History. Six titles were added to the printed list, making eighty-six papers in all on the program as offered. On account of the long list of communications, the sessions were held in two sections on Friday and Saturday, but even with this arrangement twenty-nine papers were read by title only, and most of the remainder were read in abbreviated form.

The council accepted the invitation of the University of New Mexico to hold the annual meeting of 1907 at Albuquerque, New Mexico, beginning December 30, 1907. It was felt that a meeting in the west would be of interest and advantage to the society at large, since it would give an opportunity for a general convention of the whole society, the Cordilleran section having intimated its willingness to meet in Albuquerque, if the general society were to meet there.

The society will hold no summer meeting this year, but the fellows have been invited to join with Section E of the American Association for the Advancement of Science in a field meeting near Plattsburgh, N. Y.

[1] Meeting in New York, December 27–29, 1906.

The following is the list of nominations for officers for 1908:

President—Samuel Calvin, Iowa City, Iowa.

First Vice-president—George F. Becker, Washington, D. C.

Second Vice-president—A. C. Lawson, Berkeley, California.

Secretary—Edmund Otis Hovey, New York City.

Treasurer—William Bullock Clark, Baltimore, Md.

Editor—Joseph Stanley-Brown, Cold Spring Harbor, N. Y.

Librarian—H. P. Cushing, Cleveland, Ohio.

Councilors (until 1911)—H. P. Cushing, Cleveland, Ohio, and H. B. Patton, Golden, Colorado.

The following resolutions were adopted in the general session of December 28 at the American Museum of Natural History:

After sixteen years of service as secretary of the Geological Society of America, Professor H. L. Fairchild meets with us at this session for the last time in his official capacity.

These years of his service have witnessed the robust and vigorous growth of this society. To his fidelity, enthusiasm, patience, conservatism and lofty ideals we must ascribe in very large part the virile and promising condition of this society to-day.

The fellows of the Geological Society of America desire to enter on its permanent records this expression of appreciation of such devoted service and this acknowledgment of, and gratitude for, so willing a sacrifice on behalf of the progress of geological science in America.

Since 1891 Dr. I. C. White, now retiring from office, has served as treasurer of this society. For these sixteen years he has given to the management of its finances the benefit of his clear, practical judgment and his experience, and he has so carefully nursed its slender incomes that to-day the treasury of the society presents a most substantial and very gratifying showing.

The fellows of the society desire to record an expression of their appreciation of this watchful, faithful and profitable service.

The program as read was as follows:

Memorial of W. B. Dwight, by F. J. H. Merrill.
Memorial of S. L. Penfield, by J. P. Iddings.
Memorial of I. C. Russell, by Bailey Willis.
Memorial of N. S. Shaler, by J. E. Wolff.

Cutting of the Mississippi and Missouri River Gorges: N. M. Fenneman. Read by title.

Lateral Erosion on some Michigan Rivers: Mark S. W. Jefferson. Read by title.

Graded Surfaces: F. P. Gulliver.

The author discussed the processes of aggradation and degradation, and the forms of surfaces produced. Various terms for different forms were considered. Examples of graded surfaces were given from Pennsylvania, New York and New England which show the necessity for greater precision in the use of certain terms, such as terrace, grade-level, base-level, etc.

The New Madrid Earthquake: M. L. Fuller and E. M. Shepard. Read by title.

Physiography of the Lower Hudson Valley: J. F. Kemp.

Series of borings across the bottom of the Hudson River in and above the Highlands have recently shown that the bedrock is unexpectedly deep. Other borings across its tributaries lead to the same conclusion. From these and observations in the field the writer showed a quite profound canyon for the Hudson, which coincides fairly well with the one already known to exist, opposite its mouth and in the continental shelf. The records are used through the courtesy of J. Waldo Smith, C.E., chief engineer of the Board of Water Supply of New York City.

Professor Kemp's paper was discussed by Professors Willis and Davis.

Relations of Physiography to Structure at Manhattan Island and Vicinity: Alexis A. Julien.

The paper discussed: (1) Palisades of the Hudson; latent minute foliation or flow structure; development of horizontal seams; facility of ice plucking; zone of decay; undercutting of columns; measure of depth of ice plucking and of age of decayed sheet. (2) Faults at Manhattan; ordinary effects and evidences; system of probable faults in this region; fault blocks on each side of the Hudson gorge. (3) Causes of early divergence of Hudson River into Hackensack Valley, two obstacles; early drainage system over Mesozoic terrane, Manhattan River; later drainage system over the Manhattan region, creeks in fault valleys; successive advances of the Hudson into series of preglacial channel gorges through and around Manhattan Island.

Professor Julien's paper was discussed by Professors Hobbs and Davis.

Geologic Map of North America: BAILEY WILLIS.

Exhibition and description of the map compiled in 1906 for the tenth International Geological Congress.

Professor Willis's paper was discussed by Professors Davis and F. D. Adams.

The Geologic Folio: WILLIAM HERBERT HOBBS.

A discussion of the advantages and disadvantages of the geologic folio as a medium for publication of geologic data.

Professor Hobbs's paper was discussed by Dr. Walcott.

Geological Map of Massachusetts and Rhode Island: B. K. EMERSON.

The map is on the scale of four inches to the mile, presenting with considerable detail the latest results reached in the study of the geology of these states. The author has had access to most of the material gathered by others in the parts of the territory not covered by his own special studies.

Professor Emerson's paper was read by title only, but the map was displayed in the Academy of Science exhibition in the museum.

The Cobalt, Ont., Silver Area: WILLET G. MILLER.

At the Philadelphia meeting of this society two years ago, the writer presented a short paper on the geology of the Temiskaming cobalt-silver area. Since that time the ore deposits of the area have attracted wide attention, not only from the unique character, on this continent, of the ores, but also from their great richness. The present paper contains further details concerning the character and relationship of the veins, and of the rocks. It was illustrated by lantern views, by a model of the more productive part of the area, and by a recently completed map on the scale of 400 feet to an inch with contour intervals of ten feet.

Professor Miller's paper was discussed by Professors Emmons, Bell and Buckley.

Virginia Barite Deposits: THOMAS LEONARD WATSON. Read by title.

Geology of Santa Barbara and Summerland Oil Field, California: RALPH ARNOLD. No abstract received.

Professor Arnold's paper was discussed by Professors Bell and Martin.

Personal Reminiscences of Sir William E. Logan: ROBERT BELL. Read by title.

Memorial of A. R. C. Selwyn: H. M. AMI.

Selwyn was a geologist who attained distinction for his work in Great Britain, Australia and British America. In 1845 he began his researches in England and Wales, in 1853 was appointed director of the Geological Survey of Victoria, Australia, and in 1869 succeeded Sir William E. Logan as director of the Geological Survey of Canada. Volcanic rocks and their relations to the earlier sedimentary formations formed the principal object of his energies in these three portions of the empire, whilst he did much to emphasize the economic relations of these groups to pure geological investigations. In America Selwyn wrought from December, 1869, to January, 1895, a period of twenty-five years. He traversed the continent before the transcontinental lines of railways were built and directed the efforts of his staff to many of the portions of Canada whose resources to-day prove of such remarkable value. He received many distinctions at home and abroad.

Cave-Sandstone Deposits of the Southern Ozarks: A. H. Purdue.

The rocks exposed within the area treated of in this paper are of Ordovician, Devonian and Carboniferous ages. Near the top of the Ordovician of the area is an unconformity upon which the Upper St. Peter sandstone is put down. Those portions of the sandstone that are put down in basins and cistern-like depressions are frequently preserved from erosion and stand up as conspicuous sandstone masses. Below this horizon, and at different levels in the Ordovician limestone, standing up on the sides of the ravines, are numerous large sandstone masses, similar to those already mentioned. These sometimes occur singly and sometimes collectively, one above the other. For several reasons, which are stated, these are considered to be cave deposits. The sandstone is of the same general character as that constituting the St. Peter, and is supposed to be of the same age. The manner of introduction of the sand and the age of the caverns was considered.

Professor Purdue's paper was discussed by Professors Grabau, Jaggar, Willis, H. C. Hovey, Fuller, Hopkins and Davis.

Volcanoes of Colima, Toluca and Popocatepetl: Edmund Otis Hovey.

The principal object of presenting the paper was to show the society some photographs of these three volcanoes which were taken upon excursions made in connection with the meeting of the tenth International Geological Congress in the City of Mexico, in September, 1906.

Toluca is the oldest of the three volcanoes. A feature of greatest interest in the crater is the dome of vitreous andesite which welled up in the crater as the latest phase of the activity of the volcano and shows a certain resemblance to the cone of Mt. Pelé. The volcano of Popocatepetl shows its composite character as a strato-volcano with great clearness in the walls of the crater, and streams of lava have been among the features of the most recent eruptions. The volcano of Colima is still sending up a vigorous column of steam from its central summit crater. From this summit crater there poured out, in the latest eruption (1903), streams of very frothy lava which present a strange appearance on account of the porous character of the surface blocks. The same feature characterizes the streams of earlier eruptions and has led some observers to the erroneous conclusion that flows of lava have not occurred at the volcano of Colima.

Current Methods of Observing Volcanic Eruptions: T. A. Jaggar, Jr.

Dana's 'Characteristics of Volcanoes,' based on the Hawaiian Islands, and books with a Vesuvian bias have given undue importance to lava in volcanology. As a result, active volcanoes have recently been interpreted almost invariably as being actuated by a rising lava column as first cause of their explosions. Such interpretation may be correct, but the evidence needs careful sifting in every case, with the alternative possibility in view, namely: (1) That tectonic causes precede magmatic phenomena, (2) that release of superheated steam may be the first cause, (3) that new lava may be a secondary product, and may not appear at all.

Another criticism deserved by all current volcanologic research is that the investigations always follow the event, and are wholly unsystematized. Even the Vesuvian observatory publishes nothing in systematic, tabulated form, and no instrumental records that may lead, eventually, to prediction. Reviewing Pelé, Soufrière, Bandaisan, Tarawera and Krakatoa, all had scientific commissions, and in no case did the commission observe the first eruption or its forerunning phenomena. The writer would point out the exceptional opportunity now possessed by the United States for seismo-volcanologic research.

Professor Jaggar's paper was discussed by Professors Reid and Willis.

Experiments Illustrating Erosion and Sedimentation: T. A. JAGGAR, JR.

The first experiments illustrating rill erosion were made in 1899, and described by Dr. E. Howe and the author (21st Annual Report U. S. G. S.). A coarse spray was used, and the miniature streams of water were an inch or more in breadth, eroding models made of sand, marble dust and coal dust. In 1901 a finer spray was used, produced by a direct jet of water deflected from an inclined and embossed surface. An experiment in 'grand canyon' topography was executed with this apparatus by R. W. Stone. Lateral planation and delta sedimentation were well shown. The latest apparatus consists of a series of atomizers operated by compressed air. Digitate drainage of some delicacy of pattern is etched by the rills on a model made of modeling clay or mill slimes. These rivulets apparently reproduce the mechanism of 'bad land' drainage, or that of a newly uplifted coastal plain. The models made hitherto throw new light on the angle of intersection of streams, on piracy, on parallelism of tributaries and on the general principles which govern the migration of divides. H. G. Ferguson has assisted in the last experiments.

Characteristics of Various Types of Conglomerates: G. R. MANSFIELD. (Introduced by W. M. Davis.)

The paper gave the results of a critical review undertaken in connection with the study of the Roxbury (Boston) conglomerate; examining the descriptions of conglomerates of many ages and countries as to the conditions of their formation.

Mr. Mansfield's paper was discussed by Professors Grabau, Barrell, Miller, C. W. Brown and Davis.

Dome Structure in Conglomerate: RALPH ARNOLD. Abstract not received.

River Sediment as a Factor in Applied Geology: W J MCGEE.

Various recent events and movements render it clear that the sediments transported by rivers require consideration, not merely as a factor in geologic process, but as an industrial factor of much moment. In systematic geology it commonly suffices to deal simply with the stream as a vehicle transporting load in solution, in suspension, and rolled or pushed along the bottom; while in most industrial problems the stream itself is regarded as the primary agency, and the load is commonly neglected, save merely as a disturbing factor. Yet in that constantly increasing control of streams (as sources of power, as supplies of water for irrigation and other purposes, as canals for transportation, etc.) which must eventually result in the complete artificialization of river systems, the load either immediately or remotely governs the character and behavior and ultimate utility of the stream. The bearing of these considerations on such engineering works as those of the lower Mississippi region and on such projects as that of the proposed Lakes-to-Gulf Waterway, is particularly worthy of discussion.

Relations between Climate and River Deposits: JOSEPH BARRELL.

The nature of subaerial river deposits is controlled by the geographic and climatic conditions of origin. The importance of the climatic factor is appreciated by contrasting the nature of deposits accumulating under similar geographic but dissimilar climatic conditions. As a result, where an ancient sedimentary deposit can be shown to be of subaerial origin, if the geographic conditions existing at the time can be allowed for, the climatic conditions of origin may be determinable. To that end, the geographic and climatic influences upon river sediments are examined under three headings: (1) relation of sediments to regions of erosion, (2) effects of transportation, (3) relation of sediments to regions of deposition. Under the latter heading the character of the surface of deposition is considered, and finally the relations of the deposits to four kinds of climates, namely, constantly rainy, intermittently rainy, subarid and arid. The effects of climatic changes upon sedimentation are also considered, and it is concluded that climatic as well as tectonic hypotheses must be considered in interpreting the cause of sedimentary variations.

Continental Origin of the Mauch Chunk Shale: JOSEPH BARRELL.

The discussion of the previous paper allows the presentation of the facts and conclusions of the present one. The Mauch Chunk Shale and the Pocono Sandstone represent the Mississippian, or sub-Carboniferous, in Pennsylvania. The former, consisting of red shales and sandstones, is considered in detail. After a statement of the relations to the underlying and overlying formations the lithologic and structural characters and the nature of the life record are stated. The problem of origin is then discussed, and it is concluded that within the limits of the anthracite coal fields the entire formation originated as flood-plain deposits on the subaerial surface of a large delta. The climate under which the formation accumulated is next considered, and it is inferred that it was of a subarid character. The long continuance of similar conditions through the Upper Devonian is then indicated, and emphasized by contrast with the opposite character of the coal measures. In this connection, the possible importance of subarid climates upon the evolution of amphibians is suggested.

Professor Barrell's two papers were discussed by Professors David White, Willis, Huntington, Grabau and Davis.

Origin of Ocean Basins in the Light of the New Seismology: WILLIAM HERBERT HOBBS.

A review of the arguments upon which the permanence of the ocean basins has been assumed, with the modifications in them which time has wrought; particularly, however, as a consequence of zoo-geographic, comparative geologic and structural studies. The 'distant' study of earthquakes has shown that they are fifteen to twenty-fold as numerous as formerly supposed, and that over ninety per cent. occur upon the floor of the seas, and appear to proceed from the scarps bordering the great ocean deeps. The data for elevation or depression available within the coral seas have been assembled, and differential vertical movement is thus shown to have been recently the greatest within the zones of earthquakes, as mapped by de Montessus.

Hypothesis of Continental Structure: BAILEY WILLIS. No abstract received.

Mr. Willis's paper was discussed by Professors Heilprin, Schuchert, Emerson and F. E. Wright.

The Limestone Ocean of pre-Cambrian Time: REGINALD A. DALY. Read by title.

Controlling Factors of Artesian Flow: M. L. FULLER.

The rapid extension of well-drilling in granites, schists, slates, etc., in the last few years, and the obtaining in them of true artesian flows at many points, together with the increasing development of wells from uniform unconfined horizontal sands, has made a revision of the commonly accepted 'requisites' of flowing wells desirable. The paper discusses the character of the reservoirs, the sources of water, the confining agents, and the source of pressure, and concludes that the requisites of artesian flows are only three in number: (1) an adequate source of water supply; (2) a retaining agent offering more resistance to the passage of water than the well or other outlet, and (3) an adequate source of pressure. The specific sources of water and head, and the specific character of confining agents are too variable to warrant inclusion in standard requisites.

Conditions of Circulation at the Sea Mills of Cephalonia: M. L. FULLER.

Near Argostoli, on the southern coast of the island of Cephalonia, in Greece, a number of streams have, for an unknown period of time, left the sea and, flowing inland with a volume sufficient to operate two sea mills, finally disappeared in a fissured limestone. To account for the continuous circulation under conditions which preclude any lower outlet, the action of interior heat on an unsymmetrical passage with a short 'inlet' and a long 'outlet' arm was postulated by F. W. and W. O. Crosby. In the longer arm, because of its greater exposure to heat, the water is supposed to be warmer and lighter than in the short arm, thus establishing the necessary conditions for circulation. A difference of 20 degrees in the average temperature, which may be supposed to occur with a system reaching a depth of 2,000 feet, would give an excess of height amounting to 10.6 feet to the warmer over the colder arm. The writer believes a difference of density (independent of temperature) in the water of the two arms affords a simpler and more effective explanation of the circulation. If the water remained unchanged in composition it would rise in the outlet arm of the passage only to the level of its entrance; but if it became diluted by an admixture of fresh water, the column in the outlet would be higher than in the inlet arm, and the essentials for circulation would be established. The specific gravity of Mediterranean waters is 1.03, hence a column of the sea water 100 feet in length will support a column of fresh water 103 feet high, or of sea water diluted one half by fresh water, a column 101½ feet in height. A depth of 2,000 feet with 50 per cent. dilution would furnish a working head of thirty feet as compared with ten feet under the heat hypothesis, while a head of three fourths of a foot, or enough to establish circulation, would be produced under the same dilution at a depth of fifty feet, or entirely within the zone controlled by atmospheric temperatures.

Mr. Fuller's two papers were discussed by Professors Kemp and Davis.

Normal Pressure Faulting in the Allegheny Plateaus: GEORGE H. ASHLEY. Read by title.

Geological Structure of the Uinta Mountains: S. F. EMMONS.

The Uinta Mountains form a range unique in the Cordilleran system, in that its axis of uplift has an east-west direction, and that it has a typical anticlinal structure. The conditions under which the range was studied by the fortieth parallel and the Powell surveys in 1869–74 were reviewed and the reasons given why those studies were necessarily incomplete. After referring to articles on separate parts of the range by J. D. Irving (1896) and Chas. P. Berkey (1905), the writer gave the conclusions he has been able to arrive at with regard to its structure, and the age of the older beds involved in the anticlinal fold; together with some remarks on a new type of topographic relief, as the result of field studies made by him in company with Mr. F. B. Weeks, during the summer of 1906.

Stratigraphy and Structure of the Uinta Mountains: F. B. Weeks. Read by title.

Structure of the Franklin Mountains, Texas: G. B. Richardson. (Introduced by C. W. Hayes.)

The Franklin Mountains are the southern extremity of the long narrow range, known locally by different names, that extends southward from the Rocky Mountains, east of the Rio Grande, as far as El Paso. They are composed of sedimentary and igneous rocks which range in age from pre-Cambrian to Cretaceous. The strata dip westward from 20 to 75 degrees, and the mountains as a whole have the appearance of a block of the Basin Range type. The distribution of the strata shows that the range is traversed by a complex system of faults. Detailed sections of this uncommon structure were given.

Probable Age of the Meguma (Gold-bearing) Series of Nova Scotia: J. E. Woodman. Read by title.

Artificial Production of Gneissic Structures by Crystallization under Stress: Fred. Eugene Wright.

The generally accepted theory of the formation of gneissic and schistose structures, as it has been developed, especially by Van Hise, F. Becke and others, postulates stress as a fundamental factor controlling the direction of crystal growth. Experiments with silicate glasses of diopside, wollastonite and other prismatic or tabular minerals, carried out in the geophysical laboratory of the Carnegie Institute, have shown that crystallization can be made to proceed in the glasses at a temperature much below the melting point of the individual minerals, and while the glass is still in a highly viscous state and capable of sustaining a considerable amount of applied mechanical stress. Glasses of these minerals were thus crystallized under stresses acting either in one direction or in two directions normal to each other, and a parallel arrangement of the prismatic crystals along definite planes or lines produced similar to that which characterize gneisses and schists. More recent experiments have also been performed with a view of obtaining a more definite idea of the order of magnitude of pressure necessary to influence and control the direction of crystal growth in such aggregates, and will be described briefly.

Origin of Meteor Crater (Coon Butte), Arizona: H. L. Fairchild.

Four years of exploration and deep boring in and about the famous crateriform basin in Arizona have revealed interesting facts tending to establish the 'meteor theory' of its origin.

The Afton Craters: W. T. Lee.

Two depressions, locally known as the Afton craters, occur in southern New Mexico, west of the Rio Grande, in the midst of a broad sand plain which represents the aggraded floor of the ancient Rio Grande. The craters are comparable in size and character with 'Coon Butte' and the crater of Zuñi Salt Lake. They differ notably from ordinary volcanic craters, but are situated in a region of recent volcanic action and are closely associated with crater cones of the ordinary type.

The geology of the surrounding regions indicates that the craters may be underlain by beds of salt, gypsum and limestone, the removal of which, by solution, may in part account for the depressions. The crater rims, however, composed of the material of the plain commingled with volcanic cinders, yield unmistakable evidences of explosive action, from which the inference is drawn that the craters are probably best explained as due to explosions of steam or other volcanic gases.

Volcanic Necks of the Mt. Taylor Region, New Mexico: D. W. JOHNSON. (Introduced by W. M. Davis.)

Some doubt has been expressed as to the correctness of interpreting as volcanic necks certain buttes which show vertical columnar structure, and which are surrounded at lower levels by undisturbed sedimentary beds. The Devil's Tower, of Wyoming, has been referred to a laccolithic origin, in part at least, because it shows the features mentioned, and the interpretation of buttes of the Mt. Taylor region as necks has been questioned. The paper presents evidence to show that the buttes of the Mt. Taylor region are undoubted volcanic necks, surrounded by undisturbed sediments, and exhibiting vertical columnar structure in many cases. The general history of the vulcanism and erosion of the region is considered, and the structural details of the neck discussed.

Earth-flows at the Time of the San Francisco Earthquake: ROBERT ANDERSON. (Introduced by Ralph Arnold.)

This paper treats of a variety of landslides caused by the concentration of water at certain points near the surface of gentle or steep slopes. Earth-flows are defined as slides or flows of portions of the surface of slopes where the surface material has been saturated, loosened and weighted down and caused to cave away and flow or creep as a semi-fluid mass. They are distinguished from *avalanches* or comparatively dry landslides of otherwise loosened material. A number of instances are described and emphasis is given to their importance as initial factors in the formation of drainage lines.

Radio-activity of the Thermal Waters of Yellowstone National Park: HERMAN SCHLUNDT and RICHARD B. MOORE. (Introduced by C. W. Hayes.) No abstract received.

A Lower Huronian Ice Age: A. P. COLEMAN.

Since the final proof of the Permian glacial period of India, Australia and South Africa, more attention is being paid to the evidences of still more ancient glaciations, *e. g.*, in Cambrian times. For years the writer has believed that the 'slate conglomerate' at the base of the Lower Huronian of Canada is glacial, since it contains angular and subangular boulders of all sizes up to cubic yards, enclosed in an unstratified matrix. These boulders are often miles from any possible source. Recently, striated stones have been broken out of their matrix in the Lower Huronian of the Cobalt silver region, giving still stronger proofs that the formation is an ancient boulder clay. The results of this investigation have an important bearing on the earth's early history, since the Lower Huronian has only one known formation before it, the Keewatin. The earth's internal heat was not sufficient at that time to prevent the formation of an ice sheet in latitude 46 degrees.

Professor Coleman's paper was discussed by Professors Miller, Salisbury, Lane, Bell and Clapp.

Glaciation of Manhattan Island, New York: Alexis A. Julien. Read by title.

Glacial Erosion in the Northfiord: Mark S. W. Jefferson. Read by title.

Recent Changes in the Glaciers of Glacier Bay, Alaska: F. E. and C. W. Wright.

A general geologic reconnoissance of Glacier Bay, Alaska, was made by the writers, assisted by Mr. R. W. Pumpelly, during the past summer. In the course of their investigations the existing glaciers were remapped and studied with special reference to the changes which have taken place since 1891, when a careful topographical survey was made of them by Dr. H. Fielding Reid. It was found that, in general, recession and melting on a remarkable scale have prevailed, although local advance was observed at several points. Comparative photographs were presented showing these changes and the causes were discussed which have probably been active in producing such effects. Incidentally, several phases of glacial sculpture were briefly described, also a new photo-topographic method which was applied to this region and found well adapted to work of such character.

Professor Wright's paper was discussed by Professors Blake, Reid, Gulliver and G. F. Wright.

Recent Changes in the Malaspina and other Glaciers of the Yakutat Bay Region, Alaska: Ralph S. Tarr.

In the interval between September, 1905, and June, 1906, the eastern (Marvine) lobe of the Malaspina glacier and several smaller glaciers in the Yakutat Bay region have advanced so rapidly as to break the ice into a sea of crevasses. Glaciers which were easily traversed in 1905 are now practically impassable. This paper describes these changes, shows comparative illustrations of the conditions in the two seasons, discusses the phenomena associated with the change, proves that the forward movement is still in progress, and discusses the cause of this remarkable change, suggesting its relation to the vigorous earthquake action.

Professor Tarr's paper was discussed by Professors Jaggar, Reid and Brooks.

The Glacier of the Lebanon Mountains: G. Frederick Wright.

In the autumn of 1905, in company with Professor A. E. Day, of the Syrian Protestant College of Beirut, the author took a horseback excursion of several days, leading diagonally from Beirut to the cedars of Lebanon. The results of his observations were to demonstrate, from the lack of glacial phenomena, and from the character of eroded surface, that there had been no general glaciation of these mountains. But it was clearly evident that a single glacier had extended from the highest summit of the range (10,000 feet above the sea) five or six miles down the valley of the Kadisha River, and lingered long enough to build up a terminal moraine three miles wide and four miles long and one thousand feet in thickness. Upon the surface of the upper end of this moraine the famous grove of the cedars of Lebanon is now to be found. Many subsidiary observations were recorded in correction of erroneous views which have been entertained.

Professor Wright's paper was discussed by Dr. H. C. Hovey and Professor G. F. Wright.

Ice Present during the Formation of Glacial Terraces: F. P. Gulliver.

This paper described with maps and lantern slides some glacial deposits along the Connecticut, Thames and Quinnebaug rivers, which have usually been classed with the terraces formed by the downcutting of the rivers. An example of terraces which have surely been carved by river action is found on the Westfield river west of Springfield, Mass. The deposits described along the Connecticut rivers were contrasted with those found at Westfield, and it was shown that they must have been formed before the ice had completely melted from the valleys. These deposits were therefore forms of aggradation and not forms produced by degradation. Typical eskers, deltas and kettle-holes are associated with these so-called terraces; and even where these deposits have the characteristic form of river-cut terraces, cross sections as revealed by railway or other cuts show delta structure rather than the structure of alluvial flood plains. The delta lobes point either down stream or into side valleys, and there are frequently found unfilled portions of the main preglacial valley and of its tributaries, below the level of the delta-terrace, between the delta-terrace and the rock walls of the older valley.

Dr. Gulliver's paper was discussed by Professors Clapp, Salisbury, Leverett and Alden.

Discovery of Cambrian Rocks in Southeastern California: N. H. Darton. Read by title.

Limestones of Westchester and Putnam Counties, New York: Charles P. Berkey.

In the course of detailed areal mapping of the Tarrytown and West Point quadrangles, opportunity has been offered for extensive study of the variations and comparisons of the relationships of the formations characteristic of the Highlands region of New York. Certain constants of relation and character together with the causes for occasional variability and abnormal occurrences were discussed in this paper, and their bearing upon further stratigraphic and structural work suggested.

The Galena Series: Frederick W. Sardeson.

The so-called Trenton and the Galena formations of the Galena series in Wisconsin, Illinois, Iowa and Minnesota were discussed in regard to their present and original wide extent and uniform thickness. The lithologic diversity and the faunally uniform condition of the parts of the series were briefly considered. The relation which the naming of these formations has borne to the formational uniformity was outlined. The value of the Beloit formation as a geologic unit was followed by a like discussion of the Platteville limestone, leading to the question of expediency in using lithologic, as against paleontologic, evidence, as the basis for geologic formational units, in regard to the Galena series in particular, and somewhat as to formations in general.

Age and Stratigraphic Relations of the Chattanooga Black Shale: Amadeus W. Grabau.

Recent studies made of the principal sections of the Black Shale in the southern Appalachians have convinced the author that the reported hiatus between the Black Shale and the overlying formations does not exist, and that hence the age of the shale needs to be reconsidered. The facts bearing on this problem, together with an outline of the corresponding paleographic conditions of eastern United States as interpreted by the author, were presented.

The Medina Sandstone Problem: Amadeus W. Grabau.

Following up the line of investigation which led the author, at the Philadelphia meeting of the society, to announce his conclusions that the Oneida conglomerate is of late Medina age, and the Shawangunk conglomerate is of Salina age (since confirmed by the finding by the New York Survey of a Salina fauna in this conglomerate), the present communication deals more especially with the mode of formation of these sandstones and conglomerates.

The structural and stratigraphic evidence, pointing to a continental origin (river and æolian) of portions of these sandstones were given, and further evidence presented which seems to indicate that part of the accumulation of these sands began in Ordovicic time.

Paleogeography of the American Devonic: Charles Schuchert.

A series of lantern slides was shown, illustrating the relation of the seas and lands of Helderbergian, Oriskanian, Onondaga, Hamilton and Chemung times.

Carboniferous of the Appalachian Basin: John J. Stevenson. Read by title.

Coal Measures and Higher Beds of South Brazil: I. C. White.

The author showed that a great system of rocks (the Santa Catharina system) exists in South Brazil, covering large areas in the states of Rio Grande do Sul, Santa Catharina, Parana and S. Paulo, entirely comparable to the Karroo system of South Africa. Also that, like South Africa, Brazil had its Glacial epoch succeeding the deposition of the Coal Measures, the 'Dwyka' and 'Talchir' conglomerates of Africa and India being paralleled by the Orleans conglomerate of South Brazil; that the Brazilian Coal Measure flora, as described by Mr. David White, is the same as that of the Coal Measures in South Africa, India and other southern regions during Permian time, and also that the reptilian fauna, as described by Dr. J. H. McGregor and Dr. A. Smith Woodward, is the same as that characterizing the South African beds. In other words, that the 'Gondwanaland' of the Indian geologists not only extended into Africa, but crossing from the latter into South America, most probably encircled in a broad belt the entire southern hemisphere.

Permo-carboniferous Climatic Changes in Brazilian South America, as indicated by Fossil Plants: David White.

The paper presented the conclusions, drawn from a study of the fossil plants, respecting Permo-carboniferous glaciation in Brazil, subsequent Permian climatic changes, and probable geographic relations of the southern portion of the continent, both to the Indo-Africo-Australian 'Gondwana Land' and to the northern land masses.

Structure of the Deep River Triassic: Collier Cobb. Read by title.

Red Beds of Oklahoma and Adjacent States: Charles Newton Gould. Read by title.

Stratigraphic Relations in Central Wyoming: N. H. Darton. Read by title.

Correlation of the Triassic Trap Rocks of New Jersey: J. Volney Lewis. Read by title.

Pleistocene Glacial Phenomena of the Bolivian Plateau: W. G. Tight.

The paper described the topography and glacial deposits in the vicinity of La Paz, and the Cordillera Real east of the Lake Titicaca basin, and established the fact that in Quaternary times the Bolivian plateau region was subjected to three distinct stages of glaciation with well-marked interglacial periods. The paper also described the characters of the great basin south of the lake Titicaca region which was the floor of a very extensive glacial lake. The old beaches occur in three sets which are correlated with the three stages of glaciation of the plateau.

Preglacial Drainage in the Mississippi Valley, A Working Hypothesis: W. G. Tight.

The hypothesis proposes that the present Mississippi, Ohio and Missouri drainage systems are of early Quaternary origin. That prior to the first ice invasion of the Quaternary the whole upper Mississippi drainage was to the north into the Hudson Bay. The movement of the ice into this basin advanced upon a rising plane. The margin of the ice was generally uniform and attenuated. The impounding of the drainage waters resulted in extensive frontal lakes. Sluggish action of the ice and poorly developed moraines, extensive sheets of extra morainic drift, slow movement of the waters from the ice front with deposition of loess and with general aggradation along the stream courses, the modification of preglacial topography and profound changes in drainage were some of the resultants. A new outlet to the south along the line of the present Mississippi over a low col between the southern end of the Appalachians and the Ozarks, was established and the general plan of the present drainage lines of the Mississippi basin developed. The later ice invasion of the Quaternary into the basin followed the establishment of gradients and developed the general lobate form of the margin characteristic of these invasions and produced only local and minor frontal lake phenomena south of the continental divide which was later discovered by the recession of the ice. The development of strong lobate moraines, the distribution of but a small amount of extra morainic drift, and the vigorous action of the streams discharging from the later ice fronts, were characteristic phenomena. The paper recited some of the evidence in support of the hypothesis and asked for more careful observation of the phenomena upon which data must be collected to prove or disprove the hypothesis.

Professor Tight's paper was discussed by Messrs. Leverett and Carney.

Glacial Flowage over New England: J. B. WOODWORTH. Read by title.

Quaternary Changes of Level in New England: FREDERICK G. CLAPP. No abstract received.

Mr. Clapp's paper was discussed by Professors Leverett, Hitchcock, Alden and Ami.

Glacial Lake Memphremagog: C. H. HITCHCOCK.

The existence of this lake was pointed out at the meeting of the Geological Society of America in 1894 (*Bull. Geol. Soc. Am.*, Vol. 6). Recent studies show that it was tributary to Glacial Lake Champlain by way of both the La Moille and Winooski valleys. When the ice filled the Champlain valley to the depth of a thousand feet, the impounded water upon the east side could have reached the Connecticut valley by way of White River.

Professor Hitchcock's paper was discussed by Professors G. F. Wright and Richardson.

Pre-Wisconsin Drift in the Finger Lake Region of New York: FRANK CARNEY. No abstract received. (Introduced by H. L. Fairchild.)

Mr. Carney's paper was discussed by Professors Tarr, Clapp and Leverett.

Wave-cut Terraces in Keuka Valley Older than the Recession Stage of Wisconsin Ice: FRANK CARNEY. No abstract received. (Introduced by H. L. Fairchild.)

Mr. Carney's paper was discussed by Professor Clapp.

Deposits of Glacial Age in the Non-glaciated regions of Central Asia: ELLSWORTH HUNTINGTON.

Different rates of weathering, and consequently of erosion and deposition, during glacial as opposed to inter-glacial epochs in arid regions appear to cause the alternate deposition and erosion of gravel beds in the bottoms of all the valleys, whether connected with glaciers or not. The process gives rise to a series of terraces, uniform over wide areas. Such terraces have often, and probably wrongly, been interpreted as evidence of earth movements. In the self-contained basins of Central Asia glacial epochs were characterized by enlarged lakes in which greenish clays were deposited; inter-glacial epochs by diminished lakes and by the deposition of reddish, subaerial strata upon the previous lacustrine clays. A study of such alternating lacustrine and subaerial deposits in three widely separated regions leads to the conclusion that the glacial period consisted of an *increasingly* severe series of climatic oscillations preceding the well-known *decreasingly* severe series. Coal and iron ore occur interbedded with lake deposits. This suggests that some of the coal beds of more ancient times may indicate rapid changes of climate such as those of the Pleistocene and Permian glacial periods.

Some Results from the Study of the Cambrian Brachiopoda: CHAS. D. WALCOTT. Read by title.

Cryptozoon: Genera, Species, Relationships: HENRY M. SEELY. Read by title.

Crustacean Fauna of the Shawangunk Grit in Eastern New York: JOHN M. CLARKE. Read by title.

Additional Footprints from the Carboniferous Shales of Massachusetts: J. B. WOODWORTH. Read by title.

Occurrence of Unusually Large Calcite Crystals in New York State: D. H. NEWLAND. Read by title.

Origin of the Lead and Zinc Ores in Missouri: E. R. BUCKLEY. No abstract received.

Professor Buckley's paper was discussed by Drs. H. C. Hovey and Ami.

Asymmetric Differentiation in a Syenite Bathylith: H. P. CUSHING.

This syenite occurs in the mid-Adirondack region, occupying some 100 square miles of surface. The normal rock is a highly feldspathic one, with about sixty-three per cent. of silica. It is one of the great pre-Cambrian intrusives of the region, of later date than the Grenville sediments, and the Laurentian granite gneiss. It shows contacts against anorthosite and Grenville calcareous sediments on one side, when it becomes basic, with a high content of ferro-magnesian minerals; on another side it cuts Laurentian granite gneisses and becomes acidic, approaching a granite in character and composition. In this granite portion is a considerable anorthosite inclusion, surrounded by basic syenite, which grades into the normal acid variety. The relations seem to point to incorporation and assimilation of the adjacent rocks as the cause of the asymmetry of the bathylith. Intermediate rocks, such as anorthosite soaked with syenite, are also found.

Professor Cushing's paper was discussed by Dr. Lane.

Formation of Leucite in Igneous Rocks: HENRY S. WASHINGTON. Read by title.

Genetic Connections of Some Granitic Dikes: ALFRED C. LANE.

Near Huron Mountain, Marquette County, the hornblende gneisses and schists are cut by series of red granitic dikes. The coarser pegmatitic ones cut the finer, and may be regarded as products of the same magma when the country rock had been heated up and the magma cooled.

Different Manifestations of the Ophitic Texture: ALFRED C. LANE.

The ophitic texture results when idiomorphic feldspar is embedded in augite crystals. These in their growth crowd before them corroded remnants of olivine, etc. Specimens were shown illustrating the mottled effect which this texture gives to the rock under various conditions, from those of the 'lustermottled melaphyre' to those of the 'varioloid greenstone.' The origin of the mottling is partly the ready alteration of the olivine, partly the porosity between the augite crystals. This latter character may be rather characteristic of the effusives.

Occurrence of Diamonds in North America: GEORGE F. KUNZ. Read by title.

Silver-gold Ores at San Pedro de Guanacevi, Durango, Mex.: FREDERICK B. PECK. No abstract received.

Perspective View of the Submarine Canyon of the Hudson River: J. W. SPENCER. Read by title. (Read before Section E, American Association for the Advancement of Science, December 31.)

Titaniferous Basalts of the Western Mediterranean. H. S. WASHINGTON. Read by title.

The Paleozoic Section of the Upper Yukon: A. H. BROOKS and E. M. KINDLE. Read by title.

Stratigraphic Succession North of Cook Inlet, Alaska: SIDNEY PAIGE and ADOLPH KNOPP. (Introduced by A. H. Brooks.) Read by title.

Seismological Observations in the United States: H. F. REID. Read by title.

Peale's Painting of the Exhuming of the First American Mastodon: ARTHUR BIBBINS. Read by title. (Read before Section E, American Association for the Advancement of Science, December 31.)

Relations of the Ithaca and Chemung Faunas of Western Maryland: C. K. SWARTZ. Read by title.

EDMUND OTIS HOVEY.

ZOOLOGY AT THE NEW YORK MEETING

II.

The Order of Appearance of the Ambulacral Appendages in Holothuria floridana Pourtalès:[1] CHARLES L. EDWARDS.

Tentacles.—During the fourth day the embryo has a primitive symmetry of four tentacles; one placed in the mid dorsal interradius arising from the left dorsal radial canal, one in the right dorsal interradius from the right ventral radial canal, one in the right ventral interradius from the mid ventral radial canal and one in the left dorsal interradius from the left ventral radial canal. During the fifth and last day within the vitelline membrane, the embryo buds a fifth tentacle into the left ventral interradius from the mid ventral radial canal. In this condition the Holothurid hatches during the sixth day but it is not until the eighth day that the fifth tentacle has grown to the size of the four primitive tentacles. On the fortieth day a sixth tentacle develops in the right ventral interradius from the right ventral radial canal. From the forty-second to the forty-fifth days the next three tentacles appear; the seventh, in the left ventral interradius, from the left ventral radial canal, the eighth, in the mid dorsal interradius from the right dorsal radial canal and the ninth, in either the right or left dorsal interradius, from the right or left dorsal radial canal, respectively. On the fifty-third day the tenth tentacle appears in the dorsal interradius opposite to that in which the ninth has developed. On the seventy-fifth day the eleventh tentacle appears in the mid dorsal interradius.

Pedicels and Papillæ.—The first pedicel has budded from the posterior end of the

[1] Formerly identified as *Mülleria agassizii* Sel. —Edwards, C. L., 'Notes on the Embryology of *Mülleria Agassizii* Sel., a Holothurian common at Green Turtle Cay, Bahamas,' Johns Hopkins Univ. Circ., 1889, Vol. VIII., p. 37.

mid ventral radial canal on the fourth day and, after hatching on the sixth day, develops a sucker. On the ninth day a second pedicel arises to the left from the mid ventral radius. Only two pedicels are found until the twenty-second day when a third appears also to the left of the mid ventral radius. On the twenty-fourth day buds of the first pair of papillæ ventrad from the anterior ends of the dorsal radii inaugurate the bilateral symmetry later shown in the appendages. On the thirtieth day the fourth pedicel, *again to the left,* arises from the mid ventral radius and also one bud ventrad from both right and left ventral radii. Not until the fortieth day does the first pedicel arise to the right from the mid ventral radius. On the fifty-third day the second pair of papillæ arises ventrad from toward the posterior ends of the dorsal radii. At this time twenty pedicels and nineteen buds have appeared from the ventral radii and nine papillæ and twenty-seven buds from the dorsal. The seventy-fifth day in my series from the embryos presents the largest number of appendages, forty developed and forty-five buds. Four of the smallest adults from my statistical series have 77, 99, 141 and 150 appendages and twenty, the adult number, of tentacles.

Some Further Points in the Development of Ophiothrix fragilis: E. W. MacBride, M.A., F.R.S., McGill University.

In December, 1903, the author read a paper at the Philadelphia meeting of the American zoologists on the early stages in the development of the British ophiuroid, *Ophiothrix fragilis.* Since that time he has been continuously engaged in working out the development completely and hopes to be able to publish an exhaustive account of it this summer. Meanwhile some interesting points have transpired. In the former paper on the subject the segmentation was described as leading to the formation of a morula. An invagination on one side gave origin to the archenteron, the invaginated cells not forming a simple vesicle but a sac with a solid tongue projecting from one side of it. From the apex of the archenteron the cœlom arose as a vesicle, and the embryo became triangular in shape—one point being posterior and the two others the rudiments of the first arms of the ophiopluteus larva. The interior cells of the morula gave rise to the mesenchyme from which the skeleton of these arms was developed. A subsequent visit to Plymouth and renewed experiments in artificial fertilization led to the unexpected result that the type of development previously described was that of eggs which were not quite ripe. When a male and female were enclosed in a glass jar and allowed to spawn naturally the segmentation of the egg led to the formation of a hollow blastula one side of which became thickened and gave rise to mesenchyme. Regular invagination followed giving rise to a normal archenteron entirely devoid of any such tongue as was described above, the cœlom arose as a thin-walled vesicle which became completely separated from the archenteron before dividing into right and left halves. At the opposite pole of the larva to the blastopore there was developed a great crest of vacuolated cells, probably an apparatus to assist in flotation. This crest disappeared as the first two arms of the larva became larger. It follows, then, that it is not a certain test of the ripeness of an egg that it can be fertilized, and that a small change in the chemical condition of the egg can effect a great change in the subsequent development.

The later development of the larva is interesting on account of the history of the cœlom. This becomes divided on both left and right sides into anterior and posterior halves. Then later from the posterior end of the anterior half on each side a vesicle grows out. These vesicles are the left and right hydrocœles. The latter though rudimentary in the adult is at first just as large as the left one which gives rise to the water-vascular system. The right hydrocœle in Asteroidea and Echinoidea has from the first a position near the mid dorsal line, on which account some have doubted its homology with the left hydrocœle. But there can be no doubt of its homology in *Ophiothrix fragilis* where it is not only normal in position but sometimes assumes a five-lobed form similar to that of the left hydrocœle. Its dorsal position near the primary water-pore in Asteroidea and Echinoidea is accounted for by the fact that in these groups it is oriented not with respect to the larval mouth but with respect to the permanent mouth which is on the left of the larva. In Ophiuroidea, where the larval mouth persists as the adult mouth, it is accordingly found unmistakably on the right side. Subsequently the preponderant growth of the left hydrocœle and all the structures associated with it carries the water pore dorsal to the mouth over to the right side where it comes to lie near the right hydrocœle, a position which it occupies from the beginning in Asteroidea and Echinoidea.

Exhibition of Embryos of the Japanese Frilled Shark, Chlamydoselachus angunieus, with Comments upon its Plan of Development: BASHFORD DEAN, Columbia University.

Mercator Projections of Vertebrate and Arthropod Embryos: WILLIAM PATTEN, Dartmouth College.

The Pre-placental Development in Geomys bursarius: THOMAS G. LEE, University of Minnesota.

This investigation is a continuation of the writer's comparative studies upon the earliest stages of development in North American Rodentia.

Geomys bursarius, or the 'pocket gopher' as it is commonly called, is characterized by the so-called inversion of the layers which is of a much simpler type than that found in other rodents, as the mouse and guinea-pig.

Development takes place entirely outside of the uterine cavity in a decidual cavity formed by the vascularization and breaking down of the ventral uterine connective tissue. The didermic blastocyst perforates the epithelium lining the ventral portion of the uterine cavity. This perforation is relatively large and does not become closed as in the guinea-pig or in man, but remains open for a considerable period. The epithelial lip of this rounded opening becomes somewhat thickened and everted.

The trophoblast, in a zone a little way external to the germinal area, becomes adherent to the outer margins of this epithelial lip, thus suspending the blastocyst while the decidual cavity is being hollowed out beneath it in the connective tissue.

Rauber's layer disappears from the surface of the germinal area at about the time of perforation. The entoderm forms a well-marked vesicle or yolk-sac which rapidly increases in size and which becomes invaginated on its dorsal surface by the sinking in of the germinal area, thus bringing about the so-called inversion of layers.

The amnion is formed by the folding over and fusion of the outer margins of the germinal area from all sides, while the outer portion of these folds, composed of trophoblast, form a membrane constituting the serosa or false amnion. This serosa now closes the opening through the uterine epithelium above referred to.

At a later period, secondary folds of the serosa appear which unite forming a two-layered cup or vesicle of trophoblastic tissue, the cavity of which disappears by the proliferation of the cells, and this rounded plate of trophoblast which now constitutes the fœtal portion of the true placenta is brought into contact with the epithelium of the dorsal portion of the uterine cavity, giving rise to the true placenta.

Into the ventral surface of this trophoblastic plate extends the vascular mesoderm to complete the allantoic portion of the placenta.

While these changes are taking place, the embryo has been rapidly developing, nourished by a highly developed yolk-sac placenta which ceases to be functional after the completion of the true dorsal placenta.

The Maturation of the Mouse Egg: W. R. Coe and W. B. Kirkham.

The process of maturation and fertilization in a mammalian egg was first described in detail by Sobotta in 1895. His work was with the mouse egg, and he recorded the formation of more than one polar body in only one tenth of these eggs. Gerlach, after a study of preparations made as early as 1890, has recently revived Tafani's theory that in the majority of mouse eggs the second polar body is suppressed. Gerlach's conclusion is that when a spermatozoon enters an egg some time after it has formed the second polar spindle, the second polar body fails to develop, the spindle degenerating within the egg. These observations differ not only from almost all those previously made upon other eggs, but also from the conclusions since arrived at by Van der Stricht, Heape and Rubaschkin, for the eggs of the bat, rabbit and guinea-pig, respectively, who all agree that two polar bodies are regularly formed by every ripe egg.

Careful study of numerous series of sections of eggs and ovaries of the white mouse have led to the following conclusions:

1. Two polar bodies are apparently formed by every egg which is capable of development, the first polar body appearing within the ovary, the second after the entrance of the spermatozoon into the egg.

2. At the end of the spireme the number of chromatin masses is between twelve and twenty-four.

3. Twelve masses of chromatin are cast out with the first polar body, and a like number remain in the egg.

4. There is a sharp distinction in form between the chromosomes of the first and those of the second polar spindle.

5. Every egg which we have seen in the Fallopian tube before fertilization possessed a second polar spindle.

6. The zona pellucida, which is quite distinct, may persist undiminished through the early cleavage stages; but in most cases the first polar body escapes from it during the process of ovulation, so that the majority of eggs after fertilization possess the second polar body only.

7. During the spring months ovulation commonly occurs every twenty-one days, independent of copulation.

8. The number of univalent chromosomes in the second polar spindle is twenty-four, of which the second polar body receives twelve, leaving an equal number to form the egg nucleus.

9. The second polar body is formed only after the egg has been fertilized.

10. The first and second polar bodies differ in size, shape, and especially in chromatin content, so that they are easily distinguishable.

11. At least the greater part, if not the entire sperm tail enters the egg at the time of fertilization.

12. Since the mouse egg in every case which we have observed forms two polar

bodies of typical constitution, its maturation processes are in accord with those of most other metazoon eggs.

The 'Accessory Chromosome' in Anasa tristis: KATHARINE FOOT and E. C. STROBELL, New York.

The authors interpreted the so-called chromosome nucleolus of the resting spermatocyte as the homologue of the nucleolus of the egg and not as a chromosome, as maintained by the cytologists who have previously investigated this form. They interpreted the so-called heterotropic chromosome as a bivalent, representing in value two spermatogonial chromosomes and not one. In a series of forty-nine photomicrographs they traced it from the early prophase to the telophase of the second division, demonstrating its division both in the first *and in the second* spindle. Three of the photomicrographs showed spermatogonia in which twenty-two chromosomes were demonstrated.

Secondary Chromosome-couplings in Hemiptera and their possible Significance: EDMUND B. WILSON, Columbia University.

As secondary chromosome-couplings we may designate unions or associations of the chromosomes that take place independently of synapsis, such as those described by Sinéty in *Leptynia* and by McClung in the Acrididæ. In the spermatogenesis of the *Hemiptera heteroptera* such couplings occur in several genera. In *Pachylis gigas* the 'accessory' or odd chromosome often couples with one member of one of the bivalents in the first spermatocyte-division and passes with it to one pole, but the process is inconstant and appears to be of a casual character. In *Thyanta custator,* on the other hand, there is a small unpaired chromosome that is always separate from the others in the first division but in the second is invariably coupled with one member of the smallest pair of chromosomes and passes with it undivided to one pole. *Metapodius* presents a still more interesting relation. Here a small unpaired chromosome is present in some individuals, but not in all, in addition to a pair of typical unequal idiochromosomes. The latter show the usual relation to sex-production, while the unpaired chromosome may be present in either sex and hence is of different nature from the odd or 'accessory' sex-chromosome. Here too the unpaired chromosome is always separate from the others in the first division, but in the second it is in about 80 per cent. of the cells coupled with one of the idiochromosomes. In a marked majority of cases the coupling takes place with the small idiochromosome, and the unpaired chromosome passes to the male-producing pole; but in some cases the coupling is with the large idiochromosome. We should, therefore, expect to find the unpaired chromosome present in a majority of the male individuals and in a minority of the female ones; and this is borne out by the data as far as they go, though they are somewhat scanty. Of seven males (testes) five possess and two lack this chromosome. Of five females (ovarian cells) but one possesses while five lack this chromosome. The conditions are constant in each individual.

These facts suggest that if the chromosomes embody the primary factors of heredity, the coupling of chromosomes may give the physical basis of certain forms of character-couplings. For instance, the coupling of the sex-characters with the somatic species-characters observed in certain forms of Mendelian hybrids in Lepidoptera may be due to a coupling of the sex-chromosome with one of the other chromosomes, of the same general nature as that observed in *Metapodius.* The study of the chromosomes in such cases in combination with experimental work may thus give a decisive test of the general chromosome-theory of heredity.

Maturation Processes in Paramecium caudatum: Gary N. Calkins, Columbia University.

On the Formation of Regenerative Masses in Sponges allowed to degenerate in confinement: H. V. Wilson, University of North Carolina.

Silicious sponges (*Stylotella, Microciona*) kept under favorable conditions in aquaria undergo degenerative changes, resulting in the formation of small masses of unspecialized tissue, which lie scattered through the dead sponge, like gemmules in a *Spongilla.* Such masses when returned to the normal environment transform into perfect sponges.

The Influence of a Strong Centrifugal Force on the Egg of Arbacia: T. H. Morgan and E. P. Lyon.

The Influences of External Factors, Chemical and Physical, on the Development of Fundulus Heteroclitus: Charles R. Stockard, Columbia University.

1. *Fundulus* eggs develop normally, although at a somewhat faster rate, when kept on moist plates entirely out of water. Such embryos are unable to hatch while on the moist plates, but if at any time after the control has begun hatching some of the eggs are immersed in sea-water they will soon begin hatching, commencing usually in about ten minutes after being in the water and all coming out promptly. On hatching the embryos show a positively heliotropic and a negatively geotropic reaction.

Embryos were kept thirty-three days, or twenty days after the control had begun hatching, on these moist plates without beginning to hatch. The fish within the egg membrane grows in length and absorbs its yolk at about the same rate as hatched ones do. They finally die of starvation after having assimilated all of their yolk, being still confined within the egg membrane.

2. These eggs are not entirely immune to osmotic effects though it has often been stated that they are. In weak cane sugar solutions the yolks were observed to swell; this has not been seen even in eggs developing in distilled water, and may probably be due to some change taking place in the sugar after it has permeated the egg membrane. In concentrated sugar solutions the yolk shrinks in a somewhat definite manner. A 1.53 m distilled water solution of cane sugar killed the eggs within twenty-three hours. The osmotic pressure of such a solution is about 34.278 atmospheres, about twelve atmospheres more than that of sea-water. Some salt solutions exerting even a greater pressure do not kill the eggs. The contradiction is possibly due to the cane sugar becoming inverted in the solutions and its pressure is thus more than the amount calculated. On comparing the effects of sea-water solutions of sugar with distilled water solutions it was found that a pressure more than double as high in sea-water produced a much less marked effect. Sea-water solutions were alkaline and inversion of the sugar was not so likely to occur.

3. Several lithium salts produced similar and characteristic abnormalities in development.

4. Embryos developed in solutions of KCl show no heart beat or circulation of the blood; the circulatory system is also abnormal. NH_4Cl produces a general and indefinite effect on development. $MnCl_2$ causes a definite effect on the early stages of development. $MgCl_2$ causes the formation of cyclopean monsters. NaCl causes the embryos to swim abnormally in a twisting spiral course.

5. Mixed solutions of salts and sugar act more intensely on the *Fundulus* egg than either constituent would if used alone. A small dose of a salt will give the effect of a much stronger dose if sugar be added to the solution.

The Degree of Correlation of Certain Internal Characters in the Toad: W. E. KELLICOTT, Womans College, Baltimore.

Morphogenetic Localization in Aglaophenia: RAYMOND PEARL, University of Pennsylvania.

In the plumularian hydroid *Aglaophenia helleri* each fully grown internode of the hydrocladium is divided into three regions by two incomplete, transverse, chitinous septa or ridges. The proximal one of these ridges is at the level of the intrathecal ridge, and the distal one is at the level of the proximal border of the supracalycine nematophores. A study was made of the proportionality of the parts of the internode marked out by these ridges. The general results may be stated as follows: (1) Those proportions of the individual internode and hydrotheca (= person) which involve the localization of points by a process of differentiation *in situ* after growth is entirely or nearly completed, are maintained in different persons with very much less constancy or precision than are those proportions which depend primarily on growth (as distinct from differentiation) localizations. (2) There is a distinct correlation between the proportionality of the parts and the absolute size of the person in *Aglaophenia.* In this as in all other cases which have so far been studied quantitatively with reference to this point, the actual facts are in direct contradiction to the fundamental assumption made by Driesch in the development of his so-called 'first proof of the autonomy of vital phenomena,' to the effect that the proportionality of the parts of an organism is something quite independent of the absolute size.

Correlation as the Basis for Selection in Lepidoptera: H. E. CRAMPTON, Columbia University.

The Blending and Overlap of Instincts: FRANCIS H. HERRICK, Western Reserve University.

There are many anomalous actions or peculiarities of behavior in wild birds which have not been satisfactorily explained, although certain of them have been long known. Some of the eccentricities of conduct referred to are the following: (1) Repair of the old nest or the building of a new one at the close of the breeding season; (2) omission of nest building, and dropping of eggs on the ground; (3) leaving young to perish in nest, and starting on migration; (4) offering strings or other objects to young in the place of food; (5) building more than one nest including the 'cock nests' of marsh wrens; (6) rebuilding on the same 'site,' producing superimposed nests or nests of from two to four 'stories' 'to conceal' foreign bodies, such as the cowbirds' eggs in the nests of vireos and warblers.

All of these curious actions receive much light, and in most cases are satisfactorily explained by what we shall call the blending or overlapping of instincts. As shown in another paper, the wild bird commonly passes through a cycle of instincts which mark the breeding season. This cycle is made up of eight or more terms, which follow in serial order, and some of which are recurrent. Normally the bird passes from center of influence 1 to center 2, 3, and so on, to the end of the cycle. There is little overlap or blending, the bird remaining under the influence of a given instinct or series of instincts, such as nest building, incubation, or feeding the young until its instinct in any given direction has been satisfied, before entering a new sphere or being swayed by new impulses. When the correlation or attunement is perfect the instincts of mother and child fit like lock and key. To change the figure, like clocks beating synchronously the instincts of parent and child are generally in harmony, but one of the clocks occasionally gains or loses, stops or runs down; one term is liable to be weak or to drop out altogether, so that there is an overlap or a gap in the series which may be serious. On the other hand, one term may be unduly strengthened, like nest building or incubation, and a preceding or following term correspondingly weak. In all such cases there are eccentricities of conduct, which, if not fatal to the young, are very puzzling to the naturalist.

Most wild birds normally pass one reproductive cycle in the season; a certain number, however, begin, but do not complete a second cycle; further, many like the robin and bluebird not only begin but complete a second and even a third cycle within the breeding period.

The repair of the old nest in autumn by fish hawks or eagles is not done 'in anticipation of spring,' and implies no more intelligence than the building of the original nest. It is simply the recrudescence of the building instinct, due to the beginning of a new reproductive cycle which is never finished.

Leaving the young to perish in the nest in autumn is brought about by the scamping of the cycle at the other end. The migratory impulse overlaps and replaces the parental instinct.

An adult robin has been seen to offer a string to its fully grown young, and try to cram it down the throat of the fledgling. Later, the old bird flew with the string into a tree. This was the result of the overlapping of two reproductive cycles, or of the last term of one cycle, and the first term of a succeeding cycle. The bird was alternately swayed by opposing impulses, now being impelled to gather nesting material, when she picked up the string, now by parental instinct to feed her young, when she tried to serve it, and again possibly by the instinct of building when she flew with the string into a tree.

Building more than one nest can be accounted for by excessive development of the building instinct, or by the influence of fear repeatedly interrupting the cycle, together with attachment to nesting site, but the discussion is too long for this abstract.

The rebuilding of nest on nest, giving rise to the wonderful storied structures sometimes produced by the summer yellow bird, or vireo, when plagued by the cowbird, so that the foreign egg is buried out of sight, is not an illustration of reason, as commonly believed, but the curious result of a pure instinct. The reproductive cycle is broken by fear, and a new one is begun, and in these rare cases the old nest is retained as a *site to be built upon.* Instead of having two *supernumerary* nests, both of which may contain eggs, as in reported cases of the phœbe, we have a series of *superimposed* nests. The new nest is not built to *conceal* the cowbird's egg, although it does this perfectly, any more than the addition of new materials to the osprey's nest in the fall is in the nature of repairs, although it answers this purpose admirably. The nest is built because the bird is at the opening of a new cycle, and is impelled by the building instinct.

Many confirmatory facts could be given. The herring gull will not only bury an egg, in rebuilding on its old site, in this case the discarded nest, when its cycle has been interrupted by fear, but will bury its dead young which it treats as so much nesting material.

Notes on the Behavior of Sea-Anemones: CHAS. W. HARGITT.

The paper discussed the aspects of behavior of several species of sea-anemones studied both under natural conditions and those of the laboratory. The points chiefly under observation had reference to the behavior of these creatures under the influence of light. So far as known few details along this line have been recorded.

At least three species of anemones were found which showed very evident reactions to photic stimuli, namely, *Eloactis (Halcampa) producta, Sagartia modesta* and *S. leucolena.* Of these two are tube-dwelling, burrowing in the sand near tide lines, and forming rude tubes or burrows through the adhesive secretions of the ectoderm. *S. leucolena* is occasionally found in similar habitat, though chiefly adhering to rocks or among colonies of ascidians, or sponges, on piles of docks, etc. Experiments showed that the first two species are most sharply responsive to light, and this sensory sense is located chiefly in the tentacles and oral regions of the body. *S. leucolena,* while less sensitive, is yet evidently so in strong light. Exposed to direct sunlight it quickly closes up into a hemispherical mass, or creeps over the edge of the rock or shell into shaded portions, of the aquarium. In its native haunts it may be found protruding its crown of tentacles from a crevice while the body is hidden.

Sagartia luciæ is a free-living species found abundantly almost everywhere, on rocks in open pools, or on floating fucus, and freely exposed to direct sunlight, action of waves, etc. Of similar habit is *Metridium marginatum.* Neither of these species seems in the least degree responsive to photic stimuli. Under a strong beam of sunlight reflected directly upon them for ten minutes they showed no response whatever.

These facts, together with others as to food-habits, etc., render it quite certain that their behavior is due to several factors, and that in response to light there is an evidence of adaptation involving varying physiological conditions, of which the burrowing habit is one of several expressions.

The Simulation of Death by Fishes: ULRIC DAHLGREN, Princeton University.

Spawning Behavior and Sexual Dimorphism in Fundulus heteroclitus and Allied Fish: H. H. NEWMAN, University of Michigan.

Some Points in the Development of the Florida Alligator: ALBERT M. REESE, Syracuse University.

External Morphology of the Head of Limulus: WILLIAM PATTEN, Dartmouth College.

The Function of the Gastrolith of the Lobster: L. W. WILLIAMS, Harvard Medical School.

The gastrolith appears for the first time in the fourth stage lobster at or before the middle of the period between the molts and, after the molt, there proceeds, *pari passu* with the dissolution of the gastrolith, a hardening of the gastric teeth, the mandibles, and the chelipeds. Soon after the absorption of the gastrolith the newly molted lobster attacks and eats the greater portion of its cast. Reasoning from these facts, we suggest that the lime in the gastroliths is reserved for the rapid hardening of the teeth, mandibles and chelipeds so that the relatively vast supply of lime in the slough and in other shells may be made available, at once, for the hardening of the new shell.

This paper is to appear in the Report of the Rhode Island Commission of Inland Fisheries, now going to press.

The Artificial Production of a Single Median Eye in the Fish Embryo by Means *of Sea-water Solutions of Magnesium Chlorid:* CHARLES R. STOCKARD, Columbia University.

Fundulus embryos when developed in certain strength solutions of $MgCl_2$ in sea-water form a large single median eye. This condition is comparable to the one eyed human monsters known as *Cyclops, Cyclopia* or *Synophthalmia.*

The single eye results from an antero-medio-ventral fusion of the elements of the two optic vesicles at an early developmental stage. This fusion is more or less complete in the different embryos.

The large compound optic cup induces the formation of a single lens. This lens is formed from ectoderm different in position from that of the normal lens-forming region. The lens is abnormally large in size as is also the optic cup, and the size of the former varies directly with that of the latter. It is probable that there is no localization of lens-forming substance in the ectoderm of the fish embryo. This inter-relationship in the development of the optic cup and lens is interestingly compared with the processes of development in the amphibian eye as shown by recent experiments.

Mixed sea-water solutions of $MgCl_2$ and NaCl also cause the one-eyed condition. Since such a defect is characteristic of the $MgCl_2$ action when used in sea-water solutions one must infer that the Mg constituent in the mixture is responsible for the result.

Method of Making Series of Anatomical Drawings: G. A. DREW, University of Maine.

The Influence of Direction vs. Intensity of Light in Determining the Phototropic Responses of Organisms: LEON J. COLE, Kingston, R. I.

The large land planarian, *Bipalium kewense,* was the principal animal experimented with. Its responses were first tried to shadows from a light directly overhead, *i. e.,* non-directive. It was then tested in a *partial* shadow, a strip of less intense light in an area of more intense illumination. In this case all the light came from one direction, namely, horizontally, from one side. Although strongly negative, the worms would crawl directly *toward* the light in the partial shadow rather than turn out into the greater intensity. A similar result was obtained with the earthworm (*Allolobophora fœtida*). In these experiments *Bipalium* and *Allolobophora* appeared to respond to *intensity* alone, regardless of the direction of the impinging light.

Chromatin Changes in Hydroids: W. M. SMALLWOOD, Syracuse University.

The Sexual Phase of the Life Cycle in Amœba: M. M. METCALF, Oberlin College.

The Existence of an Organ of Equilibration in Certain of the Lower Crustacea: C. O. ESTERLY, Harvard University. (Introduced by E. L. MARK.)

The Habits and Life History of Cryptobranchus allegheniensis: BERTRAM G. SMITH. (Introduced by Dr. O. C. GLASER.)

The adult *Cryptobranchus* has its dwelling place in a cavity or cavern under a large rock, in swift and shallow water. The animal seldom comes out during the daytime, except during the breeding season. The eggs are laid and fertilized during the first two weeks in September. They are deposited in the usual dwelling-place of the animal. About 450 eggs are laid by a single female. Fertilization is external as in fishes; no spermatophores are formed. After the eggs are deposited they are usually guarded for a time by the male, who fights and drives away other hellbenders which attempt to eat the eggs. The male himself eats some of the eggs, but on account of the slowness of his digestion is unable to eat more than a small proportion, hence his presence is in the main protective. In defending the eggs the male is merely guarding his own food-supply; the origin of the brooding habit in this case seems to be the feeding habit. The eggs hatch about six weeks after fertilization. The newly hatched larva is about 25 mm. long, and has a large yolk sac. Larvæ kept in the laboratory for two months after hatching retain a remnant of the yolk sac, and refuse food. Year-old larvæ are 6–7 cm. long, and retain the external gills. Larvæ two years old are about 12 cm. long and the external gills are greatly reduced. Sexual maturity is attained with a length of about 34 cm. and probably requires three or four years.

Relations between Regeneration, the Degree of Injury, and Moulting in Young Lobsters: V. E. Emmel, Brown University.

The phenomena of regeneration and moulting in the lobster present two distinct processes of cellular activities. The one, moulting, is going on more or less continuously throughout the period, or cycle, between moults: the other, regeneration, may be artificially induced at various points within this cycle. The problem is: what influence do these two processes exert upon each other?

A series of experiments were made on fourth stage lobsters to determine—first, the influence of regeneration upon the duration of the moulting cycle, or period between moults; second, the rate of regeneration at different stages of the moulting cycle; and third, the effect of different degrees of injury upon moulting and regeneration. The results obtained seemed clearly to demonstrate the following points:

1. That the effect of regeneration is to retard the process of moulting; and that this effect varies according to the time of mutilation, so that the later the process of regeneration is induced in the moulting cycle, the greater is the duration of the period between moults.

2. That, on the other hand, the rate of regeneration varies also according to the time of mutilation, so that the later the mutilation is made in the cycle, the more rapid is the rate of the ensuing regeneration.

3. That the greater the degree of injury, the slower the rate of regeneration, and the greater the duration of the moulting cycle.

These experiments, therefore, indicate that there is an interaction between the two processes of regeneration and moulting, of such a nature that the introduction of one will disturb the normal activity of the other. Since, also, this interaction varies at different times in the moulting cycle, it emphasizes the importance of taking this factor into account when drawing conclusions from experiments made upon crustacea and other animals which undergo ecdysis.

C. Judson Herrick,
Secretary

SCIENTIFIC BOOKS

L'Attention. By. W. B. Pillsbury. Paris: Doin, 1906. 8vo. Pp. 304. Bibliothèque Internationale de Psychologie Experimentale.

A sufficient number of the fifty volumes that are to form this series of handbooks of experimental psychology have appeared to justify the plan of the whole and to demonstrate their serviceability. Professor Pillsbury's volume on the attention is well conceived and well executed; it is so particularly from the point of view of the student, and thus will be a welcome addition to the pedagogical literature when available in English.

It is quite inevitable that such of the topics as are not divided by fairly settled contours of material should largely encroach upon one another's field. Attention can not be considered without equally considering the associative processes, the memory, perception, imagination and the other accepted rubrics of an academic psychology. It is accordingly the attentive side of consciousness, the attentive aspect of the mental moment and the mental movement that is thus singled out for monographic presentation. Professor Pillsbury's presentation and his conclusions alike bring this relation prominently before the reader, and produce the feeling of studying interesting aspects of a natural psychic species, not of a dissected specimen.

The volume falls into two portions; the one concerned with the exposition of the data, the other with their theoretical interpretation. In the former portion the essentially psychological *Leitmotiv* is well maintained, and physiological data (or suppositions) are never allowed to obscure or replace the essential fact that our knowledge of the attentive life is introspectively derived, though exercised upon objectively definite situations and measurably subject to verifiable experimentation. Attention is an expression of the emphasis and selection of the possible stimuli or occupations of consciousness by which the mental movement takes its direction, guides its course and shapes its progress. It brightens and clarifies a portion of the field, creates momentary foci, gives definiteness and contouring to the mental play, converts it from a nebulous monotone to a significant though shifting chiaroscuro. It is not a vague, formal or abstract concept, but is embodied in the mode of action of the nervous system as the mechanism of the mind. It thus has interesting motor accompaniments, that adjust the perceptive mechanism to finer, more discerning service, that quicken the intensity of the mental moment, and reveal their existence in independence or defiance of volition. Attention finds its course determined by all sorts of conditions; yet notably these divide according as they are objectively characteristic (the intensity and accumulative force of the appeal) or, more influentially, by the subjective factors. These really summarize the entire life history of the race and the individual, his heredity and social heritage, his temperament and naturally his momentary condition of mind and body. Interest is but a gauge, not a creator of attention; and to say that we attend to what is interesting but calls attention to the underlying community of many of these factors. Nor is attention explained by its motor accompaniments and expression. These, like the scope of the attentive searchlight, like the fluctuations under fatigue, reflect its close dependence upon physiological conditions. Attention guides and selects in the sensory as in the intellectual field, and thus becomes an expression of the mental totality or consciousness. It plays a like part in the representative field of memory and association as in the presentative field of sensation and perception; for in truth all these processes shade into one another; and their composite nature characterizes the whole mental life.

On the side of theory the most important issue is the rôle of apperception, whose functional efficiency is recognized by the subjective aspects of the attentive process. The varieties of formulation of the 'apperception' theories are so various and the differences between them so elusive, that the reader will be grateful for Professor Pillsbury's guidance, which includes as well a survey of the historical field. Next in importance is the motor theory of attention; while each of these types and their varieties takes note of—as certain theories exclusively consider—the physiological bases of attention. Professor Pillsbury's view has the merit of merging the points of emphasis of the several explanations and of presenting the attentive process not as an isolated faculty or function, but as an aspect of the totality of the natural mental state.

Reduction of these conclusions to a phrase or an outline is impossible; and the reader must be referred to Professor Pillsbury's brief résumés for a suggestion of the theoretical and controversial aspects of the attention. These, when closely considered, reveal their intensely academic character; and an adherence to one or another depends upon the bent of one's philosophic allegiance and temper.

JOSEPH JASTROW

The Value of Pure Water. By GEORGE C. WHIPPLE. New York: John Wiley & Sons. Pp. 84. Price, $1.00.

This small publication, which contains much material of both interest and value, is practically a reprint of portions of three earlier papers by the author. One from 'Biological Studies by the Pupils of William Thompson Sedgwick,' another from 'The Pollution of Streams and the Natural Agencies of Purification' and a third on 'The Disadvantages of Hard Water.'

Among the qualities of a public supply which affect the consumer 'temperature' is included. This is well, for that item receives far too little attention from those who forget that the great bulk of the people can not afford the luxury of ice.

As showing the advantages of filtration, a comparison between the typhoid rates in Albany and Troy is striking, but it should be noted that Troy never drew Hudson River water from below the mouth of the polluted Mohawk, and now takes no river water at all. In showing the pecuniary loss to a community due to water-borne typhoid, the author places the 'residual typhoid,' or number of yearly deaths not traceable to water, at a probable value of 20 per 100,000. He adds, however, that this value will doubtless diminish in the future because of a gradual decrease in the number of foci of infection. A good table is given showing the increase in cost to the laundry interests resulting from the use of hard water, and a formula is added whereby may be calculated the depreciation of the money value of a water for soap users because of hardness.

Additional formulæ are given which severally state the depreciation due to 'sanitary quality' to 'temperature' and to 'physical characteristics,' under which latter head are included 'turbidity,' 'color' and 'odor.' Odor is again divided into that due to 'organisms,' to 'decomposition' and to 'vegetable odors.'

This is all well enough, but the resulting complexity of formulæ is somewhat more than the average water purveyor might wish for.

"Habit and association have much to do with a person's views as to the attractiveness of water" is a most true statement, and upon it depends the success with which many an indifferent supply is now offered to the public.

The book is well worth its price and should be found in every water library.

W. P. MASON

Alcohol—The Sanction for Its Use Scientifically Established and Popularly Expounded by a Physiologist. Translated from the German of Dr. J. STARKE. New York, G. P. Putnam's Sons. 1907.

This book, written in defense of the use of alcohol, appears at a time when there is a world-wide movement in favor of a stricter temperance. By alcohol the author means the substance as contained in the purer beverages, not such concoctions as absinth which are compared to alcoholic solutions of opium.

The moderate drinker who experiences 'internal mental exaltation with perfectly clear consciousness' has no poisoning of the brain provided it is only occasionally that he gets 'elevated.' The book claims that a medium amount of alcohol is favorable to the performance of muscular work, and a medium allowance is put at 560 c.c. of absolute alcohol or two and three quarter pints of brandy for a man weighing 140 pounds. The author states that caffein constricts the cutaneous blood vessels and enlarges those of the interior, and since alcohol behaves in the opposite manner, therefore rum should be taken in tea and a liqueur after coffee.

While this volume will scarcely meet with unanimous approval, it might still be recommended as an antidote to the attenuated nonsense of the 'scientific temperance' of the school books.

GRAHAM LUSK

SCIENTIFIC JOURNALS AND ARTICLES

The American Museum Journal for April contains illustrated accounts of the 'Habitat

Groups of Birds,' 'The Museum's New Whales' and 'The Results of the Tjader Expedition.' It also notes that Mrs. Roesler has been appointed as guide to the collections for members visiting the museum. In this connection it may be noted that the Museum of Fine Arts, Boston, has just added to its staff a *Docent* whose office is to take parties, not exceeding ten in number, about the museum and explain the collections.

The Zoological Society Bulletin for April is termed the Aquarium Number and is devoted entirely to aquatic animals. It contains articles on 'The Care of Goldfishes' and 'The Care of Turtles and Small Alligators,' 'The Centennial of the Aquarium Building' with a cut showing its appearance in 1852. Other articles are on 'Poisonous Fishes,' 'The Drum Fish,' 'Blind Fishes' and 'The Uses of the Fins of Fishes.' The measurements are given of two unusually large sea turtles, one a loggerhead, weighing 395 pounds, the other a green turtle, weighing 540 pounds.

Bird-Lore for March-April has for its principal articles 'The House-Finch from an Office Window,' by W. H. Bergtold; 'Bird Clubs in America, III., The Maine Ornithological Society,' by J. Merton Swain; 'Clay Bird-Houses and Bird-Baths,' by R. W. Hegner, and the second paper on 'The Migration of Thrushes,' by W. W. Cooke. The Educational Leaflet, by Mabel Osgood Wright, is on 'The Red-winged Blackbird.' The report of the Audubon societies gives a résumé of various laws proposed or passed.

The Museums Journal of Great Britain for March contains various contributions to the discussion on museum cases which formed a feature of the last meeting, including a description of a 'Rotary Cabinet for Museum Specimens,' by Rev. S. J. Ford. This cabinet contains 22 trays which may be successively brought to the top, which is glazed, in order that their contents may be seen.

ANNOUNCEMENTS have been sent out from Bologna, Italy, of the publication there in the immediate future of a new scientific review, to be called *Rivista di Scienza,* which is described by the editors as 'an international review of scientific synthesis.' This new publication is not intended to present the results of special investigation in narrow fields, but rather to take a wide look over all the lines of scientific activity and to review in each in as clear and simple a way as possible the work which is being done and the problems which are coming up. Biology, chemistry, physics, mathematics, geology, sociology, political economy, psychology and pedagogy are all represented in the list of articles which are to appear in the first numbers. Subjects of general interest in all branches of scientific work will be presented, and treated in a manner as little technical as possible in order that they may be intelligible to a wide circle of readers. The correlation and connection between different groups of sciences are to be particularly developed. It is to be cosmopolitan in its outlook and almost every country in which scientific work is being done is already well represented in the list of contributors. "It is born," say the editors, "from the desire to coordinate the work carried on in different fields of knowledge and to make the task of synthesis easier" and "it invites such studious persons as are desirous (without sacrificing time employed by them in the certain way of analytical research) of discussing in its columns the general questions regarding their special branch of science, to set forth in a widely accessible form the results obtained from it. It hopes especially to have the collaboration of those who desire to study the relationship, ever new and ever closer, which exists between the different branches of study, and it counts on the favor of all those who recognize the danger of excessive specialization and would be glad to promote a wider appreciation of the problems of science." It is to be issued quarterly and each number will contain from 150 to 200 pages, forming an annual volume of from 600 to 800 pages. Two editions are to be issued: one, an international edition in which each article will appear in the language of its author, and another intended for circulation in Italy in which all articles in any foreign language other than

French will be translated into Italian. The committee of management is made up of G. Bruni, A. Dionisi, F. Enriques, A. Giardina and E. Rignano. Among those who have already undertaken to contribute articles are: Bredig, Ciamician, Ostwald and Wegschneider in chemistry; Borel, Larmor and Thomson in physics; Boutroux, Fano, Picard and Severi in mathematics; Delage, Giard, Grassi, Hartog, Raffaele, Roux (W.) and Sedgwick in zoology and anatomy; Abegg, Burian and Dastre in physiology; Darwin, Haberlandt and Wiesner in botany; Benini, Bortkiewicz, Carver, Mosca, Pareto, Sombart and Supino in sociology and political economy; Brugi, Groppali and Scialoia in law; DeMarchi, Love and Wallerant in geology; Binet, Janet and Lugaro in psychology; Cunningham and Salvemini in history, and Credaro and Tannery in pedagogy.

SOCIETIES AND ACADEMIES

AMERICAN MATHEMATICAL SOCIETY

AT the regular meeting of the society, held at Columbia University on Saturday, April 27, an especially attractive feature of the program was Professor W. F. Osgood's presidential address on 'The Calculus in our Colleges and Technical Schools.' The address will appear in the June number of the *Bulletin* of the society. The attendance at the April meeting, always ranking next to that of the annual and summer meetings, exceeded on this occasion all previous records, amounting to about seventy, including sixty-one members.

President H. S. White occupied the chair at the two sessions. The council announced the election of the following new members: Dr. Alfred Ackermann-Teubner, Leipzig, Germany; Dr. J. W. Bradshaw, University of Michigan; Professor H. E. Cobb, Lewis Institute, Chicago, Ill.; Mr. S. A. Corey, Hiteman, Ia.; Professor Floyd Field, Georgia School of Technology; Mr. G. W. Hartwell, Columbia University; Chancellor C. C. Jones, University of New Brunswick; Mr. Joseph Lipke, Columbia University; Professor Francis Regis, Christian Brothers College, St. Louis, Mo.; Mr. H. P. Stillwagen, Yeatman High School, St. Louis, Mo. Seven applications for admission were received. The total membership of the society on May 1 was 560.

Following the plan recently adopted, abstracts of the papers so far as available had been printed and issued to the members in advance of the meeting. In this way it is hoped to secure a more intelligent interest in the papers and to promote criticism and discussion.

The date of the summer meeting, to be held at Cornell University, was fixed for Thursday and Friday, September 5 and 6.

By no means the least enjoyable feature of the meeting was the usual dinner in the evening, attended on this occasion by over twenty of the members.

The following papers were read at this meeting:

G. A. BLISS: 'A new form of the simplest problem of the calculus of variations.'

R. D. CARMICHAEL: 'Multiply perfect even numbers of five different primes' (preliminary communication).

L. P. EISENHART: 'Transformations of surfaces whose lines of curvature are represented on the sphere by an isothermal system.'

F. L. GRIFFIN: 'The variation of the apsidal angle in certain families of central orbits.'

F. L. GRIFFIN: 'The solutions of central force problems as functions of the constant of areas.'

F. L. GRIFFIN: 'Note on a simple example of a central orbit with more than two apsidal distances.'

G. A. MILLER: 'Note on the commutator of two operators.'

J. E. WRIGHT: 'Arrangement of ovals of a plane sextic curve.'

W. F. OSGOOD: Presidential address—'The calculus in colleges and technical schools.'

IDA M. SCHOTTENFELS: 'Group matrices.'

C. E. STROMQUIST: 'An inverse problem of the calculus of variations.'

R. G. D. RICHARDSON: 'On the integration of a series term by term.'

A. L. UNDERHILL: 'Invariants of the function $F(x, y, x', y')$ under point and parameter transformations connected with the calculus of variations.'

EDWARD KASNER: 'The motion of particles under conservative forces.'

Edward Kasner: 'Isogonal and dynamical trajectories.'

P. L. Saurel: 'On the distance from a point to a surface.'

T. E. McKinney: 'On concylic quantics.'

T. E. McKinney: 'On continued fractions representing quadratic irrationalities.'

G. A. Miller: 'Groups generated by n operators each of which is the product of the $n-1$ remaining ones.'

F. N. Cole,
Secretary

THE AMERICAN CHEMICAL SOCIETY. NORTHEASTERN SECTION

A Correction.—In the report of the proceedings of the seventy-fifth regular meeting of this section (this journal, p. 669), through a mistake of the undersigned, the following statement appeared: "For three semesters, the speaker was Wöhler's assistant and took part in the researches on aluminium, silicon, boron, etc." * * * Dr. Remsen was not Wöhler's assistant and did not take part in the researches on these elements, but did later make them under Wöhler's personal direction, by methods which had already been worked out. The speaker left Göttingen in 1870 and became assistant to Fittig at Tübingen. With a view to correcting the false impression given by the above-mentioned report, these few lines are put forth with the hope that they may fall under the eyes of most of the readers of the original report.

Frank H. Thorp,
Secretary

DISCUSSION AND CORRESPONDENCE

THE FIRST REVISER OF SPECIES

I have followed with much interest the recent discussion in Science by Messrs. Stiles, Stone, Jordan and Allen on the proposed new rules in zoological nomenclature. The subject is one of especial concern to me at the present time as involving the propriety of numerous generic names in a work now publishing. I wholly agree with Dr. Allen in his views regarding elimination, and concur quite with his statement that elimination is practically the only rule in use by systematists, at least so far as that especial group of insects with which I am best acquainted is concerned. I believe that, upon the whole, it is the safest and most expedient rule, and one which meets the approbation of most taxonomists. Next to this I would accept the rule of the 'first species,' one that has often been used by entomologists, especially where there has been no doubt as to the meaning of the original describers.

But the first species rule would be unjust when applied to certain writers. It is well known that Meigen, the 'father of dipterology' did not consider the first species as the most typical of his genera, but rather, with the last species, as the most aberrant, and these were the ones he usually figured. Wiedemann, a very prolific describer of exotic diptera on the other hand, arranged his species in his genera usually in the order of their size, and the first here would not in the least represent his most typical species.

As to the rule of the 'first reviser,' when applied to work done in the past, I consider it vicious; so utterly unjust and revolutionary that it is to be hoped it will be stifled in its birth. I, for one, shall never recognize it. Its chief use would be to give unlimited license to the library naturalist, now that 'new genera' are not so common as they were. I will mention a single instance of the effect it would have in a case that has recently been brought to my attention. There is perhaps no genus of flies better known, save *Musca*, than the genus *Syrphus*. Fabricius named the genus in 1775, giving a list of numerous species belonging to it, a composite genus of course, as all of Fabricius's genera were. In 1839, one Curtis, knowing little, critically, of diptera, in a general work on British insects, capriciously designated the nineteenth of Fabricius's species as the 'type' of *Syrphus*. In 1860, Schiner, perhaps the ablest student of diptera, and one of the most conscientious that we have ever had, subtracted one of these original species, which happened to be this 'type' of Curtis, as the type of a new genus *Leucozona*. The genus *Syrphus*, the type of the family Syrphidæ, with all its eliminations, now comprises a hundred or two species distributed in nearly all parts of the world. The genus *Leucozona* includes a single species, possibly two. In accordance with this iniquitous, *ex post facto* law of the 'First Reviser' it is now proposed to apply the name *Syrphus* to this single species and to give to the hundreds now called by that name, the name of a synonym made years ago by the greatest blunderer that ever wrote on entomology. Schiner was remarkably conscientious, following the usages and rules of his time closely. He, of course, could not imagine that the future historical naturalist would impose so absurd a rule as would make the carelessly designated and wholly unwarranted 'type' of Curtis compulsory; did not dream that it was necessary for him to look through the writings of every author of high and low degree to see whether Fabricius's types had been arbitrarily fixed. His work was done in good faith.

This is but one example of the workings of this newly proposed, *ex post facto* law. There are scores of others not unlike it; in fact, dipterology will be a small chaos until all the present works on the science have been rewritten, and a paradise of the name tinkerer, if such a rule obtains.

I should not object to the 'first species' rule, if it were not made retroactive in such cases as would upset other names established by elimination. Surely those of the past who have done able and conscientious work under accepted usages should not be stigmatized at the caprice of any self-constituted authority. And what assurance have we that a few years hence some other *ex post facto* law will not be invoked to do the work all over again? New writers will have little opportunity to propose new generic names unless some such historical mine is opened up.

I really believe that the final solution of the ever-growing controversies and apparently never-ceasing changes will be some such commission as Dr. Davenport has recently suggested, an accepted commission to pass upon the validity of names without regard to priority or anything else. And one of the first rules that I should attempt, were I a member of such a commission, would be that he who digs up a name that has been buried for fifty years to replace some other in common use, should be ostracised and debarred from all further use of reputable scientific journals.

S. W. WILLISTON

TYPES OF GENERA BY FIRST SPECIES

IN a recent article[1] it is claimed that the first species method is opposed to the law of priority, since it supersedes the action of the first reviser. It is only necessary to reply that the action of the original author always precedes that of any possible reviser, and since the first species method determines the type of the genus solely from the first publication of the original author, it is obviously more in accord with the law of priority than any other method.

The same writer makes the surprising statement that the method of elimination and that of the first reviser are parts of one method. As a matter of fact, they are almost diametrically opposed. The elimination method, or the method of residues, tends to leave as the type of the original genus the one left last after all removals. This is usually the most obscure or unrecognizable species, since the more prominent ones are generally first selected as the types of new genera, or are otherwise removed. The first reviser method, or that of the nomination of types, tends to select some prominent species as the type of the old genus, since such will naturally be first selected by some later author as an illustration. These two opposed rules are, unfortunately, capable of being mixed in various ways (one of which is illustrated in the article here referred to), allowing of almost an infinitude of methods of selecting types. It is this extreme and most undesirable latitude in the rules that renders those most lately promulgated so unsatisfactory and impracticable.

HARRISON G. DYAR

U. S. NATIONAL MUSEUM,
April 19, 1907

A SHEEP-GOAT HYBRID

WHAT seems to be a hybrid between a sheep and a goat was produced this spring on the

[1] SCIENCE, n. s., XXV., 625, 1907.

farm of Mr. E. Arnaud, Monett, Mo. Mr. Arnaud maintains a herd of sheep and with them keeps two goats, a male and a female. There is only one female goat on the place, and she brought a kid three weeks after the animal in question was born. The hybrid is a twin to a lamb that is not a hybrid. The maternity of the supposed hybrid is not absolutely certain. Mr. Arnaud found the lambs when they were perhaps an hour old. No other sheep or goats were near, though there were others within the same enclosure. The ewe evidently regarded both the animals as her progeny. The twins are inseparable, one being an ordinary lamb, the other in most respects a goat. The tail is intermediate in length between that of a sheep and a goat, and the ears closely resemble those of a sheep. The coat is apparently that of a goat. The male goat on the farm is of mixed breeding and is white with a few reddish hairs showing on the upper part of the neck. The supposed hybrid has most of the hairs of the body of this reddish color. Mixed with them are much shorter hairs which appear like white wool. They have not yet been submitted to examination to ascertain their real nature.

While the evidence is not absolutely conclusive, there is strong reason for believing this individual to be a hybrid. Mr. Arnaud fully appreciates the importance of the freak, and will preserve it for future study and experiment. The writer would greatly appreciate information concerning other hybrids of this character.

W. J. Spillman

U. S. Department of Agriculture

SPECIAL ARTICLES

THE SIGNIFICANCE OF LATENT CHARACTERS[1]

Those of you who were present at the last annual meeting of the Botanical Society, at New Orleans, will remember that I presented a paper upon the latent characters of a white bean, showing that the appearance of two new characters in the F_1 hybrid offspring of a white bean when crossed with a plain brown or yellow bean, demonstrated the presence of a color-pattern, and of a pigment-changer as 'latent' characters in the white bean, latency meaning simply *invisibility* and not dormancy. On this basis it was predicted that in the second generation five forms would appear according to the well-known tripolyhybrid ratio, 27:9:9:3:16. These forms in the order of the ratio are purple mottled, black (dark purple), brown mottled, brown, and white. I show you to-day samples of these five predicted types taken from the second generation.

The ratios of these several groups have not yet been determined because not all of the material has been worked over, but the presence of the predicted types—especially the presence of the two forms, plain black and brown mottled, which were not known to have ever occurred in the ancestry on either side—sufficiently demonstrates the correctness of my interpretation of the allelomorphic composition of the parents. Some additional unexpected types were found which must await further breeding experiments before their significance can be profitably discussed.

It will be remembered that the condition I assumed for these hybrid beans was used to bring into harmony with simple Mendelian hybrids the apparently anomalous results of Tschermak, Emerson, Lock, Bateson, Correns, Cuénot and Castle. The prediction that the same conception of latent characters in the sense of *invisible*, not *inactive* ones would without doubt give a solution to the intricate and otherwise apparently inexplicable behavior of stocks and sweet-peas, as studied by Bateson, was fulfilled with unexpected promptness, as the third report[2] to the Evolution Committee presented in March, 1906, and published later in the same year, adopts the same theory and shows that in this way practically all of the apparent anomalies of stocks and sweet-peas may be explained upon the simple basis of typical Mendelian behavior without recourse to the hypallelomorphs or compound units earlier assumed by Bateson.

[1] Read before the Botanical Society of America, at New York, December 29, 1906.

[2] Bateson, W., Saunders, Miss E. R., Punnett, R. C., 'Experimental Studies in the Physiology of Heredity,' Reports to the Evolution Committee of the Royal Society, Report III., 53 pp., London, 1906.

This complete demonstration that latent characters, at least in many cases, are not inactive units that may be rendered active by some unknown influence, but are, instead, units that produce a visible character only when acting in conjunction with one or more other units, justifies me in calling attention again to the significance of such characters.

In order to see the bearing of these results upon the process of evolution it is necessary to realize that what we call a unit character is not necessarily produced by the activity of a single allelomorph, and I consider it probable that few visible characters are so produced. It makes no difference how many internal units are involved in the production of any so-called unit-character, so long as there is a difference of only one unit involved in the cross. Thus, allelomorphs *ABCDEFGH* may determine a single characteristic and *ABCDEFGh* an alternative characteristic. If plants having characters so determined are crossed together, they will behave as if these were unit characters, though according to our assumption one is determined by the presence of eight dominant units, the other by seven.

The best actual examples we now have of the compound nature of certain apparently simple external characters are seen in the splendid results of Professor Bateson's studies on stocks and sweet-peas. In stocks, for instance, canescence is found to depend upon the simultaneous presence of three dominant allelomorphs wholly uncorrelated and each acting in the normal Mendelian manner. In one strain of sweet-peas two such dominant units are necessary to produce any color whatever and another unit determines whether that color shall be blue or red. This condition produces the remarkable result that the first generation hybrid between two white-flowered parents have blue or red flowers.

Similar conditions were presented in two of the papers given yesterday (December 28, 1906) on the joint program of Sections F and G of the American Association for the Advancement of Science, viz., the appearance of a 'latent' agouti factor in certain guinea-pigs, and an invisible red factor underlying black in certain fowls as reported by Dr. Castle[3] and Dr. Davenport.[4] The characters of both these apparently anomalous hybrid products were recognized as atavistic or reversionary. The same is true of the purple-flowered hoary stocks produced from glabrous white and glabrous cream-colored strains. The same was true of flower and seed-coat color of beans and peas as found by Tschermak and Lock, and is no doubt the correct explanation of the purple mottling in my hybrid beans. Indeed, so many instances are now on record in which a cross results in reversion, that generalizations can be made with some degree of security.

These reversions indicate that the original character was *compound*, being determined by the simultaneous action of two or more, possibly many, dominant units, and that the later specific or varietal derivatives were produced by the disappearance of one or more of these original units as a dominant characteristic. Thus in the example assumed above in which the original character was determined by the dominant units *ABCDEFGH*, the later derivatives may be *ABCDEFGh*, *ABCDEFgH*, *ABCDEfgH*, etc., through all the possible permutations. May we not perhaps get in this way a comprehensive view of at least the later stages of evolution as a process of analysis due to the disappearance of one unit after another?

All the visible variations of the present plant and animal world were once *in*volved in some generalized form or forms, and the process of differentiation pictures itself to us as a true process of *e*volution brought about by the change of individual character-determining units from a dominant to a recessive state. This conception results in an interesting paradox, namely, the production of a new character by the loss of an old unit.

When I first became interested in the Mendelian discipline one of the most difficult things for me to understand was the fact that, somehow, every dominant character in a plant or animal finds its recessive counterpart in

[3] Castle, W. E., 'On a Case of Reversion Induced by Cross-breeding and its Fixation.'

[4] Davenport, C. B., 'Reversion.'

all of its near relatives not possessing the character in question. For a time credulity balked and I was compelled to look upon character-units as figures of speech. The origin of forms from a common parent by the loss of dominancy in its several character-determinants accounts for the general presence of a recessive unit, corresponding with each dominant unit, in all the nearly related forms.

No suggestion has been made as to the nature of the change by which a dominant allelomorph becomes recessive, but if this change be looked upon as a degenerative one which may be followed later by complete disappearance of the unit it would account for the fact that hybrids between nearly related forms are usually Mendelian, while those between more distant ones are not.

I may summarize briefly as follows:

(*a*) What appear to be unit characters may be, and probably usually are, compound characters.

(*b*) New characters appear by the change of one or more character determinants from the dominant to the recessive condition.

(*c*) Some of the partial products resulting from this process of analysis have no externally apparent distinguishing characteristic, and these supply instances of so-called 'latent' characters.

(*d*) Mendelian hybridization results in an F_1 which is a partial or complete synthesis of an ancestral condition.

(*e*) This conception gives an explanation of the general presence of recessive units corresponding to the dominant units in each closely related form.

(*f*) If the change from the dominant to the recessive condition is a degenerative process which may be followed by complete disappearance of a unit, an explanation is found for the fact that Mendelian behavior is a function of nearly related forms but not of more distantly related ones.

George Harrison Shull
Station for Experimental Evolution,
Cold Spring Harbor, Long Island,
December, 1906

CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

THE LOP-NOR DESERT

Ellsworth Huntington continues his papers on his recent explorations in Eastern Turkestan with a discussion of 'Lop-Nor—A Chinese Lake,' in the *Bulletin of the American Geographical Society* for February and March, 1907. Additional evidence is adduced regarding what seems to Huntington to be a progressive desiccation of the region within historical times. At Miran the ruins of an ancient Buddhist town, perhaps 1,500 years old, were discovered, covering an area of over five square miles. The town probably had a population of some thousands, but the "modern water supply is only sufficient to support seventy or eighty people." The saline water which the camels have to drink affects their flesh so markedly that the meat becomes 'corned' by reason of the salt accumulating in the animals' bodies from the water. The journey across the old lake bed was very tedious and difficult by reason of the irregularity of the large rock-salt blocks which cover the surface. Huntington remarks particularly upon the ability of his camel-man to endure hardship and fatigue with a minimum allowance of food and water. On one occasion the man traveled fifty miles in twenty hours without nourishment or water. This effect of a desert life in hardening man to the endurance of hunger, thirst and fatigue, as contrasted with the easier, softer life in more humid regions or in oases, has been commented on by other travelers, notably by Nachtigal some years ago. The history of Lop-Nor during the last 2,000 years seems to Huntington to show the following stages: First, a comparatively large lake, said to measure seventy-five miles each way. Next, during the early centuries of the Christian era, an increase in the recorded size of the lake, which can not have been due to diminished use of the rivers for irrigation, for the population at that time was larger than at present. Finally, in the last few hundred years there has been a decrease in the size of the lake and in the population about it. It may here be noted that not all explorers of Central Asia are agreed as to the fact of climatic change. Thus Dr. Stein, the anthropologist, writing from Kiria to the *Geographical Journal* (January, 1907), reports concerning the desert east of Khotan that cultivation in the fertile Hanguya tract is steadily advancing towards areas previously abandoned to the desert, so that much of this desolate Tati is likely to be recovered by man from the desert at no distant time. Dr. Stein was much struck by the considerable extension of cultivated ground during the past six years. Extended areas lying waste or drift-covered in 1900–1, have again been brought under cultivation. The great advance in prosperity which is taking place in the western oasis of Chinese Turkestan seems to have had a marked effect in Khotan on the extent of the cultivated area and the numbers of the population.

FRANKLIN, THE KITE AND THE LIGHTNING ROD

'Did Benjamin Franklin fly his Electrical Kite before he invented the Lightning Rod' is the title of a paper read by Professor A. L. Rotch before the American Antiquarian Society in Worcester, Mass., October 24, 1906. Professor Rotch believes: (1) that the kite experiment was probably performed later than has been supposed; (2) that even before this experiment certain buildings in Philadelphia were provided with 'points,' probably as lightning conductors; (3) that prior to Franklin's first account of the kite experiment he had drawn up precise directions for placing lightning rods upon all kinds of buildings.

R. DeC. Ward

HARVARD UNIVERSITY

A MONUMENT TO LAMARCK

THE professors of the Muséum national d'histoire naturelle, Paris, have undertaken to erect a monument in the Jardin des Plantes to their illustrious predecessor, the philosopher and naturalist Lamarck. To this end they have secured subscriptions in Paris and have formed a committee to enlist the support of botanists and zoologists throughout the world.

The proposed monument, designed by M. Fagel, gives a bronze bust of Lamarck, surmounting, after the fashion of recent French sculpture, a large granite pedestal with figures in relief. These portray the philosopher aged and blind, seated at the base of his monument, and close by, reading to him, his devoted daughter—a pathetic picture of Lamarck's last days when in poverty and in disfavor, on account of his evolutional writings, he was living as a recluse in the ancient house of Buffon, near which, probably on the very spot which he crossed in his daily walk, the monument is to stand.

It is only within recent years that the position of Lamarck among the pioneers of evolution has come to be understood. Darwin himself was distinctly unjust in his treatment of him. But from the work of the modern paleontologist on the one hand and the experimentalist on the other, tribute is coming to be paid to Lamarck's wonderful insight, imperfect though the materials of his inductions were, into the processes and factors of organic evolution. The 'American school of evolutionists,' headed by Cope, Osborn, Hyatt, Ryder, Packard, has indeed touched so closely the lines of his philosophy that it has often merited the title of 'Neo-Lamarckian.' And it is to our countryman, Packard, that we are indebted for the only work upon the life and teachings of Lamarck which has hitherto appeared.

The present project in memory of Lamarck is one, in short, which may justly enlist the cooperation and support of the botanists and zoologists throughout the United States—to give by this means tangible recognition of his services to science. The American zoologists who have been designated to receive subscriptions in behalf of the Committee in Paris are Professors Alexander Agassiz, Henry Fairfield Osborn and Bashford Dean.

THE SEVENTH INTERNATIONAL ZOOLOGICAL CONGRESS

THE revised program of the Seventh International Zoological Congress, to be held at Boston, August 19–23, under the presidency of Mr. Alexander Agassiz, is as follows:

Monday, August 19.

9 A.M. Members of the congress will assemble informally at the Harvard Medical School. Registration.

12:30 P.M. Luncheon at the invitation of the Boston local committee.

2 P.M. First general meeting at the Harvard Medical School. Opening of the congress. Election of the vice-presidents and secretaries. Presentation of delegates. Arrangement of the sections. Addresses.

The International Commission on Zoological Nomenclature, Professor R. Blanchard, Paris, president, will hold its regular meetings during the sessions of the congress.

8:30 P.M. Reception.

Tuesday, August 20.

10 A.M. Meeting of sections in the Harvard Medical School.

1 P.M. Luncheon at the invitation of the Boston local committee.

3 P.M. Excursion.

Wednesday, August 21.

10 A.M. Meeting of sections in the Harvard Medical School.

1 P.M. Luncheon at the invitation of the Boston local committee.

2:30 P.M. Second general meeting at the Harvard Medical School. Business. Addresses.

8:30 P.M. Reception by Mr. Alexander Agassiz, Hotel Somerset, Commonwealth Avenue.

Thursday, August 22.

10 A.M. Meeting of sections at the Harvard Medical School.

1 P.M. Luncheon at the invitation of the Boston local committee.

3 P.M. Excursion to Wellesley as guests of Wellesley College.

Friday, August 23.

10 A.M. Meeting of sections at the Harvard Medical School.

1 P.M. Luncheon at the invitation of the Boston local committee.

2:30 P.M. Third general meeting. Business. Addresses. Close of the congress.

8:30 P.M. Subscription dinner.

Saturday, August 24—Harvard University Day.

10 A.M. Excursion to Harvard University, Cambridge. During the day the museums and other buildings of the university will be open to members of the congress.

1 P.M. Luncheon by invitation of the corporation of Harvard University.

Sunday, August 25—Woods Hole Day.

Morning. Members of the congress will leave the South Station, Boston, for Woods Hole, via New York, New Haven and Hartford Railroad. At Woods Hole the Station of the United States Bureau of Fisheries and the Marine Biological Laboratory will be visited.

1 P.M. Dinner at the invitation of the general committee.

4 P.M. Members will leave for New York via Fall River Line.

Monday, August 26—Columbia University Day.

Morning. Arrival in New York City.

12 M. Luncheon.

Evening. Reception and concert.

Tuesday, August 27—American Museum Day.

12 M. Luncheon.

Afternoon. Reception.

Evening. Smoker.

Wednesday, August 28.

Visit to the Marine Laboratory of the Brooklyn Institute of Arts and Sciences and to the Carnegie Station for Experimental Evolution at Cold Spring Harbor. Return to New York by boat or train, according to weather.

Thursday, August 29—New York Zoological Society Day.

Morning. Reception by the Zoological Society in the New York Aquarium.

Afternoon. Reception by the Zoological Society in the Zoological Park.

Evening. Reception in Columbia University Library.

Friday, August 30.

Hudson River by day. Excursion by steamer up the Hudson to West Point and Garrison as guests of Professor Henry Fairfield Osborn.

Saturday, August 31.

Visits to Yale University and to Princeton University.

Monday, September 2.

Morning. Departure from New York for Philadelphia.

Noon. Luncheon at the Academy of Natural Sciences, Philadelphia, followed by inspection of the library and museum.

3 P.M. Carriage drive to the Zoological Gardens and Fairmount Park, terminating in a supper at the Philadelphia Country Club.

Tuesday, September 3.

9 A.M. Visits to the American Philosophical Society, Independence Hall, Girard College and other places of interest.

Noon. Visit to the University of Pennsylvania, where luncheon will be served.

Afternoon. Departure for Washington.

Wednesday, September 4.

10 A.M. General meeting in the assembly hall of the Cosmos Club, Washington, at which addresses of welcome will be given by the secretary of the Smithsonian Institution, the president of the Carnegie Institution and the president of the Washington Academy of Sciences; and the details of the program for the Washington visit will be announced. This will be followed by a visit to the National Zoological Park, to the Congressional Library, the United States Department of Agriculture, the Hygienic Laboratory and other points of zoological interest.

Evening. Reception by the Cosmos Club.

Thursday, September 5.

A visit by boat on the Potomac River to Mt. Vernon, the home of George Washington and to the United States Navy Proving Station at Indian Head, with dinner at Marshall Hall.

Evening. Reception at the United States National Museum.

Friday, September 6.

Morning or afternoon. Return to New York.

On September 7, there will be an excursion to Niagara Falls and across Lake Ontario to Toronto. In case at least fifty members agree to take part in an excursion to Bermuda, one will be arranged on September 11 or 14.

SCIENTIFIC NOTES AND NEWS

MR. EDWARD B. MOORE, assistant commissioner of patents, has been appointed commissioner to succeed Mr. Frederick I. Allen, who has resigned.

PROFESSOR FRANCIS HUMPHREYS STORER, S.B. (Harvard, 1855), since 1870 professor of agricultural chemistry at Harvard University and at the same time dean of Bussey Institution, has resigned and has been appointed professor emeritus from September 1.

DR. W. L. RICHARDSON has resigned the chair of obstetrics and the deanship of the Harvard Medical School. Dr. Richardson graduated from Harvard College in 1864 and from the medical school in 1867.

PROFESSOR AIMÉ WITZ, of Lille, has been elected a corresponding member in the Paris Academy of Sciences in the section of physics, in the room of the late Professor Boltzmann.

M. DARBOUX, permanent secretary of the Paris Academy of Sciences and professor of mathematics in the University of Paris, has been named a member of the national bureau of weights and measures in the room of the late M. Berthelot.

MAJOR JAMES CARROLL, U.S.A., will receive the degree of doctor of laws from the University of Maryland at its centennial celebration to be held from May 30 to June 2.

NEW YORK UNIVERSITY has conferred the degree of doctor of laws on Dr. Joseph D. Bryant, professor of surgery in the institution and president of the American Medical Association.

DR. J. PLAYFAIR MCMURRICH, professor of anatomy at the University of Michigan, has been elected a corresponding member of the London Zoological Society.

PROFESSOR ERNST VON LEYDEN, the eminent surgeon of Berlin, has been made a privy-councillor, with the title of excellency.

MR. F. E. BEDDARD, F.R.S., has been appointed an honorary member of the New Zealand Institute.

PROFESSOR JOHN ADAMS, who holds the chair of education in the University of London, and Dr. J. M. E. McTaggart, lecturer in moral science, Trinity College, Cambridge, will take part in the work of the summer school of the University of California, which opens on June 24.

DR. WILLIAM HALLOCK, dean of the faculty of pure science, has been elected president of the Columbia chapter of the Society of Sigma Xi.

DURING the college year 1906–7 the Sigma Xi Society of the Ohio State University has offered the following course of open scientific lectures:

November 27—'The Cellular Basis of Inheritance and Evolution,' Dr. E. G. Conklin, University of Pennsylvania.

January 10—'The Source and Utilization of Our Fuel Supply,' Professor E. E. Sommermeier, Ohio State University.

March 14—'Modern Methods in Water Purification,' Mr. John H. Gregory, Engineer in charge of Improved Water and Sewage Works, Columbus, Ohio.

April 18—'The Coal Tar Products and their

Uses in the Arts,' Dr. William McPherson, Ohio State University.

Mr. A. Rolland Rainy, M.P., delivered an address on 'The Necessity for a Minister of Public Health' before the New Reform Club, London, on May 10, when the chair was taken by Sir W. J. Collins, M.P.

Professor William Wright has delivered three lectures on the 'Prehistoric and Early Historic Inhabitants of England' at the Royal College of Surgeons, London.

Professor David P. Todd, of Amherst College, sailed on the *Panama* on May 11 for Colon, Panama, Callao, Peru and Iquique, Chile, in charge of the Lowell Astronomical Exhibit to the Andes sent out by Professor Percival Lowell, of Boston. Mr. E. C. Slipher is photographer, Mr. A. G. Ilse of Alvan Clark & Sons the instrument maker, and Mr. R. D. Eaglesfield, mechanician. The party will observe the opposition of Mars with the eighteen-inch telescope of Amherst College Observatory, and the annular eclipse of the sun, July 10, for Professor Newcomb.

Professor George P. Merrill, head curator of geology at the U. S. National Museum, has just left Washington upon an investigating tour to Canyon Diablo, near Flagstaff, Arizona. He is sent out by the Smithsonian Institution to study the geologic origin of a remarkable crater-form depression at the Canyon, in connection with which two theories have been held, one ascribing its origin to a prehistoric volcanic explosion, the other holding that it is due to the impact of a gigantic mass of meteoric iron. The object of Dr. Merrill's trip is to determine if possible which of these theories is correct, or whether its origin must be accounted for in some other manner. While on the trip, Dr. Merrill will visit the so-called petrified or fossil forest near Holbrook, Arizona, to collect for the National Museum specimens for scientific work.

The St. Louis Medical Society commemorated on April 27 the twenty-fifth anniversary of the death of Dr. John T. Hogden, the eminent surgeon.

Dr. Rudolf Aderhold, director of the Biological Department of the Agricultural and Forestry Institute of Berlin, has died at the age of forty-two years.

The deaths are announced of M. Paul Porrier, professor of anatomy in the University of Paris; of Dr. Oscar Doebner, professor of chemistry at Halle, and of Mr. George E. Davis, founder and editor of the *Chemical Trade Journal*.

The senate of the state of Pennsylvania has unanimously passed a bill appropriating $350,000 to the American Philosophical Society to provide a fitting memorial to Benjamin Franklin.

The collection of geological books and pamphlets which was the property of the late Dr. Carl Rominger has been donated to the Russell Library of the University of Michigan.

Professor Theodore D. A. Cockerell, of Boulder, Colorado, recently transmitted to Dr. Arthur Hollick, of the New York Botanical Garden, a collection of undescribed fossil plant remains from the Tertiary beds of Florissant in that state.

The Pathological Society of Philadelphia celebrated its semi-centennial on May 9 and 10. On the first day there was an exhibition in the Mütter Museum of the College of Physicians. On May 10 there was a luncheon and a dinner and the following addresses were made: 'The Rôle of Protozoa in Pathology,' by Dr. Frederick G. Novy, of the University of Michigan; 'The Dynamic Point of View in Pathology,' by Professor Alonzo E. Taylor, University of California; 'The Newer Pathology,' by Dr. Simon Flexner, Rockefeller Institute, and 'Pathology and Practise,' by Dr. William Osler, Oxford University, England.

At a meeting held recently under the presidency of Professor Golgi it was decided that an Italian Neurological Society should be established. Professor Bianchi was elected president, Professors Morelli and Mingazzini, vice-presidents, and Professor Tanzi, general secretary. The first meeting has been fixed for October 16, 17 and 18 of the present year at Naples.

The British Medical Journal states that the tenth congress of Polish physicians and men of science will be held this year at Lemberg, some time between June 16 and July 24. Connected with it there will be a scientific and medical exposition divided into sections as follows: I. Natural Science and Medicine, subdivided into the following groups: (*a*) scientific and medical; (*b*) balneological; (*c*) pharmaceutical; (*d*) industrial chemistry; (*e*) medical instruments and apparatus. II. Hygiene, subdivided into groups as follows: (*a*) hygiene of nutrition; (*b*) personal hygiene; (*c*) hospital administration; (*d*) training of youth; (*e*) hygiene of factories and workshops; (*f*) hygiene of the dwelling; (*g*) hygiene of clothes; (*h*) infectious diseases and their prevention; (*i*) hygiene of childhood; (*j*) public health. Foreign institutes and firms may take part in the scientific section of the exposition, but may only show such objects as will not interfere with Polish industries. Communications should be addressed to the director of the exposition, Dr. Kalikst Kryzanowski, Lemberg.

THE bill to prohibit vivisection in Pennsylvania has been dropped from the calendar in the legislature. This action followed an address in opposition to the measure made to fifty members of the house by Dr. S. Weir Mitchell of Philadelphia.

UNDER the direction of the College of Agriculture of Ohio State University a special train was recently run through several parts of the state bearing certain exhibits of interest to agriculturists. Brief addresses were made at the various towns passed through by instructors from the university.

THAT part of the fuel testing equipment being operated by the Geological Survey which relates to the investigation of fuels under the boilers and the gas producers, and the briquetting investigations, is now being transferred to Norfolk, Va., where during the Jamestown Exposition these will be used in testing coals, lignites and peats of the Atlantic and Gulf seaboards; and during this time will be tested specially the fuels available for the use of the navy, testing the briqueted coals as compared with the run-of-mine coals from the same mines. The other part of the fuel testing plant equipment which relates to the coking of coals, including the coal washing investigations, is now being transferred to Denver, Colo., where investigations will be conducted during the remainder of the present year to determine the coking qualities of the different coals in the Rocky Mountain and other western states.

DR. WILLIS L. MOORE, chief of the Weather Bureau, writes in his annual report for the year ending June 30, 1906, in regard to the Mount Weather Research Observatory as follows: "The meteorological work of a first-order station has been maintained throughout the year, and telegraphic reports were transmitted to the Central Office in Washington daily at 8 A.M. and 8 P.M. Work on the physical laboratory was resumed in July and satisfactory progress was made during the summer and fall of 1905. The building will probably be completed early in 1907. In the preparation for kite and balloon work a number of important instruments have been installed and made ready for systematic work. Among these may be mentioned: (1) The electrolyzer, for the manufacture of the hydrogen gas employed in the kite balloon and the small rubber balloons; (2) the apparatus for the manufacture of liquid air, employed in testing thermometers at very low temperatures; (3) the apparatus used in testing the barometers, thermometers and meteorographs employed in connection with the kites and balloons. A medium-sized power kite-reel was installed in the revolving kite-house early in the year, and experimental kite-flying was begun in September of 1905. During the year the stock of meteorographs, of kites and of kite wire was materially increased; the instrumental equipment now includes eight different styles of kite-balloon meteorographs, comprising English, German and French designs, in addition to the Marvin type heretofore used in the kite work of the bureau, and the new Fergusson pattern used at the Blue Hill Observatory. In April, 1906, systematic cooperation was begun in connection with the work of the International Committee for Scientific Ballooning by flying kites on prearranged term days, and this work is being regularly maintained. The interior finishings of the magnetic observatory buildings, the erection of the piers and the installation of the magnetic instruments were completed during the year. The instruments for absolute observations, except the declinometer and some auxiliary apparatus, were received and set up before January 1, 1906. The remaining absolute instruments were received and put in place by the end of May, and routine observations were established at the end of the fiscal year. The Eschenhagen magnetographs were set up in the basement of the absolute observatory in December, 1905, and have given a satisfactory record of the magnetic elements since that time. The Wild magnetographs were received and installed by the first of June, and were being adjusted by the end of the fiscal year. A gas plant for heating and illuminating the magnetic observatories was put in during the winter and has given satisfactory service since then. Plans were prepared for an additional office and dwelling for the director of upper-air research, and work on this building was begun July 1, 1906."

UNIVERSITY AND EDUCATIONAL NEWS

Announcement is made that Princeton University has received from donors whose names are for the present withheld a gift of $1,200,000, for the erection and endowment of two scientific buildings—one for physical science and one for biology and geology. In each case the building will be erected at a cost of $400,000, and $200,000 is provided for equipment and maintenance.

Mr. Edward W. Currier, an alumnus of Amherst College of the class of 1865, has bequeathed to the institution a large sum, said to be $500,000. As has been already noted here, by Mr. Currier's death two legacies are released—one of $180,000 to Williams College and one of $100,000 to Yale University.

Barnard College, Columbia University, has been made the residuary legatee of the estate of Miss Emily O. Gibbes. It is estimated that the college may receive $750,000.

The University of Minnesota received at the recent session of the legislature a considerable increase to be used for salaries. Previously the maximum salary was $2,700 for heads of departments. These have now been increased to $3,500, and other appointments in proportion. In addition, provision has been made for twenty-eight new appointments in the university. Several of these will be in the department of medicine, including three in anatomy, gross and microscopic, and one each in physiology, pathology and pharmacology.

The Massachusetts Legislature has rejected the bill taxing residences occupied by officers of the colleges and universities of the state. The bill was passed in the senate, but defeated in the house by a vote of 142 to 14.

The College of the City of New York celebrated the sixtieth anniversary of its foundation on May 7 by taking possession of its beautiful new buildings on Washington Heights. The ceremonies were first held in the chapel on the top floor of the old college building at Lexington Avenue and 23d St. Addresses were made by President Finley, Professor Alfred G. Compton, of the department of physics; Mr. Patrick F. McGowan, president of the Board of Aldermen; Mr. Richard Watson Gilder, of New York City, and Mr. Leslie M. Shaw. Further exercises were held in the Townsend-Harris Hall in the new buildings.

The University of Geneva will celebrate with appropriate ceremonies the three hundred and fiftieth anniversary of its foundation, which falls in 1909. A committee has been appointed to arrange for the celebration to which representatives of foreign universities will be invited.

Dr. William Jewett Tucker has, owing to ill-health, resigned the presidency of Dartmouth College.

At Harvard University, Dr. George Washington Pierce has been promoted to a professorship of physics.

Dr. Philipp Lenard, of Kiel, has accepted the chair of physics at Heidelberg, where a new physical laboratory will be built for him.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MAY 24, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE AMERICAN PHILOSOPHICAL SOCIETY

THE annual meetings of the American Philosophical Society, which this year were held April 18 to 20, have now become a permanent feature and are looked forward to by the members as an agreeable occasion for meeting colleagues from various parts of the United States and for listening to papers covering the entire range of scientific investigation. In the latter respect the sessions are unique, and it is perhaps well in these days of extreme specialization that there should be one organization which should not merely cover all the so-called natural sciences but extend its range to history, archeology and philology, as well as literature.

In all five sessions were held for the reading of papers, which left plenty of time for the full presentation of the thirty-five papers that had been announced. The session of Friday morning was rendered particularly interesting by the presence of the Honorable James Bryce, the British Ambassador, who was formally presented to the members and made a happy address. After the luncheon on Friday, Mr. Bryce presented the gold medal of the Royal Astronomical Society to Professor Ernest W. Brown, in recognition of his important contributions to the problems relative to the motions of the moon.

The sessions were, as far as possible, so arranged as to place papers of the same class together. In accordance with this plan, the papers for Thursday afternoon and part of Friday morning were chiefly philological and historical, those for the rest of Friday botanical, chemical and engineering. Saturday morning was devoted largely to geology, and Saturday afternoon to astronomy, while the papers covering other branches were sandwiched in, as it was found convenient, amid the general broad subjects. It is not possible within the scope of a general account to do more than say a few words about a limited number of the papers read.

Among the papers read at the first session, on Thursday afternoon, was one by Professor W. A. Lamberton on the Greek phrase of the New Testament ordinarily translated 'walking on the sea,' but which Professor Lamberton held, on the basis of an elaborate investigation of the text, should be rendered 'walking at the sea.' The speaker claimed that in the narrative of John, Jesus is not represented as either entering the ship or walking on the sea, but rather on the shore close to the water-edge.

Mr. Rosengarten's paper on the early French members of the American Philosophical Society included an account of the many Frenchmen who at the end of the eighteenth century and in the beginning of the nineteenth century came to this country and became associated in the work of the society. Buffon was the first Frenchman to be elected a member of the society, in 1768.

Professor A. Marshall Elliott, of the Johns Hopkins University, presented a paper of great interest on the origin of the word chauvinism, in which, among other things, he pointed out that the attempt to trace this word to a real character rested upon the slenderest foundations.

Professor Jastrow's paper on the liver as the seat of the soul was an investigation of various views held by the ancients regarding the organ associated with the soul. The paper showed that while the generally prevailing view of antiquity placed the soul in the heart and subsequently in the brain, there were traces of an earlier belief which assigned to the liver the distinction of being the seat of the soul.

Professor Paul Haupt, of the Johns Hopkins University, read a paper on Jonah's whale which was devoted to showing the kind of animal that the narrator of the Book of Job had in mind. The important feature of Professor Haupt's paper was the determination of the fact that whales were known to the ancient Assyrians and are referred to in inscriptions as early as the twelfth century before this era.

Professor Lewis M. Haupt, of Philadelphia, presented an important communication on the transportation crisis, in which, after a survey of the earlier methods of transportation in this country, he urged the importance of water highways as the remedy for the rapidly increasing difficulties of transportation by railroads.

Dr. Henry Kraemer gave an outline of a continuation of the interesting studies in producing and modifying the color in plants by treating them with certain chemicals.

Dr. Howard Crosby Butler gave an interesting account of the Princeton expedition to Syria in 1904–5 which did important work in surveying, in the study of architecture, and added considerably to our knowledge of the inscriptions and other archeological material in a comparatively little known portion of Syria.

The paper by Professor Titchener, of Cornell, in collaboration with W. H. Pyle, of Cornell, on the influence of imperceptible shadows on the judgment of distances demonstrated that the current view that optical illusions in estimating distances by the eye persist even when the lines that produce the illusion are so weak as to be imperceptible is not in accordance with the facts. The conclusion was, therefore, drawn that the subconscious need not be taken into account in the psychological explanation of optical illusions.

Professor William F. Magie, of Princeton, in a paper on the association theory of solutions, expressed the view that the heat capacity of electrolytic solutions and the volumes of solutions can be satisfactorily represented by a formula constructed on the hypothesis that each undissociated molecule of the solution associates with itself a group of water molecules. The heat capacity of the solution is additive of the heat capacities of these groups and of the water lying outside of them.

Dr. T. J. See, of Mare Island, California, supplemented his former paper on the theory of earthquakes which is published in the *Proceedings* of the American Philosophical Society, for 1906, by the further examination of the temperature and secular cooling of the earth, in which he showed that the effects of contraction are nearly insensible. The necessity of therefore abandoning old theories becomes more patent. It is interesting to note that four great writers of antiquity—Plato, Aristotle, Strabo and Pliny—ascribe earthquakes to the agitation of elastic vapors in the earth, a confirmation of his own theory to which Dr. See attaches considerable importance.

Dr. Elihu Thompson, of Swampscott, Mass., in his paper on the progress of the Isthmian Canal, gave, on the whole, a favorable account of the condition of the work. The organization was pronounced to be admirable, and this, taken together with the successful work of sanitation under the charge of Colonel Gorgas, gives every reason to believe that the canal will be finished in from eight to ten years.

Among the geological papers, that of Professor William B. Scott, of Princeton, giving illustrations of the restoration of the Santa Cruz mammals, was the most striking. Two other members of the geological department of Princeton, Messrs. Sinclair and Farr, were represented by papers, all touching on the results of the Santa Cruz expedition of Princeton University.

Another paper of general interest, that by Bailey Willis on continental development, was devoted to a discussion of the various hypotheses to account for the circumstance that each of the two great continents is composed of distinct masses which have tended to stand high in relation to their surroundings.

Professor Eric Doolittle's paper was on two remarkable stars. One, discovered in 1882 by Hough, was for a time in danger of being rejected, until in 1899 it was independently rediscovered by See. The motion of this double star during the past two years has been so rapid that already a fairly good orbit can be computed. The second pair is made up of two very faint stars more than three and one half seconds apart, a remarkable circumstance, for of all the double pairs there are none at once as faint and wide as this pair, except those which are connected with very bright stars.

The session on Thursday evening was of a popular character, being held in Witherspoon Hall and devoted to South Africa. The subject was divided between Professor William B. Scott, of Princeton, and Professor E. W. Brown, of Haverford. The former spoke about the geology and geography of South Africa, and the latter about the scenery and people. Both lectures were illustrated by stereopticon views. On the conclusion of the lectures, a reception was held by the president and council.

At the close of the Saturday morning session, balloting for new members was taken up, which resulted in the election of twelve persons resident in the United States —George Ferdinand Becker, Ph.D., Washington; Charles Benedict Davenport, Ph.D., Cold Spring Harbor, L. I.; J. P. Croser Griffith, M.D., Philadelphia; Frank Austin Gooch, Ph.D., New Haven; Herbert Spencer Jennings, Ph.D., Baltimore; James Playfair McMurrich, Ann Arbor, Mich.; Edward Laurens Mark, Ph.D., LL.D., Cambridge, Mass.; John Bassett Moore, LL.D., New York; Francis Eugene Nipher, St. Louis; Horace Clark Richards, Ph.D., Philadelphia; John C. Rolfe, Ph.D., Philadelphia; and Allen J. Smith, M.D., Philadelphia—and five foreigners—Baron d'Estournelles de Constant, Paris; George Carey Foster, F.R.S., D.Sc., LL.D., Rickmansworth, Herts, England; J. J. Jusserand, Washington; Sir William Turner, K.C.B., D.Sc., D.C.L., F.R.S.; and John C. Kapteyn, Gröningen, Holland.

On Saturday evening the sessions were closed with a banquet at the Bellevue-Stratford, attended by about a hundred members of the society. Professor Albert H. Smyth, of the Central High School, the sad news of whose sudden death has just been announced, acted as toastmaster and presented, among others, the following speakers: Judge Sulzberger, who responded to the toast, 'The Memory of Franklin'; Professor John W. Rhys, of Oxford, who spoke on 'Sister Societies'; and the Honorable Charlemagne Tower, who made an interesting address on 'The Aims and Purposes of the Society.'

All the scientific sessions were well attended, and the society has every reason to feel gratified at the interest shown by members in coming, many of them from a very great distance, to read or to listen to papers.

The following is a list of the papers read:

THURSDAY, APRIL 18

Retardation in Mental and Moral Development—A Problem of Public Education: Dr. Lightner Witmer, of Philadelphia.

Analogies between the Colonization of Ireland and of Virginia: Professor Edward P. Cheyney, of Philadelphia.

Elizabethan and Jacobian College Dramas: Professor Felix E. Schelling, of Philadelphia.

The Narratives of the 'Walking on the Sea': Professor William A. Lamberton, of Philadelphia.

Early French Members of the American Philosophical Society: Dr. J. G. Rosengarten, of Philadelphia.

The Influence of Imperceptible Shadows on the Judgment of Distance: Professor Edward B. Titchener and W. H. Pyle, of Ithaca.

Chauvin (Chauvinism-Calvin, Cauvin)*: Truth and Fiction in the Story of its Origin:* Professor A. Marshall Elliott, of Baltimore.

FRIDAY, APRIL 19

Provisional Report of the Investigation of Foreign and Domestic Stage Micrometers: Dr. Marshall D. Ewell, of Chicago.

The Liver as the Seat of the Soul: Professor Morris Jastrow, Jr., of Philadelphia.

On Jonah's Whale: Professor Paul Haupt, of Baltimore.

Charts Illustrating the Taxonomic Relations of the Monocotyledonous and Dicotyledonous Plant Families: Professor John W. Harshberger, of Philadelphia.

Some Experiments with Plant Nutrients: Professor Henry Kraemer, of Philadelphia.

The Bacteriology of Flax Retting: GEORGE T. MOORE, Ph.D., of West Chester, Pa.

Note as to the Measurement of the Action of Water upon Zinc and Lead: DR. WILLIAM PITT MASON, of Troy, N. Y.

New Results in Electro-Analysis: Dr. EDGAR F. SMITH, of Philadelphia.

The Production of Synthetic Alcohol: Dr. HARVEY W. WILEY and HERMAN SCHREIBER, of Washington, D. C.

The Progress of the Isthmian Canal: Dr. ELIHU THOMSON, of Swampscott, Mass.

On the Transportation Crisis: Professor LEWIS M. HAUPT, of Philadelphia.

The Princeton Archeological Expedition to Syria: HOWARD CROSBY BUTLER, of Princeton. (Lantern illustrations.)

The Association Theory of Solutions: Professor WILLIAM F. MAGIE, of Princeton.

The Groups which are Generated by Two Operators of Order Two and Four respectively, where Commutator is of Order Two: Professor G. A. MILLER, of Urbana, Ill.

SATURDAY, APRIL 20

Santa Cruz Typotheria: W. J. SINCLAIR, of Princeton. (Lantern illustrations.)

Santa Cruz Birds: MARCUS S. FARR, of Princeton.

Restorations of Santa Cruz Mammals: Professor WILLIAM B. SCOTT, of Princeton.

On the Temperature, Secular Cooling and Contraction of the Earth, and on the Theory of Earthquakes held by the Ancients: Dr. T. J. J. SEE, of the U. S. Naval Observatory, Mare Island, California.

On Continental Development: BAILEY WILLIS, M.E., of Washington.

A Study of the Mean Temperatures of the Surface of the Moon, Earth and other Planets: Professor CLEVELAND ABBE, of Washington.

The Solar Corona: W. W. CAMPBELL, of Lick Observatory, California. (Illustrated.)

Astronomical Photography: Professor E. E. BARNARD, of Yerkes Observatory, Williams Bay, Wis.

Conservative Systems with Prescribed Trajections: Professor E. O. LOVETT, of Princeton.

Comparison of Results of Observations at the Flower Observatory for the Years 1905 and 1906, with the Wharton Reflex Zenith Tube and the Zenith Telescope: Professor C. L. DOOLITTLE, of Upper Darby, Pa.

Comparison of Results of Latitude Observations at the Sayre Observatory, South Bethlehem, and at the Flower Observatory, Philadelphia, from September 30, 1904, to September 3, 1906: J. H. OGBURN, of Bethlehem, Pa.

Two Remarkable Double Stars: (a) the short period Binary, Hough 212, and (b) the Stellar System Krueger 60: ERIC DOOLITTLE, of Upper Darby, Pa.

THE SOCIETY OF AMERICAN BACTERIOLOGISTS

THE eighth annual meeting of the Society of American Bacteriologists was held in New York during Convocation Week. The meetings were held at the College of Physicians and Surgeons, and at the Rockefeller Institute, the latter in conjunction with Section K of the American Association for the Advancement of Science.

The first session of the society was held on Thursday morning, December 27, in the main lecture room of the College of Physicians and Surgeons, and, after a short business meeting for the reading of reports and the nomination of new committees, a number of interesting papers were presented on various general aspects of bacteriology, morphology, physiology, etc. The joint meeting at the Rockefeller Institute on Friday morning was devoted largely to the discussion of subjects of interest alike to the bacteriologist and the pathologist and chemist. The papers at this session were largely devoted to various investigations of sera, toxins, and the biology of pathogenic bacteria and other organisms. The third session, on Saturday morning, at the College of Physicians and Surgeons, dealt with laboratory procedure, methods of cultivation of certain bacteria, and studies on the bacteriology of milk.

The following officers were elected for the present year:

President—Dr. James Carroll.
Vice-president—Professor F. D. Chester.
Secretary-Treasurer—Professor S. C. Prescott.
Council—Dr. J. J. Kinyoun, Dr. F. G. Novy, Dr. F. P. Gorham, Dr. H. W. Conn.
Delegate to the American Association for the Advancement of Science—Dr. Erwin F. Smith.

The papers, of which abstracts, or titles alone, are presented below, were as follows:

Movements of Certain Bacteria in Soils: Karl F. Kellerman and Edna H. Fawcett, Bureau of Plant Industry, Department of Agriculture, Washington, D. C.

Two organisms have been studied in connection with *Pseudomonas radicicola*, *Bacillus ochraceus* and a form resembling *Bacillus coli*. The latter kills *Pseudomonas radicicola* when in synthetic nitrogen-poor sugar bouillon, but not in soil extracts of favorable soils; *Bacillus ochraceus* has little effect upon *Pseudomonas radicicola* under either condition.

In sterilized favorable soils saturated with water *Bacillus ochraceus*, *Pseudomonas radicicola*, and the paracolon organism grow with almost equal speed, progressing about one inch in forty-eight hours. In soils barely moist *Pseudomonas radicicola* progresses at the rate of about one inch in seventy-two hours, while the two other forms are reduced to a rate of about one inch in eight days. These experiments were all conducted at a temperature of 25° C. At a temperature of 10° C. the rate of *Pseudomonas radicicola* was reduced to one inch in three days in saturated soil; the two other organisms had made practically no growth at the end of thirty days.

Under none of the conditions of these experiments did there seem to be any antagonism in the soil between *Pseudomonas radicicola* and *Bacillus ochraceus* or the paracolon organism.

Further Studies on Putrefaction: Leo F. Rettger, Yale University.

1. Real putrefaction is caused only by obligate anaerobes. None of the obligate aerobes and facultative anaerobes thus far studied have revealed this property. Some organisms like *B. pyocyaneus* and *Proteus vulgaris* are able to slowly digest coagulated egg albumin and blood serum, but the products are not those of putrefaction. Mercaptan in particular is invariably absent.

2. *B. putrificus* (Bienstock), the bacillus of malignant œdema and of symptomatic anthrax are putrefactive organisms, though they may vary at times in this respect. None of the strains of *B. tetanus* so far examined have shown any putrefactive action in the so-called egg-meat medium.

3. *Bacillus aerogenes capsulatus* of Welch (or *B. enteritidis sporogenes* of Klein) is essentially a fermentative organism. At no time has it been able to cause any apparent decomposition of coagulated proteids, although cultures of the organism often have a decided putrefactive odor.

4. Normal feces in a large percentage of cases caused a marked decomposition of the egg-meat medium. In a large number of such cases the *Bacillus putrificus* of Bienstock was observed to be responsible. In at least 25 per cent. of the stools examined the bacillus of malignant œdema was present, though in small numbers. The amount of feces tested at one time varied from 2 to 32 milligrams. The *Bacillus aerogenes capsulatus* of Welch was found to be present constantly when the quantity of feces examined was at least 32 milligrams. In 2 milligram quantities, however, it failed to reveal itself in at least 50 per cent. of the tests that were made with such small amounts.

A Study of the Variation in the Biochemical Reactions produced by Cultures of the Colon Type: STEPHEN DEM. GAGE.

The study was made to ascertain how closely the variations in the intensity of certain biochemical functions of cultures usually included in the colon group by routine tests would agree with the law of biological variation.

About 200 cultures of the colon type, as interpreted at the Lawrence Experiment Station, were examined after incubation at 40° C. for the amount of gas produced in dextrose broth in 24 and 48 hours, for the proportion of CO_2 in that gas, and for the amount of ammonia and of nitrites produced in nitrated pepton solution in 24 hours.

The amount of gas produced in 24 hours varied between 15 per cent. and 100 per cent., about four fifths of the cultures producing between 40 and 70 per cent. of gas.

After 48 hours' incubation the amount of gas also varied between 15 per cent. and 100 per cent., and the distribution of the cultures among the different gas values was more uniform than at the end of 24 hours, about half of the cultures producing between 35 per cent. and 50 per cent. of gas. With 15 per cent. of the cultures the amount of gas increased between the first and second day, while with 65 per cent. of the cultures there was a decrease in the amount of gas on the second day.

The amount of CO_2 in the gas at the end of 48 hours varied from none to 75 per cent., about three fourths of the tubes containing between 10 and 25 per cent. of CO_2.

In the nitrated pepton solution at the end of 24 hours about 30 per cent. of the tubes contained less than one part per million nitrogen as ammonia, and about half of the tubes contained between one and three parts, while about one fourth of the tubes contained less than one part nitrogen as nitrites, and about half of the tubes contained between one and three parts.

The number of determinations was altogether too small to plot curves of biological variation with any degree of accuracy. The curves plotted, however, appear to be simple, unimodal curves, and indicate that the group was a true biological group and that the variations were normal biological variations.

Involution and Degeneration Forms of Bacteria: D. H. BERGEY, M.D.

The indefinite and confusing definitions of the nature of involution and degeneration forms of bacteria found in most textbooks on bacteriology are bewildering to the student, and leave him in doubt as to the exact significance of these terms.

The term 'involution forms' is defined in the Standard Dictionary as 'certain swollen, bladder-like and irregular forms which the organisms sometimes assume after their death, or as the result of deleterious influences, such as insufficient nutrition.' This definition is sufficiently clear, and is in general use by authorities in text-books on bacteriology.

The term 'degeneration forms' is, however, less definitely defined in the text-books. The Standard Dictionary defines degenerate, 'to become worse or inferior; decline in character, qualities, or excellence, as from the normal or primitive condition or from a type of standard; deteriorate.' According to this definition, the term 'degenerate forms' should be reserved for those organisms that had reached the normal stature and subsequently manifested the effects of degenerative influences. These effects are seen principally as vacuolation, granulation, and fragmentation of the protoplasm in organisms of normal size and shape.

The Production of Indol in Proteid-free Media: M. X. Sullivan, Brown University, Providence, R. I.

Several years ago, I found that *B. coli communis* would grow well on non-proteid media. At that time no tests were made to determine whether or not indol and skatol were produced on these media. Under the pressure of other work the experiments on *B. coli* were not continued. Recently, however, my attention was again directed to the metabolism of *B. coli* by L. F. Rettger's article 'Studies on Putrefaction,' *Jour. of Biol. Chem.*, '06, Vol. II., p. 71. Accordingly solutions of non-proteid media were made and inoculated with a pure culture of an indol-forming variety of *B. coli* in order to determine whether or not this bacillus could produce indol in simple media.

The culture medium on which the present conclusions are based consisted of

Asparagin	0.2	grm.
Mannite	0.2	
NaCl	0.02	
$MgSO_4$	0.01	
$CaCl_2$	0.02	
K_2HPO_4	0.2	
H_2O	100	c.c.

This medium was made slightly alkaline to litmus and divided into two portions of 2,000 c.c. each: flask (A) and flask (B) respectively. To (B) was added $CaCO_3$ to take up whatever free acid might be formed. Both portions were then sterilized and inoculated with *B. coli.*

In periods varying from five to ten days the contents of (A) and (B) were analyzed for indol and skatol. In no case was indol or skatol found, either in the distillate or in the original solution. An inoculation from the non-proteid medium containing *B. coli* to bouillon produced in a few days a good growth with the formation of indol.

From these experiments it would seem that indol is not formed synthetically, but in ordinary culture media is a result of the decomposition of the albuminous material present. Long-continued growth of *B. coli* on a non-proteid medium I believe will give a test for indol, since the death of many micro-organisms will leave an albuminous material in the medium.

The Sterilization of Sewage-Filter Effluents: Earle B. Phelps.

The development of the modern, rapid processes for the purification of sewage has led to a new conception of the functions of a sewage-disposal plant, in which conception the removal of bacteria finds but small consideration. The cost of obtaining high bacterial efficiency, by means of sand filtration, is so much greater than the cost of producing an effluent which is non-putrescible, though germ-laden, that the former can not as a rule be employed in the treatment of the sewage of large cities, unless bacterial purification is imperative. In certain cases, as for example where important shell-fish industries are threatened, it does become necessary to consider bacterial as well as chemical purification. As an alternative to treatment on sand, either as the chief, or as a supplementary process, chemical sterilization of the rapid filter effluents has been proposed. Experiments carried out by the writer at the Sewage Experiment Station of the Massachusetts Institute of Technology seem to indicate that the process is much more feasible than has hitherto been supposed, and that it can, in fact, compete with supplementary sand filtration, as a finishing process for the removal of bacteria from effluents. The most suitable disinfectant thus far tried is chloride of lime or bleaching powder.

During the past summer and fall the effluent of a trickling filter, treating Boston sewage at a rate of two million gallons per acre per day, was treated with bleaching powder at a rate of five parts of available chlorine per million of effluent. The average removal of total bacteria was 99.96 per cent. and of organisms fermenting lactose in bile media 99.993 per cent. The cost of treatment for chemicals alone is $1.08 per million. The time of contact was two hours. Some special experiments in bottles upon the germicidal action of bleaching powder on *B. typhosus* showed a practical elimination of that organism in from two to four hours, the same amount of available chlorine being taken.

Results with copper sulphate were not so satisfactory. During October the application of one part of copper per million gave a reduction of 94 per cent. of the total organisms and 98.5 per cent. of the fermenting forms. The mean temperature was 56°.

In November, with a mean temperature of 46°, the removal was 79.5 per cent. and 98.4 per cent., respectively.

It was necessary to double the strength of copper, two parts per million, to restore its germicidal value to the first figure, when, in December, the mean temperature was 43°. Under these conditions the efficiency was, for the total organisms, 97.9 per cent. and for the fermenting forms, 98.8 per cent.

The cost of two parts per million of copper as sulphate is about the same as of five parts per million of chlorine as bleaching powder. The germicidal effect of the former is not nearly so great.

Some experiments, previously reported by the writer, showed that the presence of organic matter (sugar or peptone), seriously interfered with the toxic action of copper sulfate on the typhoid bacillus.

A Substitute for Potato as a Culture Medium: P. G. HEINEMANN, University of Chicago.

Ten grams of agar are dissolved in 600 c.c. water. A solution of the following salts dissolved in 200 c.c. water is then prepared:

Dipotassium hydrogen phosphate	2 gr.
Disodium hydrogen phosphate	2
Magnesium sulphate	2
Calcium chloride	2
Ammonium lactate	2
Asparagin	5

This solution, in which a fine precipitate forms, is added to the hot agar solution, 10 grams peptone dissolved, and the whole mixture filtered after the reaction, which is about 5 per cent. acid, is brought to the neutral point with phenol phthalein.

To the hot filtered solution 30 grams of starch, previously washed in water and made perfectly homogeneous in a mortar, is gradually added with constant stirring. The mixture is then brought to near the boiling point and finally weighed. The total weight should be 1,000 grams. The medium is tubed and sterilized in the autoclav for 5 minutes at 120°, and cooled in a slanting position.

The Enzymotic Properties of Diplococcus Intracellularis: SIMON FLEXNER, from the Rockefeller Institute for Medical Research.

The brief vitality of many of the cultures of *Diplococcus intracellularis* is a point of differential importance. Many strains, grown in a favorable medium, unless transplanted to a fresh medium do not survive beyond two or three days. Cultures three days old show marked degenerations, and the latter increase rapidly with age until, at the end of five or six days, or even earlier, no normal cocci persist. As degeneration progresses, loss of staining power and disintegration ensue, until, finally, staining is lost and a formless detritus remains.

The changes in the diplococcus are associated with the action of an enzyme which brings about the disintegration. This enzyme does not exhibit the usual properties of a proteolytic ferment; it does not liquefy gelatin or coagulated serum. The degree and rapidity of its action varies with its concentration; at least a heavy suspension of the cocci in salt solution, kept at 37° C., undergoes dissolution more rapidly and completely than a weaker suspension. The vitality of the cultures is associated with the degree of autolytic alterations in the suspensions; cocci in the weak suspensions survive longer than in the stronger ones. At lower temperatures—2° C.—disintegration of the cocci either does not take place at all or progresses much more slowly. Under the latter conditions more cocci survive in the strong than in the weak concentrations, although even here the vitality is a brief one.

Potassium cyanide restrains the action of the ferment which tends to disintegrate the diplococci; after removal of the cyanide dissolution sets in. Heating the diplococci to 65° C. prevents or reduces the dissolving power of the intracellular enzyme.

The enzyme acts upon the dead cocci—probably not upon the living germs. Diplococci killed by heat (50° to 55° C.) undergo autolysis; but when the cocci are killed by the addition of toluol, autolysis is accelerated. A heavy suspension of the diplococci in salt solution, under toluol kept at 37° C., may be disintegrated in four hours.

The enzyme of the diplococcus acts energetically upon other bacteria, bringing about their dissolution. It acts upon *B. typhosis, B. coli communis, B. pyocyaneus, B. anthracis, M. catarrhalis,* and to a less degree and more slowly upon *Staphylococcus aureus.*

The Stability of Tetanus Toxin: M. J. Rosenau, Hygienic Laboratory, Washington, D. C.

Some Observations on the Blood of Horses: J. J. Kinyoum, Philadelphia, Pa.

The volume percentage of the cell content of normal horse blood ranges from 30 to 46 per cent.; an average of 37.8 per cent. The administration of diphtheria toxin, causes a diminution of the red cells to an average of 30 per cent.

The anæmias induced by the toxin and the subsequent bleedings are progressive, and are in direct relation to the length of treatment. Horses treated with tetanus toxin, or with dead or living micro-organisms, also show a progressive diminution of the corpuscular content, but it is not so great as in the cases when diphtheria toxin is administered.

The danger point for all horses, beyond which the horse can not be bled, is when the cell content falls below 20 per cent. The leucocytes bear no fixed relation to the red cells; it may be as great as 1 per cent. or as small as 0.1 per cent. normal, and the same in those undergoing treatment.

There is also no relation between the cell content and the anti-bodies.

The hæmoglobin follows very closely the curve of the cells (red), both in the normal and in the treated horses.

Toxic Effects of Horse Serum.—Guinea-pigs sensitized with horse serum begin to respond to the toxic effects of the same serum on the eighth day.

If, however, the sensitized pigs be injected with the serum of a horse which has been injected several times with progressive quantities of human blood, there is no effect.

On the contrary, if the sensitized pig be injected with the serum of a horse which has received repeated injections of horse blood rendered hæmolytic for human blood (red cells), the toxicity is not only restored, but is somewhat more toxic than the normal serum.

The toxic effect of horse serum is modified by the amount of the sensitizer; those which have received as much as 10 to 15 c.c. of serum as a sensitizer, are rendered quite resistant to the toxic dose. When the amount of the sensitizing dose is as much as one tenth the body weight, the pigs are immune.

The toxic effect of the serum is also influenced by the character of the sensitizer; for an example: If guinea pigs are sensitized by a toxin-antitoxin mixture, they are more susceptible than when given normal or antitoxic serum.

The toxic effect of a serum bears a relation to the amount given, and is influenced by the body weight.

Precipitated and dialyzed antitoxic serum is less toxic than the normal, or antitoxic serum.

Milk was found to be nontoxic to guinea-pigs sensitized with antitoxic serum.

The Alleged Rôle of Intestinal Worms as Inoculating Agents in Typhoid Fever: CH. WARDELL STILES,[1] Ph.D., D.Sc., Chief of Division of Zoology, Hygienic Laboratory, U. S. Public Health and Marine Hospital Service.

According to a theory recently advanced in France, intestinal worms (especially whipworms) form the inoculating agent in typhoid, much in the same way that fleas inoculate bubonic plague. The theory is based upon the high percentage of whipworms reported for the typhoid cases by some authors and upon the fact that intestinal worms may wound the mucosa; it is assumed that the uninjured mucosa forms an impassable barrier to the bacteria, which, however, may pass through these wounds. The theory claims that typhoid bacilli in the intestines are harmless unless parasitic worms, or some other wounding agents, are present. Accordingly, the treatment and prevention of typhoid reduces itself essentially to treatment and prevention of parasitic worms, especially of whip-worms. The theory is extended to appendicitis, cholera and certain other intestinal diseases.

More recently, the theory is also extended to include parasitic protozoa as inoculating agents in intestinal diseases, but so far as typhoid is concerned no definite statistical data are presented in support of this extension. As the fresh, warm stools should be examined to test this phase of the subject fairly, and as conditions were not favorable for such examination in the present instance, this protozoan phase of the subject could not be consistently studied in the present report.

The Washington epidemic of typhoid in the summer of 1906 presented the possibility of putting to a practical test the verminous side of this exceedingly alluring theory. The results of the study have failed to confirm the theory, for 92.5 per cent. of the patients showed no infection

[1] In making the 2,000 microscopic examinations involved in preparing this paper I have been aided by Past Assistant Surgeon Joseph Goldberger, David G. Willets, Ph.B., and Arthur E. Paterson, Ph.B.

with intestinal worms, while only 15 of them (7.5 per cent.) showed a total of 16 infections (8 infections per hundred), of which 14 cases (7 per cent.) showed whipworm infection. This represents an increase of only 1.3 infections (0.65 infections per hundred persons) over what we expected to find in the general intestinal helminthiasis, and an increase of only 1.32 per cent. over what we expected to find in whipworm infections. Considering the very wet season we have had, and especially in view of the negative findings in 92.5 per cent. of the patients, these slight increases can hardly be considered of importance.

In comparing the severity of the verminous infections (as judged by the number of eggs present) with that reported for typhoid by Guiart, in France, it was found that the Washington cases averaged only 0.47 + whipworm egg per slide, against two eggs per slide in the French statistics.

Turning to a method of indirect comparison, it is seen that while former examinations in this laboratory (for Washington, D. C., and for Connecticut) showed that the greatest percentage (13.01) of cases of whipworm infections was under fifteen years of age, in the 200 typhoid cases examined the greatest percentage (47.5) of patients fell between the ages of fifteen and thirty years, inclusive; further, the percentage of cases of typhoid does not vary parallel with the percentage of cases of whipworm infection in the other age groups.

Comparing, in reference to sexes, the statistics of whipworm infection in the world at large, and in examinations made for Connecticut and for the District of Columbia combined, with those of the 200 typhoid patients examined, it is seen that whipworms are more common in females than in males, while of our 200 typhoid cases 52.5 per cent. were males and 47.5 per cent. were females. If the comparison is restricted to the total helminthiasis of cases examined in the District of Columbia, it is slightly more favorable to the theory under discussion.

Making a similar comparison in reference to the race of patients, it is seen that in the 200 cases of typhoid under discussion (reduced to figures approximately in harmony with the general relation of the races in the population of the District) the whites were to the negroes as 55.5 to 64, while in the whipworm statistics in former examinations the whites were to the negroes as 3.75 to 9.79. The change in our summer population would account for at least a part of this excess of typhoid among the negroes.

The general conclusions are, therefore, that a study of the intestinal helminthiasis in 200 of the cases in the Washington typhoid epidemic of 1906 has not supported the theory that whipworms, eelworms, or other species of intestinal worm bear any necessary or common relation as an inoculating agent in typhoid fever; and that the view recently expressed in France to the effect that the treatment and prevention of typhoid fever practically reduces itself to the treatment and prevention of intestinal worms, especially of whipworms, does not obtain, at least so far as this locality (Washington, D. C.) is concerned. The question of the relation of protozoa as inoculating agents in typhoid is not considered in this report.

On the Absorption of the Third Serum Component: W. H. Manwaring, Indiana University.

Besides amboceptor and complement, hemolytic serum contains a third active component. This third serum component may possess hemolysis-increasing (auxilytic) or hemolysis-decreasing (antilytic) properties, depending largely on experimental conditions.

The third component is changed in its hemolytic properties by exposure to washed blood corpuscles. Part, at least, of this change is due to the giving off into the third component of antilytic corpuscle products. No actual absorption of the third serum component by washed corpuscles has as yet been demonstrated.

On the So-called Physical Chemistry of Hemolytic Serum: W. H. MANWARING, Indiana University.

Two physico-chemical laws have been proposed for hemolytic serum: (i.) a law governing the interaction of amboceptor and complement.

When heated hemolytic serum is exposed to washed corpuscles, three phenomena take place: (i.) A hypothetical absorption of amboceptor; (ii.) A change in the third serum component; and (iii.) a giving off into the serum of antilytic corpuscle products. No measurement of the amboceptor-power of exposed heated hemolytic serum will, therefore, give any idea whatever of the amount of unabsorbed amboceptor it contains.

Similarly, the existence of an active third component, which differs in different sera prepared under identical conditions, prevents the experimental proof or disproof of the physico-chemical law proposed for the interaction of complement and amboceptor.

The physical chemistry of hemolytic serum is, therefore, beyond the present power of experimental science.

On the Chemical Inactivation and Regeneration of Complement: HIDEYO NOGUCHI, Rockefeller Institute.

On the Electric Charge carried by Toxins, Antitoxins, and Agglutinins: C. W. FIELD, Research Laboratory, New York Board of Health.

I. *An Improved Technic for Tuberculo-Opsonic Preparations.* II. *Some Suggestions concerning the Terminology of Opsonic Theory and Practise:* A. P. OHLMACHER, Detroit.

Generic Characters in the Coccaceæ: C.-E. A. WINSLOW and ANNE F. ROGERS.

The difficulties in the classification of bacteria arise partly from the variations in certain characters produced by slight changes in the environment, but chiefly from the fact that characters which appear to be constant for the individual exist in an almost infinite number of minute gradations in different races. Extreme differences in any property are completely connected by a series of intermediate forms. The best basis for a natural classification is the statistical study of a large series of individuals, which will disclose the points about which the largest number of races are grouped, which are presumably the type centers round which the organisms vary. A study of 500 cultures of cocci isolated from various sources has been made with this end in view. It has shown that the variations which exist group themselves on normal 'curves of frequency.' These show in some cases a single mode, as in the fermentation of dextrose and lactose, all the cultures studied grouping themselves about a single center, and in other cases more, as in gelatin liquefaction, where two centers exist, one for the liquefying and one for the nonliquefying forms. Chromogenesis shows four centers of variation, the white, yellow, orange and red forms being quite definitely separated.

The most significant result of this study is the fact that differences in the various characters are strikingly correlated with each other and with the source from which the organisms were derived. For example, the orange chromogens are parasitic forms which never show the sarcina grouping, are generally Gram positive, and liquefy and ferment sugars very actively, while the red chromogens are their opposite in each respect. It is thus possible to divide the cocci into groups, each marked by the prevailing combination of certain characteristics. These groups are defined in relation to the central type about which they cluster rather than separated by sharp boundary lines; yet they certainly mark natural groups and seem to deserve generic rank.

The generic groups thus established are six in number. Streptococcus is characterized by parasitic origins (from the surfaces of the plant or animal body), feeble growth on media, occurrence of cells in chains or small groups, variable response to Gram stain (though generally a positive one), variable acid production in sugars (often reaching very high values), the absence of nitrate reduction, and the general absence of gelatin liquefaction. (The species in this genus have recently been very satisfactorily worked out, by the statistical method, by Andrewes and Horder, *Lancet*, September 15, 1906.) *Aurococcus* (n. gen.) is characterized by parasitic origin, good growth on media, orange pigment, occurrence of cells in irregular groups, generally positive reaction to Gram stain, and the formation of considerable acid in sugar solutions. Nitrates may or may not be reduced, and gelatin is either not liquefied or liquefied strongly. *Albococcus* (n. gen.) differs from *Aurococcus* in producing a somewhat heavier growth of white color, in forming somewhat more acid in sugar solution, and in a less vigorous action on gelatin. These three genera, with *Diplococcus,* which stands at the extreme end of the series beyond *Streptococcus,* may be grouped in a subfamily, the Paracoccaceæ, which includes parasitic forms producing faint to good growths, made up of chains or groups of cells, generally staining by Gram and producing considerable acid.

The rest of the cocci may be grouped together as Metacoccaceæ, including saprophytic forms, producing vigorous surface growths made up of groups of cells or packets, generally Gram negative and showing a slight action on sugars. Here belong the two generally accepted genera, micrococcus and sarcina, the first showing only irregular groups; the second, packets. In both the organisms are most abundant in air, water and earth, rather than on the body; the surface growths on media are abundant and the pigment is yellow; the Gram stain is usually negative and sugars are fermented very slightly. Gelatin is not liquefied, or liquefied somewhat slowly, and nitrates may or may not be reduced. Finally, *Rhodococcus* (n. gen.) includes the red chromogens, which show either groups or packets, rarely liquefy gelatin and reduce nitrates, if at all, only to nitrites and not to ammonia. In other respects they resemble *Micrococcus.* The group of the cocci, as a whole, shows in each character studied a gradual but continuous series of modifications from the strictly parasitic diplococcus to the strictly saprophytic rhodococcus.

The methods used in this study have been described in the *Journal of Infectious Diseases* for June, 1906, and the final conclusions will shortly be presented in the *Journal of Medical Research.*

Actinomyces of the Oral Cavity: D. H. Bergey, M.D.

The occurrence of actinomyces organisms in the oral cavity is believed to be a subject of interest, not only from the standpoint of infection starting from this place, but also on account of the possibility that these organisms may be directly injurious to the teeth.

Four organisms of actinomyces were isolated from healthy mouths, and studied in more or less detail. These organisms resolve themselves in two types: the one a rather short, club-shaped, striated, branching form, and the other a long, filamentous branching form.

These organisms adhere very tenaciously to the culture media on which they are growing, and this property no doubt manifests itself in the oral cavity, where the organisms probably adhere with equal tenacity to the teeth and assist in the formation of plaques. They also have the property of breaking up a number of the carbohydrates, and in this way also exert an injurious effect through the formation of acids, thus contributing to caries.

The study of the pathogenic properties of these organisms is not yet completed.

The Growth and Toxin Production by B. Diphtheriæ upon Proteid-free Media: PHILIP B. HADLEY.

This is the preliminary report of a study whose object was to learn what constituents of proteid-free media were either favorable or necessary for the formation of toxin by *B. diphtheriæ* upon such media. The results thus far attained may be summed up as follows:

1. Very few cultures of *B. diphtheriæ*, fresh from the throats of man, will take up a growth directly upon proteid-free media.

2. Most cultures which will not at first grow upon proteid-free media may be adapted to grow upon such media. The adaptation may be accomplished by gradually diminishing the amounts of bouillon and increasing the amount of proteid-free media in a combined medium consisting of both proteid-free and bouillon until the proteid-free medium itself is reached. Each tube is inoculated from the preceding tube after an incubation of 3–5 days.

3. Whether the growth of *B. diphtheriæ* upon proteid-free media be spontaneous or the result of adaptation, there may be formed a toxin as virulent as that obtained from bouillon cultures.

4. Of the three nitrogen bases tried (asparagin, urea and glycocoll), urea seemed to be of little value, while glycocoll furnished the best growth and strongest toxin. Asparagin appeared to give better results than urea, though it was not as satisfactory as glycocoll.

5. A single morphological variety of *B. diphtheriæ* may be decidedly modifiable; and there are facts which point to the view that there may be, in the life of the diphtherial organism, what we may roughly call a cycle of adaptive forms, each one of which is best suited to a circumscribed condition of environment in which it may or may not produce a virulent toxin.

6. In all cases of growth upon proteid-free media, whether spontaneous or resulting from adaptation, it is the solid-staining varieties of the diphtherial organism which manifest the most luxuriant growth.

On the Cultivation of Spirillum Obermeieri: F. G. NOVY and R. R. KNAPP, the Hygienic Laboratory, University of Michigan, Ann Arbor, Mich.

The *Spirillum Obermeieri* has been maintained in this laboratory, since November, 1905, by a consecutive passage through rats. Although during this time many hundreds of attempts to secure cultures on artificial media have been made they have given uniformly negative results. In the defibrinated blood of infected rats, the spirilla retain their vitality for a variable length of time depending upon the stage of the disease during which the blood is drawn. If drawn during the decline stage, that is to say at a time when the organisms have reached their maximum and are beginning to decrease in numbers, the spirilla will often die out in less than 24 hours. This is due, as we have shown, to the presence of specific germicidal bodies. On the other hand, in 'onset blood' drawn during the early stage of the disease the spirilla may live for several weeks. Thus, we have seen living spirilla in such blood kept for 30 to 37 days and have been able to infect rats with blood kept for 40 days. Moreover, we have been able to make use of this fact in shipping the virus to distant points, to Dr. Todd at Liverpool and to Professor C. Fraenkel at Halle.

In our first series of attempts at cultivating the spirilla on blood agar we were, as a rule, unable to keep the organisms alive for more than two or three days. Since then, however, we have been somewhat more successful and have kept them on blood agar for 22 to 28 days, and in some experiments now in progress they are still alive and numerous on the thirtieth day. As yet, however, no evidence has been obtained of actual multiplication *in vitro*. The organisms which are found to persist we prefer to regard as mere survivals until actual subcultures have been obtained.

The successful results obtained by Levaditi in the cultivation of *S. gallinarum, S. Duttoni*, and *S. refringens* in collodium sacs led us to apply this method to our spirillum. With this object in view the collodium sacs were filled with rat or rabbit blood, or corresponding sera, heated and unheated, and after inoculation with spirillar blood these sacs were placed in the peritoneal cavity of rabbits. After three to seven days the sacs were removed and contents were examined with *uniformly* negative results. Apparently the rabbit is unsuited for sac cultures.

We were finally led to make the trials under conditions approachng the natural state as much as possible. For this purpose, the collodium sacs were filled with uncoagulated rat blood and after inoculation were placed at once in the peritoneal cavity of a white rat. Three days later, on removal, the sacs were found to contain active spirilla and in increased numbers. From the sacs, transplants were made to new ones and the result was equally satisfactory. The spirilla were found to be in an extremely active condition and were undoubtedly multiplying.

From this time on the transplantations were made regularly, every three or four days, from sac to sac. After a few passages the uncoagulated blood was replaced by defibrinated rat blood or by rat serum. Defibrinated rabbit blood has also been employed to some extent, but whether it will continue to be a favorable medium we are unable to state. Two sacs were inoculated each time and placed in the peritoneal cavity of a rat. Each sac had a capacity of from 2.5–3.0 c.c. and was sealed so as to leave within as little air as possible. It is a noteworthy fact that on removal from the rat the sacs are invariably greatly distended as a result of osmotic changes. Furthermore, the air which was originally present is in large part, and at times wholly absorbed.

Since October 13, the spirilla have been carried through 20 consecutive passages in 68 days, and presumably they can be kept multiplying under these conditions indefinitely. The spirilla in the sac culture are never as numerous as in the blood of rats. They rarely exceed more than 5 to 10 per field of the one twelfth inch objective, as contrasted with several hundred per field met with in the blood of rats during the maximum period of infection. The inoculation of the sac contents (blood or serum) into rats, it is interesting to note, is followed by a mild infection in which the spirilla are not much more numerous than in the sacs. Moreover, in such infection they persist for a day or two longer than is the case with the active virus.

When the sac is allowed to remain in the rat for seven days the spirilla decrease greatly in numbers and may even disappear. In the culture sacs after removal from the rat, and kept at room temperature, the spirilla die out in a day or two.

Throughout this series the spirilla have preserved their form unchanged. They appear either as single cells (8 microns) or of double length (16 microns) but at times even longer spirals are found. The latter are the result of end to end union by means of flagella as we have heretofore shown. As in the case of blood preparations no evidence is observed of division other than transverse. One observation in this connection is deserving of special emphasis owing to its bearing upon the question as to whether spirochetes multiply by transverse or longitudinal division. In these cultures it is not unusual to find short spirals of two or three turns, and from 4 to 8 microns in length. These may occur singly or in pairs (8 to 12 microns long) showing the pale division zone. The width of the short form is the same as that of the longer cells. The occurrence of these short spirals is readily explainable as the result of transverse division. It may further be stated that the cultural spirals usually stain solid by the Romanowsky method but at times they may show granulations which to some extent may be due to granules deposited from the medium.

Sac Cultures in Rat Serum.—In view of the fact that Prowazek and others are inclined to consider spirochetes as protozoa and cell parasites it was desirable to ascertain whether or not the spirilla could be maintained in active multiplication in a clear serum. Accordingly, the spirilla were inoculated into rat serum, completely freed from corpuscles by centrifugation. Up to the present time we have effected 7 consecutive passages in such serum in the space of 24 days. At each passage a control sac containing defibrinated rat blood was placed in the rat. The serum cultures although totally devoid of corpuscles were in every respect as rich in spirilla as the blood cultures. The conclusion to be deduced from these experiments is that multiplication of spirilla may take place without any intracellular stage. The occasional presence of spirilla in a cell is to be regarded as an accident rather than as an expression of an unrecognized cycle.

A New Flagella Stain for Ps. radicola: F. C. HARRISON.

Take a loop of the mucilaginous or viscid growth from an agar culture of *Ps. radicicola* two days to several months old and spread it on a clean slide, lashing it out in slender tongues, let the film dry in air without killing or fixing, flood the film a moment with a saturated alcoholic solution of gentian violet, wash under the tap, dry between folds of filter paper and examine with the oil immersion lens. The mucilage in which the cells lie will be found deeply and evenly stained, and the bacteria scarcely stained at all, so that the preparation presents the appearance of a photographic negative. The unequal density of the protoplasm of the cells is clearly seen, as indeed it is in the living cells when examined from a hanging drop. (See photograph.)

The single polar flagellum is also clearly demonstrated by this stain since it, like the protoplasm of the cells, refuses the stain, and so it appears as a clear or uncolored streak in the surrounding deeply stained mucilage. The flagella are best stained at the margins of the films and in thin places. In parts of the film where the culture is thickly spread, the mucilage is intensely stained and the flagella, being slender and enveloped deeply in the mucilage are not distinguished. In these parts, however, the cells are beautifully contrasted with the dark background and their internal structure is clearly shown. Saturated alcoholic solutions of methyl blue, night blue or fuchsin may be used instead of saturated alcoholic gentian violet. A film prepared and stained as above, then flooded an instant with Lugol's solution, is still more intensely and darkly stained. There is no added value to be gained by using any two or even three of these substances together.

Commercial Cultures of Pseudomonas radicicola: H. A. Harding and M. J. Prucha.

During the past two years cultures of *P. radicicola* dried on cotton have been offered commercially to the agricultural public.

At the Ann Arbor meeting we reported the results of an examination of eighteen such cultures, all of which were found to be worthless for practical purposes. A portion of these examinations were done in cooperation with Professor F. D. Chester, of Delaware, Dr. E. M. Houghton and Dr. C. E. Marshall, of Michigan, and Dr. J. G. Lipman, of New Jersey.

Results from tests of similar inoculated cotton cultures have now been given out from sixteen agricultural experiment stations and in only one case have they obtained satisfactory results from such commercial cultures.

Further discussion of these cultures would be needless but for the fact that one of the commercial companies is now putting its product upon the market in metal containers, claiming thereby to obviate the objections which had been raised against the inoculated cotton cultures packed in parchment paper and tin foil as was the case last season.

We have this season examined fourteen commercial cultures of *P. radicicola* which were in such metal containers and find them as worthless as those examined last season.

Bacteria of the Dairy Wells near Washington, D. C.: Karl F. Kellerman and T. D. Beckwith, Bureau of Plant Industry, Department of Agriculture, Washington, D. C.

Sixty wells and springs from as many different dairy farms have been examined and these are believed to fairly represent the conditions obtaining in the 800-odd other dairy water supplies. Of the 60 wells examined, 22 contained below 500 bacteria to the cubic centimeter, and of this number but six showed the presence of *Bacillus coli* in samples of one cubic centimeter. Nine showed the presence of between 500 and 1,000 bacteria to the cubic centimeter, and of these three showed the presence of *B. coli.* Nineteen showed the presence of 1,000 to 5,000 bacteria to the cubic centimeter, and of these ten showed the presence of *B. coli.* Four showed the presence of 5,000 to 10,000 bacteria to the cubic centimeter, and of these three showed the presence of *B. coli.* Six showed the presence of 10,000 to 30,000 bacteria to the cubic centimeter, and of these five showed the presence of *B. coli.*

Some Relations of Bacteria in Milk: C. E. Marshall and Bell Farrand, Michigan Agricultural College.

Influence of Temperature on the Functional Activity of Lactic Bacteria: C. E. Marshall and Louise Rademacher, Michigan Agricultural College.

Lactic Acid Bacteria in Milk: D. H. Bergey, M.D.

Conn and other authorities report the occurrence of two principal types of lactic bacteria in milk. These two types of organisms correspond to *Bacterium lactis acidi* of Leichmann and *Bacterium acidi* *lactici* of Hueppe. Kruse regards *Bacterium acidi lactici* as identical with *Bacterium aerogenes,* and Heinemann is of the same opinion. Heinemann believes that *Bacterium lactis acidi* is a myth and that *Streptococcus lacticus* is the principal lactic organism in milk.

A number of samples of 'certified' and 'market' milk were analyzed in order to ascertain the nature of the lactic acid bacteria to be encountered in such milk.

The organisms encountered in certified milk were principally staphylococci and streptococci, the former type being by far the most numerous. Neither of the other types of lactic acid bacteria were found in certified milk, except in isolated instances.

The organisms encountered in market milk were principally rod-shaped organisms, and some of these were of the type of *Bacterium acidi lactici* of Hueppe.

The exact nature of all of the lactic acid bacteria encountered in market milk is still undetermined. Their sugar-splitting powers are believed to assist in a more definite classification.

Commercial Bacterial Inspection of Milk and Its Results: S. C. PRESCOTT.

The problem of providing sweet, clean, and wholesome milk to the inhabitants of large cities is one which demands attention both from the sanitary and the commercial standpoints.

Two distinct lines of action have been suggested: first, to limit the production of milk to individuals who will comply with a strict set of laws or ordinances; and second, to treat milk in some way so as to destroy the bacteria.

After more than eighteen months constant supervision of about two hundred farms I should like to present a record of some of the results of bacterial examination and farm inspection.

It must be accepted, I think, that for a long time to come the major portion of the milk consumed in a large city will be the product of the ordinary farm, handled by ordinary men, and shipped by the ordinary methods of transportation. Too sweeping demands for immediate reforms, or laws which can not at present be obeyed, will serve only to precipitate a milk famine.

In this work I have attempted first of all to visit every farm, and to give the farmer whatever help was possible by suggesting improvements in his methods or appliances, and by explaining the reasons for our inspections. Especial attention was given to the effect of cleanliness and cold. A small pamphlet on clean milk, written in very homely language, has been sent to each producer.

I have graded the milk from these farms for purposes of convenience into six groups, according to the number of bacteria, as follows:

Grade A, Below 10,000 bacteria per cubic centimeter.

Grade B, 10,000 to 50,000 bacteria per cubic centimeter.

Grade C, 50,000 to 100,000 bacteria per cubic centimeter.

Grade D, 100,000 to 250,000 bacteria per cubic centimeter.

Grade E, 250,000 to 500,000 bacteria per cubic centimeter.

Grade F, Above 500,000 bacteria per cubic centimeter.

The percentage of samples exceeding 500,000 fell from 30.5 in June, 1905, when inspection began, to 2.3 in May, 1906; but with the advent of hot weather and the scarcity of ice, rose, as would be expected, to a maximum of 12.1 per cent. in July, 1906, but dropped again as cooler weather approached.

On the other end of the scale, the percentage of samples containing below 10,000 gained from 2.0 in June, 1905, to 37.3 per cent. in February, 1906, and at no time since has it fallen below 22.2 per cent.

The percentage of samples containing fewer than 50,000 has never fallen below 50 since September, 1905, three months after inspection work began.

Experiments on the Germicidal Action of Fresh Cow's Milk: P. G. Heinemann.

The question whether fresh cow's milk contains bactericidal substances or not has been answered in the affirmative by Fokker, Ehrlich and Brieger, Park, Kolle and his coworkers, Hunziker, Hippius, Koning, and others. In opposition to this, Moro, Honigmann, Basenau, Stocking and others have denied the existence of such substances.

Experiments by the writer carried on by inoculating raw milk and milk heated to 56° C. and to the boiling point indicate that fresh cow's milk contains germicidal substances, although to a smaller degree than blood serum. Milk was obtained directly after milking and divided into three portions, one of which was heated either to boiling or kept at 56° C. for 30 minutes. The second part was left without further treatment, and then both heated and raw milk were inoculated with suspensions of bacterial cultures. The third part was kept as control. Plates were prepared from suitable dilutions and the colonies counted after two days incubation at 37° C. The three lots of milk were kept at room temperature and plating repeated at regular intervals. The results lead to the following conclusions:

Conclusions.—1. Raw milk contains substances which are germicidal to a pronounced degree for some species of bacteria. (*B. coli, B. dysenteriæ* (Flexner), *B. fluorescens, non-liquefaciens.*)

2. Raw milk contains substances which have slight germicidal action on some species of bacteria. (*B. violaceus, B. choleræ suis, B. prodigiosus* (laboratory culture), and some saprophytes isolated from milk.)

3. The germicidal substances in milk do not act strongly on *B. fluorescens liquefaciens, B. typhosus,* some varieties of *B. prodigiosus* and *B. proteus,* but the multiplication of these organisms is restrained for a limited period.

4. The germicidal action of cow's milk persists for more than 5 hours and less than 7 hours at room temperature.

5. The germicidal action of cow's milk is destroyed by keeping milk at 56° C. for 30 minutes or by heating to the boiling point.

6. The germicidal substances in cow's milk are less powerful than those of blood serum, but are inactivated under similar conditions. The relative concentration of these substances varies in milk from different animals.

S. C. Prescott,
Secretary

SCIENTIFIC BOOKS

Experimentelle Beiträge zur Morphologie. Hefte I. and II. Herausgegeben von Herman Braus (Heidelberg). Leipzig, W. Engelmann. 1906.

The study of experimental morphology, which in recent years has attracted so large a body of enthusiastic students, has been taken up very largely from the dynamic or physiological point of view. This is indicated by the title of the journal most specifically devoted to this line of work—W. Roux's 'Entwicklungsmechanik der Organismen.' Yet, although some physiologists, like Pflüger and Loeb, have done much to stimulate interest in this direction, it is chiefly to professional zoologists and anatomists that the subject has appealed, while the immediate predecessors and many of the contemporaries of these same zoologists and anatomists have been interested rather in phylogenetic and historical than in dynamic biological questions.

The purpose of these Beiträge is to emphasize the value of experimental and accidental teratology for the elucidation of phylogenetic morphological problems. This purpose is set forth in the paragraph which concludes a preface to the Beiträge in which Braus dwells on the distinctions between the historico-genetic and the dynamic aspects of biological problems. "The close relations," he says, "of this branch of experimental embryology to the program of 'Gegenbaur's Morphologisches Jahrbuch' led me, by agreement with the editor and publisher of this journal, to embody my 'Experimentelle Beiträge zur Morphologie' in it and thus to certify their appurtenance to the life work of the founder of modern morphology. On the other hand, in order to emphasize the unity of purpose of the 'Experimentelle Beiträge' and to make them more readily accessible to those who care especially for the broad aspects of experimental embryology and for its relations to the physiology of development, the Beiträge following their appearance in the Jahrbuch are to brought out in the form of separate Hefte. It is anticipated that several Hefte can be bound into a volume. Should they grow in general into a collection for experimental-embryological work which has the historic-morphological problem as its aim, yet in which sight is never lost of the relations of the facts discovered to the other aspect of biological investigation, that of the physiology of development, there may be added other volumes to the series and the progress here aimed at may prove lasting."

Thus far two Hefte have appeared. These contain reprints from the *Morphologisches Jahrbuch* of articles on experimental embryology and teratology by Braus and one of his pupils. Bd. I., Heft 1, is given up to the preface above mentioned, and to a paper entitled 'Ist die Bildung des Skeletes von den Muskelanlagen abhängig.' A series of experiments on the developing pectoral fin of sharks leads Braus to conclude that in the selachian fin the skeleton develops independently of the musculature but that parts of the skeleton are dependent upon other parts for stimulus to development.

In Bd. I., Heft 2, there is an article by O. Bender entitled 'Zur Kenntnis der Hypermelie beim Frosch.' This gives a description of the external form and of the skeletal muscles and nerves of a left supernumerary hind leg of a frog. Deductions are drawn as to the probable cause of the anomaly.

The chief paper in this Heft is one by Braus on the 'Vordere Extremität und Operculum bei Bombinatorlarven' in which Braus shows that although normally the fore limb at a certain stage of development appears to break through the operculum yet, if no fore-leg is developed, there is none the less an aperture formed as if the leg were present. The developing limb has the power to force its way through a covering of skin artificially formed above it. The results of the experiments are considered from the point of view of phylogenetic morphology.

All anatomists acknowledge the great help which the study of normal embryology has been in the development of a science of structural form. That accidental and experimentally produced abnormality of structure may help to clear up obscure fields of anatomy has been abundantly proved of late years. Abnormalities and variation in the structure of man and some other animals have long been used as a basis for phylogenetic speculation. It is not improbable that teratology controlled through experiment may throw interesting light on this aspect of biology. We trust that the 'Beiträge' may serve to stimulate more work in experimental embryology in its morphological aspects. On the other hand, it is to be hoped that vague speculation will not take up an undue amount of the pages devoted to the subject.

C. R. B.

Rhythmical Pulsations in Scyphomedusæ. By ALFRED G. MAYER, Director of Department of Marine Biology of the Carnegie Institution of Washington, Tortugas, Florida. Pp. 62, with 2 plates and 36 text figures. Washington, D. C. The Carnegie Institution. 1906.

Among the wealth of new material for investigation which the Carnegie Station at Tortugas has placed at the disposal of the scientific public Dr. Mayer has discovered that a small scyphomedusæ, *Cassiopea xamachana*, offers exceptionally favorable material for the study of rhythmic pulsation; and to the reactions of this species he devotes the greater portion of his paper on rhythmical pulsation. In this species he has discovered a new and very interesting way in which rhythmical pulsations may be maintained. In general, in medusæ, the pulsations are originated in the margin, which contains the sense organs and the greater part of the nervous system. If this margin be cut off the central part of the disc does not, in general, continue to contract in sea water. Romanes, however, found that by passing a weak constant current through such an emarginated disc rhythmical contractions could be kept up in *Aurelia;* and Loeb has found that the central part of both *Gonionemus* and *Polyorchis* can be made to pulsate continuously in appropriate chemical solutions, which are different for the two forms. Mayer has found that if in *Cassiopea* the marginal sense organs are removed, the remainder of the disc does not pulsate in sea water. If from this disc a ring of tissue be cut, or if the disc be cut in other ways, so that a closed circuit of uninjured tissue be left, in which an impulse can pass continuously from one point, around the circuit, to its point of origin, without passing over any portion of its path twice, it still does not pulsate. If, now, a disc which has been cut in this way is strongly stimulated electrically, mechanically or chemically, a wave of contraction starts from the point of stimulation, and passes with constantly diminishing intensity around the circuit. When it reaches its point of origin it is suddenly reinforced, and starts around the circuit again with renewed vigor. In this way a perfectly regular series of contractions is originated which keeps up indefinitely. The rate at which these contractions follow each other depends upon the length of the path from the point of origin around the circuit and back again. If, now, during such a series of pulsations, the ring of muscular tissue be cut across so that there is no longer a closed circuit, the pulsations stop instantly. The point from which the pulsations start does not always remain at the original point of stimulation, but may shift to some other place. In such cases, and when the series of pulsations has been started by an unlocalized stimulus, there is a decided tendency for the point of origin of the pulsations to be located in the geometrical axis of symmetry of the figure into which the disc has been cut.

Although Dr. Mayer does not discuss this point, phenomena of this kind have a special interest for us on account of the analogy which they may furnish for some of the processes going on in the central nervous system; and for these reasons it is to be hoped that the cause for the characteristic location of the point of origin of the pulsations, the nature of the reinforcement given to the wave of contraction, the nature of the reciprocal influence of two points of origin on each other, and similar questions will be still farther studied.

Many other questions, which it will be impossible to discuss here, are also investigated. Among them the rôle of the various salts of the sea water in stimulating and inhibiting the pulsations is considered, but as these chemical questions are not considered from the point of view of the modern general physiology, which is based upon physical chemistry, they are less satisfactory than other portions of the paper. FRANK W. BANCROFT

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Experimental Zoology, Vol. IV., No. 1, February, 1907, contains the following papers: 'Abnormal Development of Toad Ova Fertilized by Spermatozoa exposed to the Roentgen Rays,' by Charles R. Bardeen. 'An Ecological and Experimental Study of Sarcophagidæ with Relation to Lake Beach Debris,' by William B. Herms. This is a study of the conditions presented by the flesh-feeding fly-larvæ, and the adaptations which have resulted from a certain amount of isolation and apparently fairly regular periodicity of food (fish) supply in the particular locality studied, and determination of minimum food supply required for the maturity of individuals as well as the optimum and maximum periods of feeding. 'Rejuvenescence as the

Result of Conjugation,' by Sara White Cull. Experiments were made to determine (*a*) whether conjugation always produces rejuvenescence and (*b*) whether the fertilization resulting from conjugation is or is not mutual. Conjugating pairs of *Paramecium caudatum* were isolated and the individuals, after they had been separated, were isolated and counted at intervals for a month. The statistics gathered show that (*a*) conjugation frequently fails to produce rejuvenescence in either conjugant and (*b*) that conjugation, among the infusoria, is not mutual and there are strong indications that it is incipient fertilization as seen among higher forms. 'Artificial Parthenogenesis in Thalassema mellita,' by George Lefevre. An investigation of artificial parthenogenesis in *Thalassema mellita* has shown that the unfertilized eggs of this worm can be induced to develop into actively swimming trochophores by immersion for a few minutes in very dilute solutions of both inorganic and organic acids. Nitric, hydrochloric, sulphuric, carbonic, acetic and oxalic acids were used successfully, and in favorable experiments 50–60 per cent. of the eggs developed into swimming larvæ that were hardly distinguishable from normal trochophores of a corresponding stage. The parthenogenetic development in many cases involves a perfectly normal maturation, a more or less regular cleavage, and the usual processes of differentiation leading up to the formation of the normal larva. The reduced number of chromosomes (twelve) persists when the maturation has been normal, and has been repeatedly counted even in late blastula and gastrula stages. Differentiation of the egg does not occur in the absence of cleavage, and all ciliated bodies, whether normal or abnormal, possess a cellular structure. 'Concerning the Theory of Tropisms,' by Jacques Loeb. 'The Mechanism of the Galvanotropic Orientation in Volvox,' by Frank W. Bancroft. O. P. Terry's result, showing that volvox colonies subjected to a bright light swim to the cathode, while those subjected to a dim light or darkness swim to the anode was confirmed. This difference was found to be due to a reversal of the pole at which the galvanic current acts. In anodic colonies the galvanic current stops the flagella current on the anode side of the organism. In cathodic colonies the orientation is produced by stopping the flagella current on the cathodal side. Pressure on anodal colonies reverses the pole at which the galvanic current produces its effect.

SOCIETIES AND ACADEMIES

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 633d meeting was held on April 13, President Hayford in the chair.

Professor Newcomb discussed the question of the variability of the sun's radiation, giving the results of a statistical investigation on that subject. The line of investigation was directed upon the point whether there is any tendency to synchronism between spells of high and low temperature in widely separated regions of the earth. The material included annual mean temperatures, as observed from 1820 to 1904, and deviations during terms of ten days, and of one month, from 1872 until 1904. The general conclusion was that there were no well-marked deviations other than those which resulted from local causes, and that when these were duly allowed for, the temperature of the earth at large remained constant within a fraction of a degree. The only appreciable indication of any cosmical cause affecting the whole earth was in the monthly deviations. So far as the investigation had been carried, these indicated that there really was a deviation, which could be accounted for by a change in the sun's radiation from month to month, sufficient to change the temperature of the equatorial regions by an average amount of about one third of a degree centigrade. But even this small change may be due to the trade winds and other great movements of the air, by which a body of air colder or warmer than the normal is carried from one region to another within the monthly limits. The only ascertained law of change was that of the sun-spot period. The result reached by Köppen, that the temperature of the earth generally was somewhat higher the fewer the sun-spots, is confirmed; but the amount of the change is less than one half that found by Köppen, the deviation being only 0°.13 C. on each side of the mean.

The general conclusion was that if the sun's radiation of heat is variable at all, the amount is too small to have any appreciable meteorological effect.

It was distinctly set forth that the research referred only to the radiation of heat, and not to the electric radiations which are known to proceed from the sun, which vary from time to time and which are related to magnetic storms, and perhaps also to the atmospheric electricity.

In the course of the general discussion of Professor Newcomb's paper, Dr. Abbott exhibited some diagrams showing the variations of terrestrial temperatures corresponding to certain variations of solar radiation as derived from bolometric measurements at a mean latitude of 39°.6 N. He called attention to other curves he had constructed showing the annual temperature variations observed at coast and inland stations in the United States, the variation at the inland stations being three times that at the coast station. Another curve was shown of an hypothetical earth (earth assumed as a black body) which truly followed the variation of solar radiation.

At the conclusion of the meeting Dr. Abbott called attention to a disc photometer which he had recently designed, and which he afforded the society the opportunity of seeing in operation.

R. L. Faris,
Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

At the 189th meeting of the society held in the Cosmos Club on Wednesday evening, March 20, the president announced the receipt of a letter from the Washington Academy of Sciences stating the intention of the academy to erect a building to be devoted to the use of the scientific societies of Washington, and asking that the Geological Society indicate the amount of room needed and how much it would contribute to the building fund. Action concerning the matter was deferred to the next meeting.

Regular Program

The following papers, illustrated by lantern slides, were presented:

Some Features of the Geology of Magdalena and Black Range Region, New Mexico: C. H. Gordon.

The area to which this paper relates lies along the west side of the Rio Grande Valley, extending from near the north line of Socorro County southward to Deming. The valley of the Rio Grande here is from twelve to forty miles in width and is bounded on the west by several parallel mountain ranges which constitute the southeastern border of the Great Plateau country as outlined by Dutton. In the Black Range a nearly complete sequence of Paleozoic rocks occurs, resting upon the eroded surface of pre-Cambrian granites and schists. In the Magdalenas the Lower Carboniferous limestone rests directly upon greenstone schists and granites. Rocks of Cretaceous age occur in a number of places, resting upon the eroded surface of the Pennsylvanian series. No Tertiary sedimentaries were observed, this period being characterized by extensive eruptions of andesite, and rhyolite. Intrusions of monzonite and quartz-monzonite and their associated porphyries are common, and with them occur also granite porphyries, and rocks of basic composition like diabase. Remnants of extensive flows of basalt occur, resting upon the mesa gravels filling the valley.

Structurally the region is characterized by block faulting. With the uplift of the plateau on the west there appears to have been a relative sinking of the belt now represented by the Rio Grande Valley, with crustal breaking and readjustment giving rise to tilted blocks which appear here and there as elevations rising out of the general plain level. Extensive erosion has supplied from the adjoining slopes a vast amount of débris which covers the valley in places to a depth of from 1,000 to 2,000 feet.

At the north end of the Magdalena range near the town of Magdalena are located the lead and zinc mines of Kelly. The ores occur along the bedding planes of carboniferous limestones which have a westward inclination of 30° to 40°. The sedimentary block which constitutes the back (west) slope of the mountain is affected by numerous faults, the chief of which extend north and south, and constitute the main fault scarp on the east side of the range. At the foot of the range on the west side, the sedimentary rocks are cut off by quartz-monzonite-porphyry the intrusion of which appears to have furnished the solutions from which the ores were derived. Of interest in this connection is the occurrence of pyroxene intergrown with amphibole and specularite together with quartz and calcite along the boundaries of the sulphide ore bodies with the inclosing limestone.

The Santa Maria Oil District, California: Ralph Arnold.

The Santa Maria oil district, comprising the Santa Maria, Lompoc and Arroyo Grande fields, occupies the central and northern portions of the Lompoc and Guadalupe quadrangles, northern Santa Barbara County, and the southern part of the San Luis quadrangle, southern San Luis Obispo County.

The San Rafael Mountains, 2,000 to 6,000 feet high, trend northwest across the northeastern portion of the area, while the Santa Ynez Range skirts the southern. Occupying the angle between the two ranges are low hills and broad valleys. The developed fields cover the flanks of two of the low ridges in this basin region, the main or Santa Maria field proper being located on Graciosa Ridge, about eight miles south of the town of Santa Maria.

The formations involved in the geology of the district include the Franciscan (Jurassic) sandstone, shale, glaucophane schist, jasper and intruded serpentine; Knoxville (lower Cretaceous) conglomerate, sandstone and shale; pre-Monterey (which may include both Cretaceous and older Tertiary) conglomerate, sandstone and shale; Sespe (Eocene or Oligocene) sandstone; Vaqueros (lower Miocene) conglomerate, sandstone and shale; Monterey (middle Miocene) diatomaceous and clay shale and volcanic ash; Fernando (Miocene-Pliocene-Pleistocene) conglomerate sandstone and shale; and Quaternary gravel, sand, clay and alluvium.

Two structural systems prevail in the district, those features in the northeastern portion striking northwest and southeast, those in the southern portion striking east and west; while in the intervening region are features trending in a direction intermediate between the two. Few faults of importance were noted in the field. The productive territory lies in a region of more or less gentle folds in the central part of the area, the wells usually being located along or near anticlines.

The wells vary in depth from 1,500 to over 4,000 feet. In the Santa Maria and Lompoc fields they obtain their oil from zones of fractured shale or sandy layers in the lower portion of the Monterey (middle Miocene) formation. The production of the individual wells varies from 5 to 3,000 barrels. The gravity of the oil ranges from 19° to 35° Baume, the yield from the greater part of the field being about 25° to 27°. In the Arroyo Grande field the oil comes from sandstone at the base of the Fernando and is of 14° gravity.

Notes on the Geology of Japan: Robert Anderson.

This paper gave an outline of the general topographic features and geological structure of the Japanese islands. To sum up the main points: The ground plan of the group was laid during the earliest geological times, Archæan gneisses and schists and a great thickness of Paleozoic sedimentary, metamorphic and igneous rocks being the basement complex. Mesozoic, Tertiary and Quaternary formations are wide-spread and diversified, having the character of a superstructure over an older land mass. The island chain is continental in character, not chiefly of volcanic origin, although volcanic activity has always been a feature of its history. It is a country of long-continued and great geological activity, as shown by the variety of its rocks, by the presence of a thick column of strata representing the earliest and latest as well as intermediate times, by the metamorphism and excessive disturbance of these strata as the result of repeated movements, by the presence of many igneous intrusions and volcanic remains which show that similar igneous forces as are at work to-day have always been active, and by the modern land movements forming depressed broken shore lines and terraces and keeping a fairly new face on the topography of areas occupied by the oldest rocks, these movements being illustrated in the every-day earthquakes to which Japan is subjected.

In a number of points a similarity exists between the geology of Japan and that of the western coast of America: In the existence in both California and Japan of a somewhat similar thick basement complex; our Franciscan is probably Jurassic in age; the Japanese terrane with which it might be compared is supposedly Paleozoic; both formations are intruded by similar serpentine of Jurassic or Cretaceous age. In the apparent Mesozoic age of the wide-spread and important intrusions of granite. In the similarity of the old and recent faunas and floras, as has been partially brought out by a few paleontologists. In the presence in both countries of a large amount of schist formed of the blue amphibole glaucophane, whereas it is rare in all but a few places elsewhere. In the fact that both countries have been long coastal belts of volcanic activity during Tertiary and Quaternary times, the thickness and wide extent attained by the Tertiary tuffs in Japan being remarkable. In the recency of earth movements, as shown in the multitude of earthquakes, the presence of upraised Quaternary deposits and marine terraces, and by the evidence of present oscillations in the level of the coasts. And the resemblance holds good particularly in the tremendous land-building activity of the Tertiary on both sides of the Pacific, contributed to by tectonic movements and the rapid denudation of land areas and deposition along narrow belts. This feature is illustrated by the great thickness of the Tertiary formations, their structural disturbance and their lithologic alteration.

Ralph Arnold,
Secretary

THE CHEMICAL SOCIETY OF WASHINGTON

The 174th regular meeting of the Chemical Society of Washington was called to order by President Fireman, on April 11, at 8 P.M.

A committee, consisting of Messrs. L. M. Tolman, F. K. Cameron and S. S. Voorhees, was appointed to consider the advisability of cooperating with the Washington Academy of Science relative to the erection of a suitable building to serve as a home for the academy and all the affiliated societies.

The president of the society was invited by the commissioners of the D. C. to confer with a committee appointed to consider the milk supply of the district.

S. S. Voorhees read a paper on 'Buying Coal under Specifications.' The speaker said that the present specification was the outgrowth of six years' experience in the purchase of coal by the Treasury Department. The specification requires the bidder to state the name and location of mines from which deliveries will be made with per cent. of ash in dry coal and B. T. U. in coal as delivered; the price paid for deliveries is based on price stated in proposal corrected for heat value and ash found in average sample representing deliveries above or below standard established by bidder.

W. O. Robinson read a paper on 'The Solubility of Calcium Sulphate in Aqueous Solution of Sodium Sulphate and Sodium Chloride at 25°.' At 25°, Cameron and Seidell found the solubility curve for the system sodium sulphate, sodium chloride and water to consist of three branches representing solution in equilibrium with sodium sulphate decahydrate, anhydrous sodium sulphate and sodium chloride.

Calcium sulphate was introduced into this system and, after complete reaction had taken place, was found to occur in the solid phase as gypsum at the sodium sulphate and sodium chloride ends of the curve. In intermediate concentration, however, the solid phase containing calcium occurred as fine needle-like crystals. As these were decomposed by washing, ordinary methods of analysis could not be used. By the double triangular diagram method, this calcium salt was found to be represented by the formula $3Na_2SO_4 \cdot 2CaSO_4$. This conclusion was fully confirmed by the zero method and by precipitation with a weighed amount of calcium sulphate.

H. E. Patten's paper on 'Energy Changes accompanying Adsorption' showed that absorption could be looked upon as a special case of adsorption by combining the inbibition effect with the adsorption effect where both take place in the fine pores of a cellular body. Where the pores become very minute we may think of a solid solution as a limiting case of such an absorption effect. A résumé of the energy changes accompanying absorption was given.

On May 1, a special meeting was held at the George Washington University Lecture Hall. This was the first of a series of meetings to be held for the discussion of sanitary matters. W. C. Woodward, M.D., health officer of the District of Columbia, spoke on the 'Health Department of the District of Columbia, its Functions and Organization.' The speaker gave a history of the department; its relation to other branches of the city government; and told about the work of enforcing the smoke-, food-, marine products-, milk- and slaughter-house-regulations.

J. A. LECLERC,
Secretary

BUREAU OF CHEMISTRY,
WASHINGTON, D. C.

DISCUSSION AND CORRESPONDENCE

ANOTHER WORD ON THE VULTUR CASE

IN SCIENCE of May 3 (pp. 708, 709) Mr. Stone makes a brief reply to my article on how the 'first species' rule works in determining genotypes in ornithology.[1] Inasmuch as he makes no attempt to traverse the principal positions there taken, it is perhaps almost ungracious again to open the subject. For the expert no reply is necessary, but the general reader may be misled by some of his statements.

Of the seven cases he would throw out from my list of twenty-one generic changes made necessary by the first species rule, *Spinus* may be saved by the rule of tautonomy, and *Colymbus* may be excluded by the provision exempting Linnæan genera from its scope. Respecting the other five cases, Mr. Stone and I simply hold different views, and the details need not be here discussed.

In regard to the 'several inconsistencies' he claims to have pointed out in the *Vultur* case, one I frankly admitted, and explained as a pure blunder; the rest of the 'several' exist only in his imagination. While *gryphus* is the type of *Sarcorhamphus,* founded in 1806, it did not become its type at that date; it did not become the type till the other two of the original three noncongeneric species had been removed, and thus does not in the least affect the type of *Vultur* as determined by my elimination. By the current usage of all 'experts' in elimination—except Mr. Stone—*aura* and *papa* both go out at 1816, instead of the latter at 1854, as claimed by Mr. Stone. So this 'excellent illustration of the complexity of the elimination method and the opportunities it offers even to experts to fall into errors' fails completely to illustrate anything except Mr. Stone's ideas about methods of elimination.

J. A. ALLEN

NEW YORK,
April 8, 1907

[1] See SCIENCE, N. S., Vol. XV., No. 640, pp. 546–554, April 5, 1907.

SUNSPOT ZONES

TO THE EDITOR OF SCIENCE: It occurs to me that Sporer's law of the sunspot zones might be accounted for in this way: When the last ring of planetary material was detached, it seems likely that a part of the material of the sun should have been lifted with this ring, only to fall back into the sun after the moment of parting. In the gaseous mass of the sun this may be supposed to have produced a system of waves of ring-like shape, whose velocity of propagation might be such as to pass from latitudes 30 to 5 in fourteen years. Their paths might perhaps be such as to come nearest to the surface in the latitudes where the sunspots have their maxima.

Any such progressive disturbance near the surface of some deep layer in the sun might be sufficient, in connection with the deflective influence of the sun's rotation, to occasion surface eddies, 'cyclones' as suggested by Faye. Or, they might cause 'eruptive' phenomena in the unstable static conditions of the outer layers of the sun.

I am not aware that such a suggestion as this has ever been made to account for Sporer's law. Of course I see many objections to it. The improbability of such waves long retaining their shape, and the observed absence of sun spots in the north hemisphere from 1672 to 1704, suggest themselves as obstacles.

J. A. Udden
Rock Island, Ill.

AFRICAN BASKETRY WEAVES

A large collection of ethnological specimens recently received by the Museum of Natural History, New York city, contains a selected series of baskets from the Barotse and Bechuana tribes. These baskets compare favorably in technique and finish with those of California and, what is of special interest, present all the typical weaves known in America. Among the Barotse baskets alone we find the following kinds of woven basketry: wicker, checker, twill, wrapped, plain twine, open twine, twilled twine, and the California 'ti.' Also in coiled basketry, one rod coil, grass coil closely covered, also with foundation showing bifurcated coil closely covered, also with only lines of stitching and coil without foundation.

The more elaborate manipulation of warp elements or materials in general, seems not to be practised, for there are neither cross-warp checkers nor cross-warp twines in the collection. The edges, while of all types, represent rather the refined variations. The thought of solidity or stability seems to have been the main idea. This is particularly well illustrated in the large rope coil and one-rod foundation coil chests and storage baskets. The technic is perfect and with the close-fitting lids give exceptional protection to grain or other contents. Strengthening by bands of the 'ti' weave, a technic heretofore thought to be limited to the Pomo Indians of California, is found; also an unusual wrapped twine, with the horizontal warp on the outside, like some Aleutian burden baskets.

The decorations in color occur chiefly in coil baskets. The color of the designs is uniformly black.

The main point of interest is that within a definite ethnic area of South Central Africa an aboriginal people practise basketry in which are found practically all of the typical weaves known to the world.

Mary Lois Kissell
American Museum of Natural History,
New York City

THE RECUPERATIVE POWER OF ITALIAN AND ENGLISH WORKMEN

To the Editor of Science: I am afraid that the statements contained in the letter of Mr. Joseph Y. Bergen,[1] as to the recuperative power of Italian and English workmen may induce some readers to unwarranted generalizations.

A statement of the kind would have more value if a comparison were made between the diet and the conditions of people of the *same race* and the same locality, provided in each case there existed a sufficiency of the articles of diet.

On the other hand, it is a known fact, that, generally speaking, the rural population of Europe has better recuperative powers than the inhabitants of the cities, although the latter eat much meat, while the country people live almost exclusively on a vegetable diet, meat being considered too expensive.

L. H. Baekeland

SPECIAL ARTICLES

SOME LATENT CHARACTERS OF A WHITE BEAN[2]

In order to secure material to display as simple illustrations of Mendel's laws of dominance and gametic purity, I made reciprocal crosses last year (1904) among four different varieties of the common bush bean. These varieties were the 'Prolific black wax,' with purple-black seeds, the 'Ne plus ultra' with yellow-brown seeds, the 'Long yellow six-weeks' with seeds of a light greenish-yellow color, and the 'White flageolet,' whose seed-coats are wholly without pigment, being transparent when saturated with liquids, but nearly white because of the inclusion of air when dry.

[1] Science, May 3, 1907, page 709.

[2] Read before the Botanical Society of America, at New Orleans, December, 1905.

I have this year (1905) the first generation of the hybrids, and all the crosses behaved in the expected way except those in which the 'White flageolet' entered as one of the parents. The crosses between black and brown, black and yellow, and between brown and yellow showed in every case the complete dominance of the darker pigment over the lighter. Thus, the first-generation hybrids between black and brown beans and those between black and yellow were in every case indistinguishable in color from the black parent, whether the black bean supplied the egg or the sperm. Similarly the hybrids between the brown and yellow were in each case not to be distinguished from the brown parent. But the crosses between each of these three pigmented varieties and the 'White flageolet' gave F_1 hybrids so different from either parent that, if unknown, their origin would never be guessed. These seemingly anomalous hybrids were quite indistinguishable from one another, whether the pigmented parent was black, brown, or yellow. They were characterized by a dark purple pigment, and by an aggregation of the pigment-bearing cells to form a mosaic or mottled surface. Although these hybrids were quite unexpected, it was recalled that Tschermak[2] had secured similar results in a number of cases, and Emerson[3] describes them also in crosses between 'Ultra' and 'Navy,' this being presumably the very same cross that I present here as 'Ne plus ultra' $\times$ 'White flageolet.'

Very similar phenomena have been seen by Tschermak,[4] Bateson,[5] and Lock[6] in peas, by Tschermak[4] and Bateson[5] in stocks, by Bateson[5] in sweet peas, by Correns[7] in *Mirabilis*, by Cuénot[8] in mice, and by Castle[9] in guinea-pigs. It is not because of the newness of the phenomenon, therefore, that I draw attention to the subject, but for the purpose of discussing the phenomenon of latency in the light of the accumulated data.

The appearance of hereditary characters which are not traceable to the immediate ancestry offer the most difficult problems with which the student of heredity must deal. These characters may be recognizable as having belonged to the more remote ancestry of the form in question, *i. e.*, they are atavistic, or they may be wholly new. Sometimes they occur under known conditions, at other times there is no clue to the causes upon which they may depend.

These bean hybrids which possess characters not seen in either parent furnish good examples of latency and if we can determine whence the new characters came we shall be far advanced toward a correct conception of latency. Taking, for instance, the hybrids between the yellow and white and between the brown and white, two characters may be recognized as new, *viz.*, the dark pigment and the mottled color-pattern. The fact that the very same characters appear in the hybrid offspring, no matter what the character of the pigmented parent, leaves little doubt that both of these new characters are traceable to the white parent, and we should seem to be warranted in saying that this white bean con-

[2] Tschermak, E., 'Weitere Kreuzungsstudien an Erbsen, Leukojen und Bohnen,' *Zeitschr. Landw. Versuchsw.*, **7**: 533–638, 1904.

[3] Emerson, R. A., 'Heredity in Bean Hybrids,' Ann. Rept. Agr. Exp. Sta. Nebraska, **17**: 33–68, 1904.

[4] Tschermak, *loc. cit.*

[5] Bateson, W., Saunders, Miss E. R., Punnett, R. C., 'Experimental Studies in the Physiology of Heredity,' Second Report to the Evolution Committee of the Royal Society, London, 1905.

[6] Lock, R. H., 'Studies in Plant Breeding in the Tropics,' *Ann. Roy. Bot. Gard. Perideniya*, **2**: 299–356, 1904.

[7] Correns, C., 'Ueber Bastardirungsversuche mit Mirabilis Sippen,' *Ber. d. d. Bot. Ges.*, **20**: 594–609, 1903; 'Zur Kenntniss der scheinbar neuen Merkmale der Bastarde. Zweite Mittheilung über Bastardirungsversuche mit Mirabilis Sippen,' *Ber. d. d. Bot. Ges.*, **23**: 70–85, 1905.

[8] Cuénot, 'Le loi de Mendel et l'hérédité de la pigmentation chez les souris,' *Arch. Zool. expér. et gén.*, —: 27, 1902; 'L'hérédité de la pigmentation chez les souris' (2me note), *Arch. Zool. expér. et gén.*, 4 Se., **1**: 33–41, 1903.

[9] Castle, W. E., 'Heredity of Coat-characters in Guinea-pigs and Rabbits,' Publ. Carnegie Institution of Washington, No. 23, 78 pp., Feb., 1905.

tains latent purple and latent mottling. Tschermak[10] takes just the opposite view, however, and would say that the purple mottling is latent in the pigmented bean, and that the white bean acts simply as a releasing agent which allows the latent character to become manifest. Bateson[11] also subscribes to the same view. On Tschermak's hypothesis it would appear to me a remarkable coincidence that the black, brown and yellow varieties should all contain the same latent characters, and I am forced to the conclusion just stated, that the novelties which appear in these hybrids are directly derived from the *white* parent. The new characters thus lose the mystery that otherwise invests them. They appear not as released prisoners or awakened sleepers, which had for some inconceivable reason remained inactive, but are *combination* phenomena.

The color characters of these beans are not dependent upon a single pair of units, but upon three pairs, *viz.*:

1. Pigment *vs.* no pigment.
2. Modifier which changes pigment to purple *vs.* no modifier.
3. Mottled color pattern *vs.* self color.

Of these three characters, the brown and yellow beans contain only the dominant pigment character and might be represented by the formula, *Pbm*; the black bean contains the pigment and the modification to purple, *PBm*; and the white bean contains both the modifier and the mottled color-pattern but no pigment, *pBM*. It is only because of the lack of pigment that these two characters possessed by the white bean are not apparent. *They are latent only in the sense that they are invisible.* Whenever the 'White flageolet' is crossed with any variety of self-colored bean, the three dominant allelomorphs, *PBM*, are brought together with the result seen in these hybrids, namely, a first generation characterized by dark purple mottled seeds.

[10] Tschermak, E., *loc. cit.*

[11] Bateson, W., Saunders, Miss E. R., Punnett, R. C., 'Experimental Studies in the Physiology of Heredity,' Second Report to the Evolution Committee of the Royal Society, London, 1905.

The great advantage of this explanation over that of Tschermak is that it brings these apparently aberrant results into harmony with typical Mendelian cases, and allows with a reasonable degree of accuracy, a prediction as to the composition of subsequent generations.

On the assumption that the F_1 hybrids between the 'White flageolet' and either of the non-purple self-colored beans contain the three dominant allelomorphs, *PBM*, associated with the corresponding recessive allelomorphs, *pbm*, the following forms and proportions may be expected in the second generation. In each 64 plants belonging to the second generation there should be

27 *PBM* = purple mottled like F_1.
9 *PbM* = brown mottled or yellow mottled.
9 *PBm* = purple self-colored or black.
9 *pBM* = white, exactly like the white parent.
3 *Pbm* = brown or yellow self-colored.
3 *pBm* = white (with the modifier but no mottling).
3 *pbM* = white (without the modifier but with mottling).
1 *pbm* = white (with neither modifier nor mottling).

Owing to the fact that the internal composition of the white beans has no external manifestation, the four white classes having different allelomorphic composition are indistinguishable from one another, thus resulting in a frequently found ratio for tripolyhybrids, 27:9:9:3:16.

In this interpretation of latent characters are to be found explanations of several observed phenomena. Tschermak, Bateson and Emerson have noted that the behavior of a given character in one strain can not be used safely as a criterion for predicting the behavior of an apparently like character in another strain. A good illustration of this is seen on comparing with these hybrids of the 'White flageolet' bean, Emerson's cross between the 'Ultra' and 'Marrowfat,' the latter being likewise a white bean. The F_1 hybrids were brown mottled, thus showing that the 'Marrowfat' differs from the 'Navy' or 'White flageolet' in having no dominant pigment-changing allelomorph. The gametic formula of the 'Marrowfat' is, no doubt, *pbM*, and in the table above, representing the F_2 of the cross between 'Ne plus ultra' and 'White flageolet' it is seen that one of the white derivatives from this cross has the same gametic composition as the 'Marrowfat.'

Emerson attempted no explanation of the different behavior of these beans, simply presenting them as exceptions to Mendel's laws or as evidences of the limitation of the usefulness of those laws in predicting the results of hybridization. Practically all of the exceptional results obtained by him cease to be exceptional when we cease to look upon the products of his crosses as monohybrids with respect to seed color. His second generation hybrids were classifiable into four categories instead of the three he expected, but his expectation was based upon the assumption that the black, brown, white, etc., are unit characters, and that the mottled hybrids were simply mosaics or blends between the white and the self-colored parents. The simple assumption, demonstrated in my hybrids, that the pigmentation and the mottling are distinct unit characters, harmonizes his results perfectly, though the numbers with which he dealt were too small for the satisfactory determination of the agreement or disagreement with the theoretical ratios of a dihybrid.

In discussing the appearance of purple spotting as a novelty in peas, Lock[12] follows Tschermak in referring the latent character to the pigmented parent, saying that "On crossing $A(B)$ (B being latent) with ab we get: F_1 $ABab$ (B latent having become B active)." If instead he had considered that Ab is crossed with aB, the A producing the pigment and the B aggregating it into spots, he would get the same F_1, namely $ABab$, but would have avoided the difficulty of a capricious unit which may be active or inactive under conditions that can not be determined. He would then have had no latent character in any other sense than that it was invisible owing to the absence of pigment.

The explanation here offered is essentially that presented by Cuénot for mouse hybrids, in which one unit is assumed to give pigmentation and another to determine the color which this pigment will exhibit. Cuénot considers the various colors to be latent in the albino and he is supported in this respect by my hybrids, but I prefer not to call this character a *latent pigment* but an *active pigment-changer*.

This reference of various colors to the action of a pigment-changer requires that the pigments upon which the various colors depend shall bear some simple relation to each other. I have made some preliminary studies on the pigments of these beans and have partially demonstrated this simple relation by converting the yellow and brown pigments to black by the use of alkalies but I have not yet been able to reverse the process. It is easily demonstrable that the black (dark purple) bean contains anthocyan, and this gives a simple explanation of the correlation between black seed-coats and red flowers, observed by Mendel and all other students who have chanced to use black-seeded peas or beans.

That the yellow, red, and black pigments of animals are closely related is also well known, and there can be no doubt that the 'latent black' which Castle[13] reported in certain albino guinea-pigs is to be interpreted exactly as Cuénot's mice, the black being due to the presence of a melanizer which is a unit character wholly independent of the pigment-producing unit. The fact that half the gametes of this individual carried the so-called 'latent black' simply showed the animal to be heterozygous with respect to this allelomorph, and the extracted recessives which did not in subsequent generations produce any black offspring could not do so for the simple reason that the pigment-changing unit had acted in a perfectly normal way and had been absolutely separated out into the black offspring while its recessive counterpart was segregated with equal purity into the non-black.

A very important consideration in this connection is the frequency with which the new character is atavistic. This shows the process

[12] Lock, R. H., 'Studies in Plant Breeding in the Tropics,' *Ann. Roy. Bot. Gard. Perideniya*, **2**: 299–356. See p. 341.

[13] Castle, W. E., *loc. cit.*

by which these various color varieties were originally produced. The original character was compound and the new variety was produced by the loss of one or more of the components. In other words these varieties are retrogressive. Beginning, for instance, with a purple mottled bean, one variety was formed by the loss of the mottling, another by the loss of the pigment, and another by the loss of the pigment-changer. Then by hybridization every possible combination of these three characters became the constant characteristics of distinct strains. When these varieties are crossed together the original variety may be reproduced by bringing together the several component parts of the original compound character.

There are still many mysteries regarding latent characters or qualities, but I believe the considerations here presented bring a large number of otherwise anomalous phenomena into perfect harmony with typical Mendelian cases of alternative inheritance. It appears to me certain that this conception of latent characters as *invisible* ones, which has already been used by Correns[14] to interpret in part the behavior of Mirabilis hybrids, can be extended to clear up his remaining difficulties, and that Bateson will find in the same conception an explanation of the complex behavior of his sweet peas and stocks without resort to the inexplicable synthesis and resolution of supposed hypallelomorphs.

George Harrison Shull
Station for Experimental Evolution,
Cold Spring Harbor, Long Island,
December, 1905

[14] Correns, C., "'Zur Kenntniss der scheinbar neuen Merkmale der Bastarde. Zweite Mittheilung über Bastardirungsversuche mit Mirabilis Sippen,' *Ber. d. d. Bot. Ges.*, **23**: 70–85, 1905.

QUOTATIONS

THE UNIVERSITY OF OXFORD

This appeal is now made on behalf of Oxford by the Chancellor and Vice-Chancellor of that University, and it is to be enforced at a public meeting of all persons interested in the subject to be held in London on Thursday, May 16, with the Chancellor of the University in the chair, supported by many men of light and leading from among those whom the University has already trained for the high station they adorn in Church and State. The appeal, however, is not made to old Oxford men alone; it is addressed to "all who are interested in the continued wellbeing and usefulness of the oldest University in the Empire." Nor is it in its present form and purpose an appeal for the complete equipment of the University with all the appliances, institutions, and endowments which would enable Oxford to hold her own in the coming time among the leaders in all departments of letters, learning, science, and the arts. That is an ideal which would require millions for its effective and practical realization. Cambridge has already asked for something like a million and a half and could probably find plenty of use for as much again. American Universities are almost daily being endowed on this hitherto unprecedented but by no means extravagant scale. Oxford is, for the present, less ambitious and perhaps more practical. She recognizes that even for Universities it is true that *non omnia possumus omnes*. The days are gone by, perhaps, when any University, even a multi-millionaire University, can profitably do as Bacon did when he aspired to take all knowledge for his province. Hence such new endowments as Oxford now hopes to obtain—£250,000 is all that is asked for at present, merely a paltry million dollars as American founders and benefactors might regard it—are to be directed into certain definite channels. Largely at the instance of Mr. T. A. Brassey, who has already set a goodly example by his active exertions and personal munificence, a scheme has been prepared which had received the hearty approval of the late Chancellor, Lord Goschen, before his death, and is supported by many high academical authorities and by a number of old Oxford men of the highest capacity and experience in many walks of life. An outline of this scheme will be found in the letter of the Chancellor and Vice-Chancellor. It includes provision for the promotion of modern studies, literary and scientific, such as modern languages, electrical research, the scientific basis of the training of practical engineers, the study of hygiene and of scientific agriculture, and, last but not least, for the adequate equipment and endowment of that greatest of Oxford's academical institutions, the ancient and world-renowned library of Thomas Bodley's foundation.—*The London Times.*

CURRENT NOTES ON LAND FORMS

NARROW COASTAL PLAINS

WELL-DEFINED land forms have an importance in systematic physiography that is not yet fully enough recognized by travellers. Hence all the more satisfaction is felt when an article gives so definite an account of such a feature as a narrow coastal plain that it can be easily appreciated by the reader. Such is the case in the 'Notes on the Raised Beaches of Taltal (Northern Chile),' by O. H. Evans (*Quart. Journ. Geol. Soc.,* LXIII., 1907, 64–68).

The coastal plain at Taltal has a gently inclined surface, fringing the coastal ranges and extending up the broader valleys to a considerable altitude and distance from the present shore. There is local variation in the width of the plain, and in the altitude of its inner border (200 feet, back of Taltal) along the base of the mountains. The surface of the plain is thinly covered with angular fragments from the hills; but where sections reveal its structure, it is seen to consist of stratified sand and gravel, containing recent shells which are sometimes plentiful enough to form distinct beds. Here and there the subjacent rocks rise through the plain in curiously weathered remnants of former islets and stacks. Evidence of intermittent uplift is found in several terraces, three of which are relatively well defined at altitudes of about 15, 80 and 200 feet above sea-level; two more obscure terraces are seen at intermediate heights. Where the mountains approach the sea and the plain narrows, the terraces are replaced by lines of boulders; at other points a rock shelf and again a series of shallow caverns marks the former shore line. Sudden uplifts are inferred from the well-preserved condition of the shells. Although no explicit statement is made as to the relation of the larger inland valleys to the plain, it may be inferred from certain phrases that the valley floors are now well opened somewhat below the plain surface. Regarding the smaller ravines of the old-land hills, it is said that their beds "suddenly alter in inclination and become precipitous as they approach the sea. Were streams suddenly to start running in these old gorges, they would terminate in waterfalls." Whether this sudden steepening is at the former or at the present shore line, does not clearly appear.

Another narrow coastal plain is described by W. D. Smith as forming an interrupted rim around the mountainous island of Cebu, and containing nine tenths of its large population ('Contributions to the Physiography of the Philippine Islands: I., Cebu Island,' *Philippine Journ. Sci.,* I., 1906, 1043–1059). The basis of the plain is of coral rock, over which alluvial deposits have been spread by the streams and rivers from the interior valleys.

Brief description of what appears to be a small and undissected coastal plain on which Sidon is situated on the Mediterranean border of Palestine, is given by Libbey and Hoskins in their account of a journey to 'The Jordan valley and Petra' (2 vols., New York, Putnams, 1905). Its low and well-watered surface has a 'carpet of green' in strong contrast to the gray foot-hills which rise from its inner border. A similar coastal or littoral plain extends southward from the hills by Beirut (see p. 66, frontispiece, and plate on p. 41).

I. B. AND W. M. D.

GLACIAL TROUGHS AND HANGING LATERAL VALLEYS

IN view of the ever-increasing volume of evidence to the effect that every glaciated mountain range in the world thus far studied shows a systematic association of peculiar features, such as valley-head cirques often holding rock-basin tarns, over-deepened main-valley troughs with floors of a considerable width, hanging lateral valleys, and Piedmont morainic amphitheaters, it is interesting to scrutinize the statements of certain geologists who still maintain that glaciers are ineffective eroding agents. Among these is W. Kilian, of Grenoble, whose studies have made him intimately familiar with many valleys of the French Alps, which most physiographers would regard as overdeepened by intense glacial erosion, but which Kilian explains otherwise ('L'érosion glaciare et la formation des terrasses,' *La Géogr.*, XIV., 1906, 261–274). These trough-like or broad U-shaped valleys, sharply incised beneath the gentler upper slopes ('paliers') which steepen somewhat in ascending to the peaks, he regards as the result of retrogressive torrential erosion during a late interglacial epoch, with slight modification by ice action during the last glacial epoch; the torrential action thus appealed to being itself explained as the result of an assumed change in the attitude of the land-mass with respect to base-level, and the hanging lateral valleys being accounted for as a result of the preponderance of erosion by the main stream.

It would certainly seem appropriate that those who adopt this hypothesis should, in the interests of thorough investigation, themselves be the first to give it an impartial test by looking to see if it applies in non-glaciated regions, where independent and acceptable evidence of change of base-level may be found, and where the disputed element of glacial erosion is ruled out of the case. Curiously enough, Kilian does not do this; nor does Heim, nor Garwood, nor Fairchild, who all, like Kilian, deny the efficiency of glacial erosion. It is true that reference is made by Kilian to recent articles by J. Brunhes, of Fribourg ('Sur les contradictions de l'érosion glaciare' and 'Sur une explication nouvelle de l'érosion glaciare,' *C. R. Acad. Sci.*, Paris, CXLII., 1906, 1234–1235; 1299–1301), to show that U-shaped valleys occur in non-glaciated districts; but Brunhes's articles are brief and general, and give little aid in solving the problem at issue.

Kilian himself states that U-shaped valleys may be produced by stream erosion in certain structures, and on this simple point there need be no dissent; but the peculiarity of glaciated valleys is that the U-shape prevails in all sorts of structures; thus indicating that glaciation and not structure is the determining factor. The Allegheny plateau, south of the glaciated area in the eastern United States, contains a good number of open U-shaped valleys, especially where relatively weak underlying strata are capped by stronger overlying strata; but all such open main valleys receive their lateral valleys at accordant grade, and thus differ most significantly from glaciated U-shaped valleys. It is singular that the non-glacialists do not themselves discover and accept this suggestive fact. As to the working hypothesis that retrogressive torrential erosion, excited by favorable change of base-level, may in time produce great trough-like valleys, certainly no one should, on *a priori* grounds, object to its due consideration; but this hypothesis normally requires the development of lateral valleys accordant with their main valley by the time that the main valley has gained an open floor; and as soon as the inquirer sees that the open Alpine troughs are constantly associated with discordant or hanging lateral valleys, it would seem to be incumbent upon him to set the working hypothesis aside as invalidated, and to look for another of greater competence.

Kilian and Brunhes both emphasize the importance of subglacial torrents, as Frech has done even more strongly ('Ueber das Antlitz der Tiroler Zentralalpen,' *Zft. Deut.-Oesterr. Alpenver.*, XXXIV., 1903, 1–31; see especially p. 22), in causing whatever erosion may have taken place during the glacial occupation of a valley; and it is to be presumed that no one would wish to minimize whatever aid may thus be given in a difficult problem; but it is inconsistent with all that is known of torrential action to think that, even under the constraint of subglacial flow, torrents can have accomplished the major part of the work that must be attributed to the general process of glacial erosion. Sharp-cleft gorges, such as that of the Aar in the rock-sill above Meiringen, and of various other Swiss rivers, may well be ascribed in large part to subglacial torrents; but the trough-like cross-section of a glaciated valley is not in the least what would be reasonably expected from the work of a high-pressure torrent. Indeed, the fact that the lip of many hanging valleys is so little trenched is strongly suggestive of the relative inefficiency of sub-glacial torrents; for precisely in such positions of sharply increased descent should the torrents have been most effective.

W. M. D.

HANGING VALLEYS IN ENGLISH LAKELAND

AMONG the recent essays which explain hanging lateral valleys otherwise than by glacial overdeepening of the main valley is one by J. E. Marr, of Cambridge, England, on 'The Influence of the Geological Structure of English Lakeland upon its Present Features' (presidential address, *Quart. Journ. Geol. Soc.*, London, LXII., 1906, lxvi–cxxviii; see p. cvii–). The greater depth of the open main valleys is here ascribed to normal erosion on weak structural features called 'shatter belts'; but under this explanation—glacial erosion being disregarded—it is difficult to understand why the lateral streams, which often mouth from 500 to 1,000 feet over an open main valley floor, have accomplished so little trenching of their hanging valleys during the long period in which the main valleys were well widened by the slow processes of general subaerial erosion. There are many cases in non-glaciated districts where wide longitudinal valleys on belts of weak rocks are joined by narrow lateral valleys whose small streams enter the main valley through belts of hard rocks; the Allegheny Mountains present hundreds of examples of this kind: but in practically all such cases, even when the contrast in resistance of the hard and weak rocks is strongly pronounced, the small lateral stream has been able to cut its narrow notch in the hard rocks down to accordant grade with the main valley floor that has been opened on the weaker rocks; for the widening of the main valley by general subaerial erosion has been a relatively long process even in its weak rocks. The streams in the hanging valleys of English Lakeland would then be exceptions to this rule, if their hanging position is not to be explained by the glacial overdeepening of the main valleys. A number of cases of stream capture and rearrangement are described by Marr in this connection; but as glacial erosion is entirely omitted from the problem, the explanation by normal stream action alone must remain in the same measure of doubt as that which now obscures the explanation of the rearrangement of various branches of the Rhine in the neighborhood of Chur, Switzerland, by normal stream action, as stated a score of years ago by Heim. W. M. D.

HANGING VALLEYS IN GENERAL

SINCE the convincing report on the glaciers of Alaska by Gilbert (Harriman Alaska Expedition, III., 1904), additional accounts of main-valley troughs and hanging lateral valleys, regarded as the result of glacial erosion, are given for the mountains of Alaska by R. S. Tarr ('Glacial Erosion in Alaska,' *Pop. Sci. Monthly*, LXX., 1907, 99–119), and by R. S. Tarr and L. Martin ('Glaciers and Glaciation of Yakutat Bay, Alaska,' *Bull. Amer. Geogr. Soc.*, XXXVIII., 1906, 145–167; see p. 159, figs. 17 and 18); for Norway by A. P. Brigham ('The Fiords of Norway,' *ibid.*, XXXVIII., 1906[1]); for the Tian Shan mountains by Friederichsen (see these 'Notes,' March 8, 1907); for the New Zealand Alps by E. C. Andrews ('Some interesting facts concerning the glaciation of southwestern New Zealand,' *Trans. Austral. Assoc. Adv. Sci.*, 1905, 189–205; good plates); for the Sierra Nevada by A. C. Lawson ('The Geomorphogeny of the Upper Kern Basin,' Bull. Dept. Geol., Univ. Calif., III., 1904, 291–376; see p. 329); and for the Sawatch range of the Colorado Rocky Mountains by L. G. Westgate ('The Twin Lakes Glaciated Area,' *Journ. Geol.*, XIII., 1905, 285–312) and by the undersigned ('Glaciation of the Sawatch Range, Colorado,' *Bull. Mus. Comp. Zool., Harv. Coll.*, XLIX., 1905, 1–11).

[1] The pages of this article are not cited here, because the reprint from which this reference is made has been repaged, and consequently affords no sufficient indication of its original place. If editors of scientific periodicals still persist in the troublesome habit of repaging reprints, it is to be hoped that authors and reviewers will protest against it.

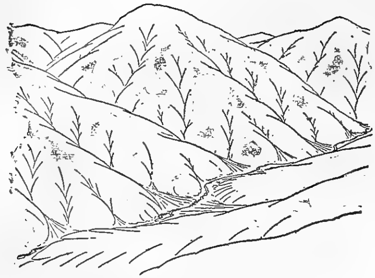


FIG. 1.—A normally eroded mountain mass, not affected by glacial erosion.

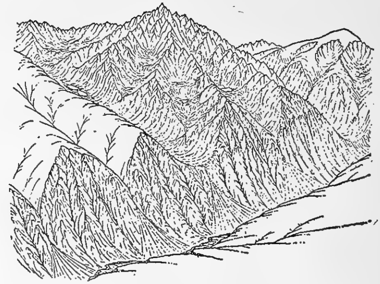


FIG. 3.—The same mountain mass as in Fig. 2, shortly after the glaciers have melted from its valleys.

The three diagrams here presented are reproduced from an article by the undersigned on 'The sculpture of mountains by glaciers' (*Scot. Geogr. Mag.*, XXII., 1906, 76–89), in which evidence for glacial erosion is found in a comparison of glaciated and non-glaciated mountains, entirely independent of whether glaciers are known to be capable of eroding or not. In view of the inaccessibility of the

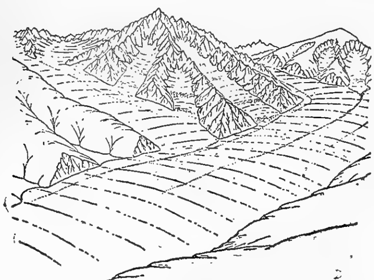


FIG. 2.—The same mountain mass as in Fig. 1, strongly affected by glaciers which still occupy its valleys.

bottom of a large Alpine glacier, it is believed that the best means of determining whether it acts as a sculpturing agent or not is to be found in a comparison of districts, otherwise similar, one of which has not been glaciated, while the other has been glaciated. The diagrams are not drawn from nature, although they summarize a variety of facts seen in various mountain ranges. The third one of the series may be taken as typical of La Plata peak, in the Sawatch range of Colorado, and of the overdeepened trough of Lake Creek beneath it, with a well-defined hanging lateral valley between the two.

It is of interest to note in this connection that a good explanation of hanging lateral valleys was given earlier than the date, 1898, usually assigned for this important advance in rational physiography; namely, in 1888, by La Noé and Margerie, in '*Les formes du terrain*' (Paris, *Service géograph. de l'armée*, p. 177), where the cause of the discordance of hanging lateral valleys over their trough-like main valleys is very clearly set forth.

W. M. D.

THE WISTAR INSTITUTE OF ANATOMY

The annual meeting of the advisory board of anatomists of the Wistar Institute was held on April 14 to 16. The members of the board present were Professors Barker, Donaldson, Gage, Huber, Huntington, Mall, McMurrich, Minot and Piersol; of the institute's staff, Drs. Greenman, Hatai, Stotsenburg and Streeter; of the institute's board of managers, Drs. Brown and Lewis.

The board held two sessions on Monday, April 15.

The general work of the year and the financial condition of the institute were explained by M. J. Greenman, the director.

The research in neurology was reviewed by Professor Henry H. Donaldson, chief of the neurological research of the institute. Professor Donaldson also reported upon the Vienna meeting of the International Brain Commission (May, 1906) and stated that an important result of this commission would be the organization of special institutes for the study of the central nervous system and the establishment of international relations for the advancement of this study.

From Professor Donaldson's statement of the research work it appears that the investigations now under way at the institute are as follows:

The development of the fiber tracts in the brain of the human embryo, together with some experimental work for the control of these observations (Streeter); biometric studies on the skull and nervous system of the albino rat as modified by age and nutrition, and the chemistry of nerve tissue (Hatai); the growth of the white rat before birth (Stotsenburg); the development of the heart and vessels of teleosts (Senior); regeneration of the peripheral nerves (Greenman); and a comparison of the growth of the nervous system and its parts in the albino rat and man (Donaldson).

The committee on further organization of the neurological workers of the country consisting of Donaldson, Piersol and LeConte reported progress.

Professor Gage, chairman of the committee on relations of the Wistar Institute to American anatomists, reported that they had issued a circular letter to the anatomists of the country, and that this had served the desired purpose of bringing the anatomists and the Wistar Institute into closer relations. The committee is to remain as a permanent one for further work of this kind.

As a central institute for brain research, the institute is desirous of securing for distribution exact information concerning the facilities for neurological work in this country. It was proposed to obtain this by means of both a circular letter and special inspection of laboratories, but the matter was left with the institute staff for final action.

Dr. Huntington, chairman of the committee on comparative anatomy and embryology, spoke encouragingly of the outlook for the future, and suggested that material for comparative anatomy be collected, with especial reference to the general subject of vascular morphology. Dr. Piersol urged in this connection the collection of human embryos.

The following general suggestions were made:

By Professor Barker, that the question of library be considered at an early date, and that the institute prepare a statement concerning the collections of anatomical literature in the city; secure the more important periodicals, and be prepared to purchase essential books of reference and new monographs, taking care not to duplicate books or journals, except where these were constantly needed in the building.

Professor Gage made the suggestion that the institute organize a bureau for information, bearing on the acquisition of materials, apparatus, models, photographs, etc., useful to anatomists. In this the institute would have the assistance of the board and others interested, and the work would be thus facilitated. The suggestion was recorded and will be carried out. Any assistance which anatomists or zoologists can render the institute in executing this plan will be gratefully received.

The publication of monographs was suggested by Professors Huber and Mall. It was the general opinion of the board that desirable monographs might be accepted by the board, published in the *American Journal of Anatomy* at the institute's expense, and as the edition was sold, the institute could then be reimbursed. This suggestion was so recorded, and will be carefully considered with reference to future action.

Professor Minot proposed that the institute take charge of the reproduction of original models and lantern slides. He stated that there were many original drawings which authors would entrust to the institute for the purpose of having lantern slides made from them, when they would not loan them to dealers. The reproduction of original models, as well as lantern slides, was favored by the board, and it was unanimously agreed that this was a most desirable undertaking. The suggestion did not imply that the institute should in any way act in a commercial capacity, but merely publish or reproduce models and lantern slides which can not be otherwise obtained. The cooperation of the members of the board was promised for this work, and it is expected that the institute will take it up soon.

BERMUDA BIOLOGICAL EXPEDITION

By an arrangement with the Bermuda Natural History Society, apportunity is offered for a limited number of instructors and research students in zoology and botany to spend a few weeks at the Bermuda Biological Station this summer.

Members of the expedition will sail from New York on the steamer Bermudian (Quebec Steamship Co.) at 11 A.M. on Wednesday, June 19, arriving in Bermuda June 21, and returning will sail on August 7. Those who can not sail on June 19, may do so two weeks later—July 3.

The expense will be $107 for first-class passage from New York to Bermuda and return, and for board and lodging at the Islands six weeks and five days. For the shorter time—four weeks and five days in Bermuda—the expense will be $90. Payments are to be made to the undersigned,—fifty dollars twenty days before sailing, the balance on arriving in Bermuda.

For further information apply to

E. L. Mark

109 Irving Street,
Cambridge, Mass.

MINUTES OF THE FIRST MEETING OF THE COMMITTEE ON SEISMOLOGY

The initial meeting of the committee on seismology of the American Association for the Advancement of Science, was called to order in the council room of the Cosmos Club, Washington, D. C., at 10 o'clock on April 19, 1907. There were present L. A. Bauer, W. W. Campbell, J. F. Hayford, W. H. Hobbs, A. C. Lawson, C. F. Marvin, W J McGee and H. F. Reid. The members of the committee unable to attend the meeting were C. E. Dutton, G. K. Gilbert, L. M. Hoskins, T. A. Jaggar, Otto Klotz, C. J. Rockwood, Jr., and R. S. Tarr.

Mr. H. F. Reid was made temporary chairman, and in the deliberations of the committee which continued throughout the day, the counsel was sought of the heads of the principal government and other bureaus likely to be instrumental in furthering the objects before the committee. O. H. Tittmann, superintendent of the U. S. Coast and Geodetic Survey; G. O. Smith, director of the U. S. Geological Survey; C. D. Walcott, secretary of the Smithsonian Institution, and R. S. Woodward, president of the Carnegie Institution of Washington, were all for a portion of the time in attendance upon the meeting and expressed their approval of the plans and purposes of the committee. Willis L. Moore, chief of the U. S. Weather Bureau, whose counsel was sought, could not be reached.[1] The following resolutions were adopted by the committee: (1) In the judgment of the committee its functions should be regarded as initiatory and advisory. (2) In the judgment of the committee the time has come for asking the support of the federal government in seismological work. (3) This seismological work requires a cooperation of the various scientific bureaus of the government. (4) The appropriations for seismological stations should be made through the U. S. Weather Bureau, and the results of the observations should appear in its publications. (5) A subcommittee of three, to include the chairman of the committee, should confer with the chief of the Weather Bureau, the superintendent of the Coast and Geodetic Survey, and the director of the Geological Survey with reference to framing the legislation providing for seismological stations and the publication of observations, as recommended in the preceding resolution.

A permanent organization of the committee was effected by the election of H. F. Reid, chairman, and William H. Hobbs, secretary.

The following subcommittees were named: (1) A committee to determine the best form or forms of seismograph for the seismological stations to be established—Messrs. Reid, Marvin and Bauer. (2) A committee of three members with power to add to its number, to report as to what action is deemed desirable

[1] Chief Moore later expressed his hearty approval of the committee's plans.

in reference to recommendations concerning earthquake-proof construction—Messrs. Lawson, McGee and Campbell. (3) A committee for carrying out resolution 5—Messrs. Reid, Marvin and Hayford.

The committee then adjourned to reassemble during the meeting of the American Association in December next, when the association will be asked to make a grant for defraying the necessary expenses of the committee.

William H. Hobbs,
Secretary

SCIENTIFIC NOTES AND NEWS

Sir William Ramsay has been elected an honorary member of the Academy of Sciences of Christiania.

The degree of doctor of science will be conferred by Cambridge University on Sir Clements Robert Markham, Colonel Sir Thomas Hungerford Holdich and Sir Thomas Richard Fraser, professor of materia medica and of clinical medicine in the University of Edinburgh.

The Royal Institution has awarded the Actonian prize of one hundred guineas to Madame Curie.

M. Henri Poincaré, professor of mathematical astronomy at the University of Paris, has been appointed a member of the Council of the Teaching of Fine Arts in the room of the late M. Berthelot.

Dr. Frank Billings, dean of the Rush Medical College, University of Chicago, has been elected president of the National Association for the Study and Prevention of Tuberculosis.

Dr. George A. Piersol, professor of anatomy at the University of Pennsylvania, was on May 2 elected president of the Pennsylvania chapter of the Society of Sigma Xi.

At the annual banquet of the Alumni Association of the Philadelphia College of Pharmacy, held on May 14, there was presented to the college an oil portrait of Professor Joseph P. Remington, given in honor of his thirty-fifth anniversary as member of the faculty.

Dr. L. R. Abrams, of Stanford University, has been granted a leave of absence to enable him to review the manuscripts of a series of volumes in course of preparation by the Cree Publishing Company, giving a popular account of the plant-breeding work of Mr. Luther Burbank. Dr. Abrams moved to Santa Rosa, Cal., and assumed his new duties on May 1.

Dr. Edward Caird, the master of Balliol College, Oxford, has been compelled by ill health to resign the mastership, to which he was elected in 1893, in succession to Professor Jowett, having been formerly fellow of Merton College and professor of moral philosophy in the University of Glasgow.

Charles C. Adams, of the University of Cincinnati, has resigned the directorship of the museum of the Cincinnati Society of Natural History.

Plans have recently been perfected for a detailed and systematic investigation of the Atlantic and Gulf Coastal Plain stratigraphy and paleontology, several State Surveys, including North Carolina, Georgia, Alabama and Mississippi acting in cooperation with the United States Geological Survey in the studies. The aim of the work is to determine the extent of the subdivisions recognized in New Jersey and Maryland on the north and Alabama on the south, to determine their relations to one another, and in general to establish satisfactory correlations throughout the district between the Potomac and the Mississippi River. Economic studies, especially on the phosphates, will also be made incidentally. The general supervision of the work rests with a board of supervising geologists, consisting of the state geologists in the Coastal Plain districts and the chief geologist and chief hydrographer of the national survey, Dr. W. B. Clark being chairman. The field work is in charge of Mr. M. L. Fuller, who will put seven parties into the field the coming summer. It is hoped to complete the investigation in Virginia, North Carolina, South Carolina and Florida during the next year, while the work in the remaining states will be finished in 1908 and 1909.

The medical corps of the navy has not been able to get enough surgeons to meet the needs of the service. Admiral Rixey, the surgeon general, has undertaken to give temporary appointments as 'acting assistant surgeons' to the young men who will pass a satisfactory preliminary examination and come to Washington for instruction. They will receive six months' special training at the Naval Medical School and Hospital, or at the Mare Island Naval Hospital. At the end of that course they will receive appointments as assistant surgeons, with an annual salary of $1,760, supplemented by allowances of $432 and mileage. The shortage of doctors in the navy has become really serious. There are sixty-four vacancies in a corps that at its maximum should number only 350.

At its meeting of May 30, the Wisconsin Natural History Society will celebrate the fiftieth anniversary of its foundation. Mr. Charles H. Doerflinger will give a brief sketch of the founding of the society, and Professor E. A. Birge, of the University of Wisconsin, will speak on 'Science and the People.'

UNIVERSITY AND EDUCATIONAL NEWS

An alumnus of the College of the City of New York, who prefers to withhold his name has given $10,000 to the institution.

Columbia University has received a gift of $1,000 from Mr. Charles S. Bartow, towards a mathematical laboratory and museum, and $500 from an anonymous donor for researches in anthropology.

Dr. Benjamin Ide Wheeler, since 1899 president of the University of California and previously professor of Greek at Cornell University, has been offered the presidency of the Massachusetts Institute of Technology by the subcommittee of the corporation. The offer must be confirmed by the corporation, and it is not certain that President Wheeler will accept.

Dr. John Scholte Nollen, head of the German department at Indiana University, has been called to the presidency of the Lake Forest University, to succeed Dr. Richard Harlan, who resigned last autumn.

Frank P. McKibben, associate professor of civil engineering at the Massachusetts Institute of Technology, has been appointed professor of civil engineering at Lehigh University, in charge of the department, in place of Professor Mansfield Merriman, who has resigned after a record of twenty-eight years' service.

Dr. J. Bishop Tingle, who during the past three years has been assistant in charge of the Laboratory of Organic Chemistry in the Johns Hopkins University, and assistant editor of the *American Chemical Journal,* has been appointed professor of chemistry at McMaster University, Toronto, Canada.

Professor Frederic E. Clements, of the chair of plant physiology in the University of Nebraska has accepted a call to the professorship of botany in the University of Minnesota. He will sever his connection with the University of Nebraska, in which he has been a teacher for thirteen years, in time to enable him to assume the duties of his new position at the opening of the fall semester. He continues the present summer his studies of Rocky Mountain Botany at the Pike's Peak Laboratory near Manitou, and in addition is to make more extended studies in other parts of Colorado. As heretofore, he is to be accompanied by a party of advanced students.

Dr. G. F. Ruediger, until recently of the Memorial Institute for Infectious Diseases, Chicago, has been appointed professor of pathology and bacteriology in the University of North Dakota. The position includes the directorship of the state health laboratory.

Dr. Elias Potter Lyon was unanimously elected dean of the Medical Department of the St. Louis University at the May meeting of the board of directors. Dr. Lyon received his bachelor's degree at Hillsdale College in 1892; the doctorate in philosophy at the University of Chicago in 1897. He was instructor in biology at the Bradley Institute, Peoria, assistant professor of physiology at the Rush Medical College and finally dean of medical work at the Chicago University before taking charge of the Department of Physiology at the St. Louis University in 1904.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, MAY 31, 1907

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THE INTERNATIONAL AERONAUTICAL CONFERENCE AT MILAN

THE history and organization of the International Commission for Scientific Aeronautics, whose name does not indicate that its purpose is to explore the atmosphere, are briefly described in SCIENCE, Vol. XXI., page 461. The fifth meeting of the commission had been appointed for Rome in 1906, but on account of the exposition at Milan, with its aeronautical section, the place of meeting was changed to the latter city. The conference began on October 1 and lasted through the sixth, there being about forty members of the commission and guests in attendance. The proceedings were opened by Professor Celoria, representing the exposition of Milan, and a further welcome was extended by Signor Gavazzi on the part of the municipality, by Professor Palazzo for the Italian government and by Professor Hergesell as president of the commission. Two presiding officers for each session were chosen from among the foreigners present, who were chiefly Germans. England, however, was unusually well represented by four delegates and guests. The writer was the official representative of the United States Weather Bureau, as well as of the Blue Hill Observatory, and on his proposition Dr. O. L. Fassig, research director at the new Weather Bureau observatory on Mount Weather, Virginia, was elected a member of the commission, as were also M. Lancaster to represent Belgium and Signori Gamba and Oddone from Italy.

Professor Hergesell reported on the progress of the work which the commission furthers, since its meeting at St. Petersburg in 1904. In Spain unique observations had been obtained with balloons during the total solar eclipse of August 30, 1905; two expeditions had been sent from France by Messrs. Teisserenc de Bort and Rotch to explore the atmosphere above the tropical Atlantic; in Italy manned and registration balloons at Rome, Pavia and Castelfranco had contributed data, while kites had been employed in the vicinity of Monte Rosa; in Russia the observatory at Pawlowsk was making aerial soundings and other stations were being equipped for this purpose; in Switzerland Dr. Maurer had compared the data on mountains with those in balloons and in Austria numerous scientific balloon ascensions had taken place. In Great Britain and India kite flights were being made and in the United States the government Weather Bureau had joined the Blue Hill Observatory in making kite flights on the term-days. Germany was very active: there were daily observations in the free air at Lindenberg and Hamburg, and in Munich Baron von Bassus and Professor Ebert were experimenting with balloons; the money for a floating observatory on Lake Constance was assured, so that ascents of balloons or kites would eventually be made from a fast steamer; the German Marine had sent a surveying ship, equipped also with apparatus for exploring the air, into the tropics, and the Prince of Monaco, with the cooperation of the speaker, had executed such explorations over the Mediterranean, and over the tropical Atlantic and Arctic oceans. Belgium was now participating in the despatch of *ballons-sondes* and Roumania had promised to cooperate. The cost of publishing these observations executed in the free air, amounting to about 12,000 francs a year, is defrayed by the countries which collect them. General Rykatchef, in reporting on the resolutions adopted at St. Petersburg, stated that it had not been possible to secure the free entry into the different countries of the balloons and instruments which were used in the experiments.

The topics discussed in the subsequent sessions related to the methods of investigation or the results obtained and a summary of the most important follows. Dr. Erk, of Munich, advocated balloon ascensions in the neighborhood of the Alps in order to study local phenomena, such as the föhn wind. Professor Ebert indicated the methods which he employed to determine the deformation of equipotential electrical surfaces around a balloon and showed a new apparatus to measure atmospheric ionization.

The use of small balloons to determine the currents in the high atmosphere was discussed by Dr. de Quervain and others. If a barometer is carried by the balloon from its trace and from the measured angles of the balloon the course can be plotted. A small balloon may be observed with a telescope to a height of ten or twelve kilometers and Professor Hergesell was able in the clear air of Spitzbergen to follow a rubber balloon, which expanded to one and a half meters in diameter, during seventy-four minutes, at the end of this time the balloon being eighty kilometers distant. Micrometric measurements of its diameter showed the velocity of ascent to be nearly constant, since the loss of gas is slight, so that the height when it enters the different currents may be calculated from a single station, even if the balloon carries no barometer or is not recovered. A mechanical triangulating device has been used by De Quervain for finding the height of the balloon, but this is similar to the apparatus which Mr. Clayton devised for getting the height of clouds at Blue Hill. Colonel Vives y Vich recommended sending up paper pilot balloons simultaneously with the *ballons-sondes* in order to see how the wind changed in the isothermal zone. Baron von Bassus exhibited an apparatus for reading the curves of the self-recording instruments and Dr. de Quervain discussed the thermal inertia of the different thermometers, concluding that the metallic bar of Hergesell was more sensitive than that of Teisserenc de Bort. An interesting discussion followed as to the relative value of observations obtained with kites and balloons, General Rykatchef, Professor Berson and others favoring the former and Professor Hergesell alone championing the latter method.

General Rykatchef, for Mr. Kouznetzof, explained a method that had been employed at Pawlowsk to ascertain the height of clouds at night by projecting a searchlight upon them and measuring the vertical angle of the spot of light, which elicited the information that the same method had been tried in France, at Hamburg and at Blue Hill. Captain Scheimpflug showed how photographs of the ground taken from a balloon could be rectified so as to be transformed into topographical plans.

A number of communications giving the results of observations in the free air were presented. General Rykatchef stated deductions concerning the vertical gradient of temperature in the free air at Pawlowsk, which is greatest near the ground and during the month of June and least in December. Another paper by Dr. Rosenthal discussed the diurnal range of temperature at different heights over the sea. While in the first 100 meters there is a fall of 1° C. in the day and 0.2° at night, in the stratum between 300 and 400 meters the decrease is 0.6° during both day and night.

Mr. Rotch gave the results of the first *ballons-sondes* in America, fifty-three of the fifty-six balloons which he had despatched from St. Louis in 1904–'06, having been recovered. One of the lowest temperatures ever observed (— 79° C.) was recorded in January at a height of only 14,800 meters, and the isothermal, or relatively warm current, which had been found in Europe, was shown to exist at a greater height in the United States. Dr. de Quervain presented proofs of this isothermal stratum above 12,000 meters, which had been furnished by ascents of balloons in the daytime. Professor Hergesell related some experiments which he had made to measure the vertical movement of the atmosphere by getting the difference between the calculated rate of ascent of the balloon and the vertical movement of the air recorded, amounting in one case to a downward current of half a meter per second. Professor Berson offered two papers, one being a discussion of more than a thousand kite flights at Lindenberg, in order to ascertain the variation of wind-velocity with height, the author concluding that the velocity increases faster than the density of the air decreases. The other paper discussed the data from sixteen *ballons-sondes*, sent up from Milan the previous summer, nine of which could be followed in the telescope to a distance of eighty kilometers. Very low temperatures were recorded, and — 64° C. at 12,000 meters corresponded to a change of 100° C., from sea level, or nearly the adiabatic rate. Mr. Dines showed views of the kite windlass used by Mr. Cave and gave an example of a large inversion of temperature observed in England up to 2,000 meters.

The most interesting communications related to the exploration of the atmosphere over the ocean during the preceding year. M. Teisserenc de Bort gave the results of the last cruise of his steam-yacht *Otaria*, which had been sent across the equator by

Mr. Rotch and himself. Thirty-nine pilot balloons were launched and twenty-two balloons with instruments, of which seven were lost. A captive balloon ascended to 7,500 meters and kites were used in the lower strata. The existence on the open ocean of the southwest anti-trade above the northeast trade, and of the northwest anti-trade above the southeast trade, was demonstrated and it was shown for the first time that the temperature high above the thermal equator is lower than it is at the same height in temperate regions, owing to the absence of isothermal strata. Professor Hergesell gave a brief account of the cruise which he had made to Spitzbergen on the Prince of Monaco's steam-yacht *Princesse-Alice.* Owing to fog and cloud no lofty observations were obtained, but a slow decrease of temperature and a rapid increase of wind with height were indicated. Professor Hergesell explained his method of releasing one of the tandem balloons at a given height, so that the other balloon with the instrument would soon drop and be recovered, even in cloudy weather. It was suggested that the balloon might be liberated also by electrical waves. The same speaker and Professor Köppen described the survey steamer *Planet* of the German Marine, which is making soundings of both the water and the air in the South Seas. The thanks of the commission were voted to the German Minister of Marine, to the Prince of Monaco and to Messrs. Teisserenc de Bort and Rotch for their researches over the oceans.

M. Teisserenc de Bort submitted a memoir on the necessity of extending the territory for the international ascensions. In Europe almost all the stations are grouped within an area having less than a thousand kilometers radius, and there are none to the north and southeast. It is necessary to get data from a point to the north of the Scandinavian peninsula and also to the north of Great Britain. It would be interesting to have one station near the center of the Mediterranean, such as the Etna Observatory at an elevation of 3,000 meters. In Algeria it is proposed to launch pilot balloons and to measure their angles, and in Cairo, where there is a well-organized meteorological service, it is probable that observations can be obtained with kites and pilot balloons and possibly with *ballons-sondes.* In the United States we have observations, due to Mr. Rotch, at Blue Hill and at St. Louis and an aerial observatory has been established by the government on Mount Weather in Virginia. The most important place is Newfoundland, where *ballons-sondes* could be launched, even during storms, as the writer, M. Teisserenc de Bort, had done with success in the more restricted region of Denmark. In order to bridge the gap over the ocean, as much as possible, it is proposed to request the Canadian meteorological service to make ascensions with pilot balloons at Bermuda; to have this done at the Azores, and to secure the cooperation of the Jamaica and Havana observatories. In Mexico *ballons-sondes* might be used and the system thus developed will permit the general circulation to be determined at different heights around two or three of the most important centers of action in the atmosphere.

At the close of the meeting eleven resolutions were voted, chief of which were the following: The commission, on the recommendation of M. Teisserenc de Bort, realizing the great importance of collecting sufficient observations to chart the meteorological elements at various heights under different atmospheric conditions, believes that its efforts should be concentrated upon four groups of ascensions during the year, called 'grand international ascensions,' in order to distinguish them from the monthly ascensions. These last are optional for stations which do not make aerial soundings their chief work. The quarterly ascensions will be made during three consecutive days, on dates to be named hereafter. It is recommended that the trajectories of the *ballons-sondes,* and of the pilot balloons, when only these are used, should be determined by angular measurements and that the same thing be done for clouds. It is also desirable, as General Rykatchef has suggested, to have at least one temporary station for these international observations in the midst of the great Asiatic anti-cyclone, especially in winter. If this can be established the observations should last seven days instead of three days, that is to say, two days before and two days after the normal days.

A subcommission consisting of Messrs. Teisserenc de Bort, Berson, Hergesell, Köppen, De Quervain and Rotch decided to adopt Professor Köppen's proposition to publish a compendium of the best methods of sounding the atmosphere, for which the several establishments actually conducting such investigations will be consulted and the publication made by the International Commission. The subcommission also recommended that a form of publication, similar to that used by the Deutsche Seewarte, be adopted for statistics relating to the kite flights and that a similar résumé for balloon ascensions be used by the institutions participating in them.

The commission expressed its satisfaction that atmospheric soundings had been begun by the United States Weather Bureau on Mount Weather and hoped that they might be extended to other stations of the service.

The conference agreed with Major Moedebeck that it would be useful for scientific as well as for ordinary balloon ascensions, if, on the topographic maps of the various states there should be indicated in red the location of collections of lights which could serve to orient the aeronaut at night, and if the lines of high electrical potential, and also the places which were sheltered from wind, should be marked on the maps.

The propositions of Professor Assmann, relative to the meetings, were adopted in this modified form: The commission shall meet but once in three years, unless there is special reason for assembling earlier. The reunions are intended to consider the organization of the work and to discuss methods and instruments, scientific communications being relegated to the last and only presented then if time allows.

It was the sense of the meeting that the entertainments in honor of the commission should be restricted henceforth and at the present convention they had been mostly combined with technical demonstrations of aeronautical apparatus in the exposition and elsewhere. Thus, on one excursion to Pavia the aero-dynamical observatory of Signor Gamba was inspected. Afterwards the university was visited and a lunch tendered by the municipality. On another excursion to Lake Maggiore, through the courtesy of Signor Mangili, president of the exposition committee, experiments in flying kites and liberating *ballons-sondes* from a steamboat, were attempted, although without much success. After the close of the meeting members of the congress had the opportunity of making balloon ascensions, under ideal conditions of weather, in eight balloons which rose from the exposition grounds and landed not far from Milan, a few hours later.

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THE DANGER OF OVERSPECIALIZATION[1]

In the ever-recurring discussion of our

[1] Read before the meeting of April 5, 1907, of the New York Section of the American Chemical Society.

methods of education and of our pursuits in life, it has been asserted over and over again that 'this is the age of specialists.'

Is this really so? Does this age create more specialists than former epochs in history? If this be the case, does this tendency keep on increasing, and if so, may it not ultimately impede the higher development of mankind and reduce us to mere automatic machines?

The political economist tells us that division of labor increases and improves production. We also know that in any pursuit of life specialization enables us to more thoroughly master the details of a subject.

On the other hand, we ought to admit that even to-day, truly great men, who have achieved distinction by exercising a beneficial influence on the development of our race, were not merely specialists. They were persons of broad general tendencies, although sometimes their superiority was more accidentally manifested in some special line of work. If, to stave this assertion, I started to mention a list of names, I might possibly omit many men of merit known or preferred more particularly by any of you. But I shall take the liberty of turning my argument, by challenging you to name any truly great man who was merely a specialist in one single small branch of human activity.

That one-sided pursuits are apt to make us very narrow-minded will be conceded by many whose misfortune it has been to have to work or live with people who led such a specialized existence. Even the professional fields of specialized activity may lead to short-sighted pettiness. Andrew Carnegie reenforces my own belief, based on personal experience, when comparing the better class of business men to artists, he wrote: "I have learned that the artistic career is most narrowing, and produces such petty jealousies, unbounded vanities and spitefulness, as to furnish me with a great contrast to that which I have found in men of affairs. Music, painting, sculpture, one would think, should prove most powerful in their beneficent effects upon those who labor with them as their daily vocation. Experience, however, is against this."[2]

This apparent shortcoming of artists may be explained by the fact that as a class they are generally very ignorant outside of their own art, which requires more skill than knowledge, and what is worse, to many of them exact knowledge, which might help to broaden their views, is almost repulsive.

But—to come back to first principles—we ought to consider all pursuits of life from a broad general standpoint. I dare say that human life includes, as its noblest attributes, three fundamental tendencies to which all others converge, directly or indirectly. Indeed, nature prompts us—

First, to develop ourselves physically and intellectually—the latter word including all moral development.

Second, to reproduce ourselves and lend our short existence as individuals for the physical and mental betterment of our race, towards a higher goal of absolute good.

Third, to enjoy life in its material and intellectual comforts as far as the latter contribute directly or indirectly to the two first-named functions. This idea includes naturally the production of wealth and the better use of the same.

Whether we be scientists, philosophers, laborers, artists, merchants, money lenders, beggars or thieves, we all obey those laws which predetermine the ultimate destiny of mankind. Whoever, in some way or another, works in harmony with these dictates

[2] 'The Empire of Business,' Andrew Carnegie.

can not help being a benefactor to his own race and to himself in particular.

No individual whatever can try to divest himself of any of the above-mentioned attributes or functions of life, without jeopardizing in some degree or another the progress of his race.

Let us imagine, for an instant, a society only composed of four distinct classes: one, wealth-producing but vicious; a second, very moral but inactive; a third, very intelligent but bodily weak; while a fourth class would be composed of physical athletes, very stupid. Fortunately for the welfare of our race, in such a heterogeneous society, free intermarriage would tend to offset the one-sided grouping of these abnormal individuals or specialists—to call them by another name—and would bring about more homogeneity for their descendants. Furthermore, education—the great leveler of one-sided tendencies—would prove another active factor for accomplishing this result. When I speak here of education, in its broadest sense, I do not merely limit myself to so-called school education; but I include in this term all influence towards mental development, proceeding from any source whatsoever, as, for instance, self-culture and environment. But, whenever education tends to develop one of our faculties beyond reasonable necessity and to the detriment of other functions, we drift towards mental deformity—a freak; and this in about the same manner as a particular muscle exercised beyond normal requirements will leave the remainder of the body insufficiently developed and out of harmony with the other anatomical parts.

Many fundamental errors have been committed by such men as spoke or wrote of the culture or the civilization of a given nation, without according full importance to above considerations. Moreover, history teaches us that a one-sided or specialized education has been the defect of nations of the past, even to a greater extent than it occurs in our present civilization. Many examples can we give to show that such a one-sided culture was largely to blame for the downfall of at one time powerful races. The Greeks, in their overspecialization of art, neglected beyond measure the study of nature. Had her philosophers steadied their thoughts by giving more attention to the careful observation of natural phenomena, instead of dreamingly searching a solution for all problems by analytical reasoning, they would not have been led astray into casuistry and sophistry and skepticism. The great laws of nature might have opened their minds to a better understanding of equity and rights of man; while now history has to record that even the most progressive and radical Greek philosophers proclaimed chattel slavery as an indispensable institution of society. In the same way, a large part of their literature was devoted to beautifully sounding, well polished sentences, dealing mostly with imagination. The Greek writers, in their search for effect, gave their fancy full play whenever they described the war exploits of their heroes, with the result that they put themselves on record in history as the biggest braggarts in prose and in rhyme.

As another example of one-sided culture let me remind you of the inhabitants of India, who became overawed by the conception of the immensity of the universe, and the relative insignificant smallness of man. Exaggerating this feeling, they too failed to grasp the full meaning of harmony in nature and so neglected to give sufficient attention to the material development of their race. This doomed to stagnation an otherwise very intellectual people; it rendered possible their subjugation under the strong and forceful arm of warlike tribes, morally less developed, but physically better adapted for the perpetuation and multiplication of their sturdier race.

In the same way, the Roman empire fell as a result of the wilful ignorance of the true principles of equity; her poorer classes, or vanquished foes, were denied their natural rights by their aristocratic masters. This heterogeneity of the people led to all the excesses which brought about the fall of what once had been a mighty empire.

After the advent of Christianity, the despotic Church of Rome retarded the progress of all Christendom as soon as she tried to specialize all human knowledge, so as to make it agree with her own bible. The result was that long and sad period of the dark middle ages. In the meantime, Saracen and Jew, on the north coast of Africa, or on the Iberian Peninsula, were able to cultivate science less trammeled by a restrictive religion. To their broader activity do we owe it that scientific investigation was kept alive until the day when the dawn of Reformation enabled backward Christendom to resume again the search for truth.

But, even now, our educational system is still much under the chilling effect of that cloud which during the middle ages hid the light of true knowledge. Respectable pedagogues have taken care to hand us down from generation to generation a curriculum which includes most of what formerly was erroneously called *a complete education*. In the latter, ancient literature, holy or profane, has always played a paramount importance. In its program, scant consideration is given to more real modern knowledge which refers broadly to the world we live in, or to the burning questions of the day. I know of many instances where, under the name of liberal education, such an antiquated tuition is still dealt out to the younger generation of both sexes. In fact, many well-meaning persons think that this is the kind of respectable training most desirable for a rich young man of good family and good manners. In reality, such an education is merely an overspecialization of the kind of culture which was meted out to studious sons of patricians some two thousand years ago. For our modern requirements, it is an anachronism, if not a positive danger: a danger—because it is liable to select as standards the undeveloped or erroneous thoughts of antiquity. In many instances, the tendencies of the ancients clash with our more advanced ideas of truth and justice—even if the latter are not always consistently practised by modern society.

The French Revolution encouraged some reforms in this antiquated system of education. But even to-day modern science and modern thought are grudgingly allowed a very small place in the classic curriculum so faithfully defended by some pedagogues. Small wonder, then, if we hear so many respectable people use flowery rhetoric on such inconsistent themes as 'science versus religion,' or, 'science versus art'; as if there were any *versus* possible whenever we speak of science as true science, religion as true religion and art as true art—as if truth were different, whether expressed scientifically, religiously or artistically!

Luckily for the progress of humanity, now and then some young men, less blessed with worldly goods or wealthy parents, and more eager to make a living by their own work and education, have been compelled to give a vigorous kick to the classic curriculum fetish. Some of them decided to take their education 'à la carte'—as President Eliot expressed it so picturesquely. They were compelled to select substantial and up-to-date meals, more suitable to their eager modern appetites; they had to shun the stale and indigestible dishes of education made up in antiquity, to please the palates of bygone times.

Unfortunately, the very cause of this new tendency has carried us so far as to seriously threaten us with the pitfalls of the other extreme.

Furthermore, the fact that scientific learning has found unceasing applications in the production of wealth has fostered the constantly increasing tendency for finding in scientific education or scientific pursuits a mere means of earning a living or making money. This has led into scientific vocations a large number of persons who in their profession expected to find a quick and easier means for making a living, but outside of this stimulant possessed few if any of the qualifications of the true scientist. Impractical in their selection, they deceived themselves by choosing an occupation which less than any other leads to wealth or power. But once having decided to enter a scientific profession, they soon became aware that the call is for specialists, and they were forced to specialize one thing or another by their employers, whether the latter were manufacturers, merchants or even some educational institutions whom they served as teachers.

In accordance with this same tendency we find manufacturers who, themselves without other training excepting what long experience has taught them, blame our colleges, universities or technical schools because they do not turn out graduate chemists who can jump right away into their manufacturing works fully acquainted with all the intricacies of the processes; of course, all this with the prospect of a small salary so as to act in competition with a few able and better paid men who had to sacrifice a lifetime in order to acquire experience.

That greed or ignorance should make such claims is quite natural; but that we should find teachers or students who are willing to admit such abnormal educational methods, and change them into a kind of apprenticeship, is a matter of regret for anybody who believes in education as a means for the healthy mental development of his country.

What is worse, our own way of living shows beyond doubt that we all have undergone, more or less, the effects of overspecialization against which I have come to protest. Too much have we learned to look upon our usefulness in life as depending almost exclusively on the concentration of most of our energies, most of our thoughts, upon a narrow line of specialized action. Without knowing it we drift into a mere routine occupation that makes automatic machines out of us. For all generalities of life which do not fall immediately into our own specialties, we are willing to assume respectable conventionality: We are willing to join the herd of docile and unthinking sheep who are following a leader. In science as well as in politics, we are ready to follow this leader, for better or for worse, as long as we can shift upon him our own responsibilities of thought or action.

Busily burrowing along like moles, in the pursuit of our own little specialties, we are dizzily preoccupied with our specialized routine work. We lose the desire of coming once in a while upon the surface of the earth to take a stimulating look at the grand view of nature and its inspiring entity. Once upon a while, we are rather disturbed in our narrow scientific beliefs when some Curie announces radium or radioactivity, or when some Ramsay upsets our orthodoxy by pronouncing the words: evolution of elements. We get fairly shocked when a Crookes speaks of death of matter.

Just in the same way, after admitting as holy faith that weight and matter are constant or indestructible, some day, somebody may have to rouse the most timid of us and force us into the belief that gravitation, like all other energies, can be modified into any of them, or better perhaps, that gravitation, being the more stable of all energies, is the final energy toward which light, health and electricity tend to change. Who knows but that ultimately a less neglected study of gravitation may allow us a glimpse into the secret of the destiny of our universe?

I admit, many of you will smile at these unorthodox hypotheses or conjectures. Yet, let me ask you: With what methods have we thus far measured any possible changes in weight? We have pinned all our faith, all our beliefs, on a mechanical instrument called a balance. A very delicate method indeed, if judged from our conceited one-sided standpoint of specialists. We are proud if we possess a balance which can weigh a one hundredth of one milligram; we work ourselves into awe and admiration before an instrument such as the one I saw two years ago which can detect a difference of a one thousandth of a milligram. A one thousandth of a milligram! How infinitesimally small such a weight appears to our limited conceptions; and yet, what a ponderous quantity this same weight becomes if we try to compare it with the mass of an electron. Our whole science of chemistry is based on the fundamental law of the conservation of matter as formulated by Lavoisier and accepted by us as an axiom. However, by what means has this law been verified, if not by balances more crude, more imperfect, than that clumsy instrument which can not weigh anything beyond 1/1,000 of a milligram? It is high time that science should discover a more delicate means for determining small weights than a mere mechanical balance; then, but only then, may we be able to demonstrate beyond doubt whether all the assumptions on which we base our chemistry are correct, or whether we simply have been building a whole science on false premises.

While we are at this subject let us continue this act of self-examination. When we speak of the descriptive part of the science of chemistry, when we describe any reactions, any compounds, any laws, we all refer these to phenomena which take place within an abnormally small range of temperature. Lately, Dewar opened our eyes to some unexpected phenomena which occur at very low temperatures; on the other hand, the electric furnace so ably manipulated by our regretted Moissan enabled him to establish many unsuspected facts at temperatures which our imperfect thermometric methods do not allow us to measure accurately. Yet, if we will drop for a moment our one-sided considerations and look upon everything in true proportions, we must admit that the range of temperatures within which we have studied natural phenomena is disappointingly small, as compared with the possible range of temperature of the universe.

Not so long ago, chemists had no better definition for organic compounds than to designate them as those that were produced under the intervention of vital forces; inorganic bodies, on the contrary, were supposed to be made under the influence of ordinary physical forces. We all know since how Liebig and Wöhler disposed of this mistake by the memorable discovery of the synthesis of urea from inorganic bodies. Nevertheless, many of us to-day are prone to think that the more delicate organic bodies, as, for instance, the constitments of the protoplasm, will never be obtained synthetically. These doubters point to the fact that as soon as we try to imitate these subtle, synthetic reactions which take place in the living cell we remain powerless to accomplish anything beyond splitting or simplifying the molecule. And yet, let me ask you, what are the laboratory methods with which we try to imitate the subtle biological processes? Heating, boiling, distilling, desiccation, precipitation, electric currents, every one of them barbarously destructive methods, with which we blast away at exceedingly delicate compounds: We might just as well try to imitate the melodious music of a Gounod by firing some dynamite cartridges between the delicate strings of a piano!

One-sided as we are, we witness every day of our lives the fact that all vegetation accomplishes its processes of synthesis or assimilation under the indispensable action of light; nevertheless, thus far we have tried very little to avail ourselves of this powerful yet delicate source of synthetic energy. Up till now photochemistry has scarcely been used for any other purposes but the art of photography.

What have we done to utilize the effect of pressure in the study of natural phenomena? Very little, even if we take in consideration some half-hearted attempts in this direction. What are the pressures we dispose of as compared with those which exist in the center of the earth? We hear of mines about one mile deep of which the tunnels are submitted already to such a tremendous natural pressure that their walls snap together shortly after an excavation is made, leaving the miner barely time to get out, so as to save his life. If we calculate the pressures existing at these depths we come to very awe-inspiring figures. But if again, we invoke the sense of proportions, we must recognize that a mine one mile deep is a mere insignificant and imperceptible pin prick as compared to the size of the earth. After such considerations, can we expect to duplicate certain chemical or physical processes which have been going on under tremendous pressures in the bosom of the earth? Or shall we try to find means to enormously increase the pressures of which we have thus far disposed in our laboratories, and have considered sufficient, although they are absurdly small.

And how about the element of time in chemical reactions? We all now are aware of the fact that even an explosion of dynamite takes an appreciable and measurable time. On the other hand, Berthelot, in his memorable studies on estherification, has demonstrated that in some cases it requires sixteen years of continuous action before the limit of estherification is reached and a final equilibrium is maintained. We are not inclined to patiently study reactions which take months or years, and yet, in the great laboratory of nature, phenomena are accomplished just the same whether their fulfillment requires seconds or æons. But in our lives, which are of such an infinitesimal shortness if compared with eternity, we look at everything according to the very short lapse of time which is allotted to our little individual existence. We refer and compare everything to it, in about the same way as I suppose the mayfly does to her own little life, after she has become accustomed to the fact that her existence is counted only by a few hours.

I am perfectly aware that these and many other philosophical conceptions are receiving consideration from such broad-minded scientists as have not grown up to consider science as divided in water-tight compartments. For them the borderland between the different fields of specialized science becomes the favorite hunting-ground for the philosopher. To the latter, scientific pursuits mean something broader, something higher than a mere concentration on a special field, to the exclusion of all others.

On the other hand, over-specialized science is apt to degenerate into a mere hobby, where all conceptions of true proportions and harmony are lost. The corner grocer who knows all about the prices and qualities of sugar, coffee and tea, and little else, is nothing less than an exaggerated example of over-specialized knowledge. In the same way, I am sorry to say it, I have met many an example of so-called scientists whose science does not rank higher than what is involved in the pursuits of my boy when he is eagerly engaged in the collection of cancelled postage stamps. Knowledge does not contribute necessarily to the wisdom of the individual, unless that knowledge be sufficiently diversified to stimulate his thinking powers. The scientist who spends all his time in purely theoretical work and looks down upon the man who tries to find industrial applications for our knowledge, shows just as much unwarranted one-sidedness as the so-called 'practical' man or empiricist who expresses contempt for purely scientific pursuits.

For fear that I may be misinterpreted, let me repeat that I shall be the last to deny that every one of us is compelled to specialize more or less, in order that we may become thorough in some one branch of human activity and in order to develop our individual usefulness. I am aware that even the dullest specialist, who does conscientious work, will be of some use to the community: If he be engaged in scientific research work, and carefully records well-observed facts, he renders a service to mankind; he presents us with his own home-made little bricks, which in time, will be used by the architects of science to build up the ever-increasing edifice of knowledge.

I hope, I need scarcely add, that I further believe that well-recorded scientific facts of small immediate importance may in time become immensely valuable as compared with elegant but wrongly conceived theories based on hasty generalizations.

I believe, also, that in these times of unbalanced industrialism and greed, the law of self-preservation commands us to select a specialty as a bread-winning pursuit. I am fully aware that insufficient pecuniary resources compel many of us to curtail our preliminary studies to the very minimum consistent with what we absolutely need so as to enter into a remunerative trade or profession.

But however light-weighted our educational baggage may be, when we enter practical life, nothing but our own indifference, our bad judgment, our lack of aspiration towards nobler aims, prevents us from remedying this. Every day of our life, as long as we live, is given to us for increasing by self-culture the slender outfit with which we left school. And self-culture, in order to be effective, ought to be directed so as to counteract any one-sided tendencies resulting from our specialistic daily occupations.

The majority of individuals give by far the largest amount of their work, their endeavors, their thoughts, to the production of wealth, or to put it simpler: the art of making money. Yet, engrossed as we are in this one-sided occupation, very few of us think it worth while to undertake the study of that science which investigates the laws of production and distribution of wealth. Ignoring the true, if elementary, principles of political economy or believing in a perverted political economy which has been invented to serve the ends of a few as against the rights and interests of the many, we help to perpetuate the main cause of numerous social ailments. The scant attempt of serious attention which is given to this branch of knowledge by any but a few specialists, has rendered possible the exaggerations and irregularities of our system of industrialism. It has helped to keep in bondage many deserving men of exalted character but unable to develop their possibilities, as they might do, under a wiser system. Who does not know of some otherwise highly developed individual who now is treated with contempt because he committed the crime unpardoned by our modern society; of failing to master that greedy art of accumulating money for himself, or for others.

Over-specialists as we are in our daily pursuits, we are ever prone to scorn the politicians; we think we have done all our duties as citizens if on election day we take time to cast our vote instead of standing all day on the golf links, but only to find out afterwards that we have supported men who, through ignorance and selfishness, hoodwink us and prevent us making our country the true democratic republic so simply and forcefully defined by Lincoln as: a govenment of the people, by the people and for the people.

If we have any fault to find with our politicians and lawmakers we should blame none but ourselves and that tendency of overspecialization which keeps us in our own narrow routine and lets the politician-specialist rule us and the country as well.

If political economics is a science of momentous interest to everybody alive, it is specially of interest to the chemist, who, thorough believer in the laws of nature, can not fail to admit the same universal yet simple laws in sociology; and who, therefore, is less apt to be misled by those juggleries of reasoning which are so cleverly used to favor private interests against the weal of the community at large. Political economy, different from most sciences, can be mastered without any preparation whatsoever, excepting the relinquishment of all bias and all petty ideas of greed, conceit or inequity.

But what shall I say about our criminal neglect of eugenics, a science which goes to the very roots of our lives; a knowledge which deals with the future of our children, the happiness and betterment of our race, and yet so neglected that its very name is scarcely known in our usual vocabulary. In the meantime, we go on in our happy-go-lucky-slip-shod way; we assume the tremendous responsibility of parentage and we jeopardize the health and happiness of our children and grandchildren by our carelessness. Instead of trying to bring together in marriage, by orderly, careful and methodic selection, such persons as are physically and mentally best fit for ennobling our race, we leave this important matter entirely to the whim of chance, blinded by emotion or prejudice. Our actions in this as in many other instances, are but the logical outcome of a thoughtless one-sided education which does not deal with these subjects, while under the name of *belles-lettres* our thoughts are still further perverted in prose and in rhyme by romantic novelists, who in their own way write on the subject of love and marriage. Neither should we be astonished, if frequently those who feel proudest or brag loudest about their ancestry make very light work of their own lives, as far as their actions involve any responsibility towards their offspring.

Our one-sidedness of conceptions has fraught our whole social system with inconsistencies: we grant the unwarranted privilege to vote to illiterate blacks and whites, tramps or idlers; but from the intelligent, virtuous and active woman who is the mother of our children we absolutely withhold the *right* to participate in the affairs of the nation.

Our lack of broad-mindedness is shown in many other ways. We admit the principle of evolution, but when it comes to concede rights and friendliness towards animals—fellowbeings—we fall short of our theories: we eagerly forget that other living creatures enjoy life and suffer, feel and think as we ourselves, if not exactly in the same way. Some of us claim to be civilized and yet find high pleasure and recreation in hunting, killing, maiming and torturing defenseless animals, although we go on criticizing the Spaniards who enjoy the gore of a bull-fight. And even those of us who admit the savage cruelty of hunting and kindred sports do not hesitate to elevate, propagate and degenerate certain species of domestic animals with the express purpose of killing them for food. We do not see anything inconsistent in the fact that, scientific though we are, and while we talk snobbishly of our refined taste, we are much less particular than plant-eating animals, and we keep feeding on corpses of fellow creatures.

We call ourselves scientists because we believe in the laws of nature. In our studies and our research work we have never-ending opportunities for admiring the marvelous harmony of nature, the invariable laws of God. Yet when we hold our annual banquet of scientists we fail to see that we blaspheme the God of law and order and deny the immutability of his laws by asking him in prayer (and in this similar to savages) to disturb these eternal laws of nature so as to grant us some petty favors, forgetting that we are merely insignificant little dots in the immensity of the universe.

Let me conclude this essay by repeating the main points mentioned therein:

If specialization may be advantageous for increasing our productiveness in a given field of activity, over-specialization, on the other hand, may develop one-sidedness; it may stunt our growth as men and citizens; even for persons engaged in scientific pursuits it may render impossible the attainment of true and general philosophic conceptions.

If I have succeeded in convincing some of us that over-specialization does not bring forth the very best there is in us, if I have contributed ever so little to keep us aloof from the life of dizzy automatic machines, if I have succeeded even in the smallest degree in stimulating you to nobler endeavors, then I shall indeed feel very amply rewarded by your kind attention.

L. H. Baekeland

SCIENTIFIC BOOKS

The Syllogistic Philosophy or Prolegomena to Science. By Francis Ellingwood Abbot, Ph.D. Boston: Little, Brown & Co. 1906. 2 vols. Pp. xiv + 317; vi + 376.

These volumes are the philosophical testament of their author (d. 1903), whose previous works—'Scientific Theism' (1883), 'The Way out of Agnosticism' (1890), and contributions to 'The Index' (1870–80), of which he was editor—constitute preliminary surveys. The work has been in preparation more or less since 1859 (*cf.* ii., 291), and was reduced to its present form in the decade 1893–1903. In his pathetic preface and valedictory words, Dr. Abbot states his purpose and expectations with no uncertain sound. He puts in a claim to have superseded all previous thinkers, to be enrolled with the greatest classics. In so doing, he remembered, doubtless, that he was also courting the stringent criticism which men accord to the classics only.

> If at last it shall receive sober, just and intelligent appreciation, I believe it will be found to have done for philosophy what was done for botany in transition from the artificial Linnæan classification to the natural system of classification by total organic and genetic relationship—a revolution never to be reversed; and to give to ethical and free religion what it has never yet had, a basis in scientific reason (I., xi). My work of forty-four years is done, and I commit its destinies to the Master of Life, whom I have resolutely but reverently sought to know by using the free reason which is his supreme gift to man (II., 296).

In the circumstances, and face to face with Dr. Abbot's *ex cathedra* earnestness, criticism becomes an ungrateful task. One can only say, to begin with, that whether these tremendous expectations are to be justified time alone can tell. But after a careful and sympathetic perusal of the contents, I feel compelled, meantime, to reply in a decided negative. I can not find that Lucretius's address to Epicurus applies:

O tenebris tantis tam clarum extollere lumen
Qui primus potuisti, illustrans commoda vitæ;

and nothing short of this would befit the plea set forth. Well equipped with wide and careful reading as Dr. Abbot evidently was, he seems to have fallen upon an arid formalism which forces him to serve up afresh, and with reiterated emphasis, many of the contingent features peculiar to idealistic absolutism in the nineteenth century. In short his scholasticism is such that he is unfitted by sheer mental constitution for the leadership of that new and transitive school for which he longed. Indeed, it is plain, and to be deplored possibly, that his 'Syllogistic Philosophy' must remain a sealed book to all except a few curious specialists. And, even for this select company, its interest, I apprehend, is already largely historical. For it furnishes what might be termed a species of epilogue to transcendentalism as understood in America. I should judge it typical of certain tendencies of New England unitarianism, rather than symptomatic of the fresh philosophical synthesis which, as many admit, may emerge during the present generation. True, propinquity may have made me myopic; but I can not see the conclusion otherwise. For, despite Dr. Abbot's blindness to his historical position and obligations—a blindness which, paradoxically, lends his work its chief interest—he is little more than another of the many derivants from Hegel, but, as so often, from Hegel with his concrete thinking omitted.

The crux of Dr. Abbot's position resides in his criticism of Hegel. Here he has failed to appreciate the Hegelian distinction between *Verstandes-Allgemeinheit* and *Allgemeinheit des Begriffes*. He would reduce Hegel to the level of a mere continuator of Aristotle, nay, of Aristotle taken at his worst. It is surely a piece of extraordinary perversity to find Hegel's characteristic doctrine of universals in the Nürnberg *Propädeutik* (*cf.* i., 265 f.), even if one may forgive the oversight whereby, at this late date, a writer omits to notice that Aristotle's metaphysical teaching implies a principle by which the 'Paradox' of his logic can be overpassed. And it is still more astonishing to discover that the criticism of Hegel proceeds from a standpoint already made abundantly plain by Hegel himself. No doubt, the Hegelian exploitation of the evolution of the categories may be regarded now as insufficient, or even inapplicable, thanks to those very historical investigations which originated in the impetus exerted by the Hegelian system. But, then, Dr. Abbot offers no concrete *Darstellung* of his own categories. No doubt, evolution is a problem to-day as it could never be to Hegel. But, then, the mere statement that Darwin, by his discovery of 'advantageous variations,' set this new problem, by no means solves it *philosophically*. If the problem is to be attacked from the logical side, a reconsideration of the entire office and operation of disjunction becomes inevitable, and of this Dr. Abbot betrays no consciousness. From first to last he remains curiously impatient of doubt as a test of his own position—he is too sure of it for this, and so he fails to reap the results which follow only from the 'labor of the notion.' The one possible conclusion is that he was so much of an intellectual recluse, even an ascetic, as to injure his perspective.

What quarrel with Hegel has the man who can write as follows? And what obligation does he not owe him? "The only possible modes, functions, or faculties of knowledge are, from the sheer necessity of the case, in the uncreated 'nature of things,' those two forms of activity of the one knowing-faculty which, on the side of the unit, we call sensibility, or perception, or experience, and, on the side of the universal, understanding or conception or reason" (i., 207). Obviously, Dr. Abbot belongs with the monistic idealists; but is so obsessed of abiding a priest continually that he confesses to being without father, without mother, without descent. One does not accuse him of mere apprenticeship to the Berlin master. But, in spirit, general outlook, and necessary consequence, where do we find, if not in Hegel, the kinship of the following, which is Dr. Abbot's conclusion of the whole matter? Not in the Nürnberg *Propädeutik,* truly, nor yet in *reines Denken* as a purely 'subjective' function; but Hegel had some other things to say! "It becomes very clear that one and the same method obtains" in each of the three spheres of being, knowing and doing, "the method of absolute syllogistic. Genera, species and specimens are the only realities in being; genera are realized only in the whole of their species, and species only in the whole of their specimens; the relation of genus, species, and specimen is necessarily that of the three terms in the syllogism. * * * Similarly, ideas, concepts and percepts are the only realities in knowing; ideas are realized only in concepts, and concepts only in percepts; the relation of idea, concept and percept is that of the three terms of the syllogism. * * * Lastly, ideals, purposes and deeds are the only realities in doing; ideals are realized only in purposes, and purposes only in deeds; the relation of ideal, purpose and deed is that of the three terms of the syllogism. * * * Through this principle of absolute syllogistic as the law of unit-universals, or apriori of being, or necessary identity of methods in the sphere of reality and ideality alike, philosophy attains its end in syllogistic as the principle of absolute methodology, and in personality as the topmost reach of its application in human knowledge" (ii., 285 f.). By how much does this differ from, say, the *Rechtsphilosophie?* And by how much the *Rechtsphilosophie* differs from this, because based on an analysis far more profound than that offered in 'Syllogistic'!

For the rest, suffice it to say that students of technical philosophy will find some suggestive criticisms in these pages; for, notwithstanding its author's avowed purpose, the work ranks much stronger in destructive than in constructive material, a circumstance in itself indicative of much. Second, a number of acute interpretations, particularly of Aristotle, Kant and Fichte, are presented, which will raise controversy, and possess the merit of sending the reader to the original sources. Third, Darwin is hailed, not simply as a great scientific man, but as the herald of a new philosophy which, in all likelihood, he would have failed to comprehend. Lastly, much is offered which could be worked up into an epistemology or logic with advantage, were it first subjected to fundamental analyses. For example, we read:

> Every logical conclusion from true premises, that is, every concreted syllogism of knowledge, every true judgment, or real cognition, is one of the ultimate cells which syllogistic, as the cell-theory of the organism of universal human knowledge, recognizes as the indivisible living components of all science and all philosophy. The object, we repeat, determines the subject in knowing. That is, what the object is in itself, even on the idealist's assumption that the subject has created it, must determine all possible knowledge of it; the relations immanent in it must determine all relations immanent in the cognition of it, since any variation in these at once vitiates the cognition so far (II., 247 f.).

Elements are presented here which idealism has not been too prone to emphasize; but they stand in sore need of the regress of criticism.

Dr. Abbot's intense seriousness and total lack of humor, added to his exasperating repetition of formulæ such as the mystic 'My self as one of the we,' and the 'I in the we,' render the work difficult reading; but as a mental gymnastic, the effort to discover the author's special originality and to justify his treatment of the classics of the past, may be recommended. An admirable index makes reference easy.

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American Fossil Cycads. G. R. WIELAND. The Carnegie Institution of Washington, 1906. Pp. viii + 296. Pl. I.–L.

The appearance of this handsome quarto volume marks a very important forward step in our knowledge of the Cycadales, while it also throws a great deal of light upon the general problem of the phylogeny of the gymnosperms and their supposed relation to filicinean ancestors. It is the result of studies carried out by Dr. Wieland since 1898, when the first field work was undertaken. In the present treatment the author devotes his attention to establishing the obvious boundaries and botanical aspects of the cycads, reserving their classification and nomenclature for future consideration; nor does he fail to indicate what must be of the greatest interest to those who are concerned in the solution of broad biological problems, that a study of the seed is certain to reveal much of the highest importance, and we are led to believe that on a future occasion the author may take up this part of the subject more in detail.

The first cycadean trunks from America to receive scientific mention were obtained from the Potomac formation of Maryland and were noticed by Philip Tyson in 1860; but for more than a quarter of a century they remained practically unnoticed until, in 1889, some of the original Tyson specimens which had come into possession of the Maryland Academy of Natural Science were described and illustrated in accordance with their macroscopic characters by Fontaine.

A further collection of Maryland cycads was made in 1893 by Mr. Arthur Bibbins, and this valuable material is now in the museum of the Woman's College, Baltimore. Most of it was gathered from various country people between Baltimore and Washington, representing in all some sixty specimens which had 'been unsuspectedly sequestered from time to time during the preceding hundred years.' Much loss to science resulted during that period, owing to the fact that, being regarded with idle curiosity or with more or less superstitious interest, the specimens had been carelessly treated, while many of those which were too large to handle with ease were broken up and many valuable parts were lost. Characteristic methods of branching were thereby wholly destroyed. As later determined by Professor Ward, this collection was found to embrace seven species of *Cycadeoidea*.

At various times trunks of cycads have been obtained from other widely separated localities, such as the Trias of Prince Edward Island, the Dakota formation of southern Kansas, from one or two localities in Colorado and from California, but the richest deposit of these remains is to be found in the Mesozoic rim of the Black Hills of South Dakota and Wyoming.

Scientific attention was not directed to this locality until 1893, although on several previous occasions miners proceeding to Deadwood had observed them at Black Hawk and Minnekahta; but at that time six silicified trunks were received at the United States National Museum, and five years later they were described by Ward, who found them to include four species of *Cycadeoidea*. This collection, together with another lot of twenty trunks obtained by Professor T. H. MacBride later in the same summer, served to arouse great interest and to awaken the special enthusiasm of Professor O. C. Marsh, of Yale University, whose efforts to secure a representative collection resulted in placing more than seven hundred trunks, many of them of large size and fine preservation, in the Yale Museum. This truly magnificent series furnishes the greater part of the material upon which the present monograph is based.

In the Freezeout Hills of Carbon County, Wyoming, there is another cycad locality which ranks as third in importance on this continent. The discovery of this locality is due to Professor Marsh, who obtained a very large collection of specimens representing exclusively the genus *Cycadella*.

An examination of the American distribution of the cycads shows them to be represented in the following geological horizons:

		Species
1. Trias of Prince Edward Island	*Cycadeoidea*,	1
2. Trias of York, Pa.	*Cycadeomyelon*,	1
3. Upper Trias of North Carolina	*Cycadeoidea*,	1
4. Jurassic of Colorado	*Cycadeoidea*,	1
5. Upper Jurassic, Wealden or Cretaceous of the Black Hills of South Dakota and Wyoming	*Cycadeoidea*,	27
6. Upper Jurassic of Central Wyoming and Black Hills, and from Freezeout Hills, Wyoming	*Cycadella*,	21
7. Potomac Formation of Maryland	*Cycadeoidea*,	7
8. Lower Chico of Colusa County, Cal.		1

9. Dakota Formation of Kansas	*Cycadeoidea*,	1
10. Pre-Laramie (?) of Golden, Colo.	*Cycadeoidea*,	1

From this it may be observed that up to the present it is possible to recognize

	Species.
Cycadeomyelon	1
Cycadella	21
Cycadeoidea	40

with one specimen as yet unidentified.

The author discusses at some length the varying conditions of fossilization and the resulting effect upon structure; and he further directs attention to the alterations of external form due to pressure, either during or after fossilization. The cutting of such bulky material, and more particularly the excision of special parts, required the elaboration of special methods and the manufacture of specially constructed cutting tools; but the exercise of ingenuity, skill and great patience enabled the author to cut all his own sections with great success.

As in existing cycads, the fossil forms show a highly developed armor composed primarily of the persistent leaf bases; but in addition there is a ramentum which is borne over and densely packed between the leaf, peduncle and bract surfaces, as well as thickly enveloping the entire crown. The presence of such a ramentum is well known, not only in the cycads, but also in the ferns. The special features of this structure in the present case are, first, the perfection of its preservation, probably resulting from the free percolation of silica-laden solutions through the hairy ramental mass; and second, the fact that among fossil species the ramentum shows a profuse development which is in striking contrast with its very reduced condition in existing species, and which in *Cycadella* results in fully half the bulk of the trunk being made up of this material. This variation, while at first appearing to constitute the basis of a broad differentiation of the great groups, is in reality little more than of generic importance. The various stages of development found show conclusively that the Cycadeoideæ and Cycadaceæ have been alike subject to a progressive reduction of the profuse ramentum characterizing their common Paleozoic filicinean ancestry.

The most striking external feature of all the cycads is to be found in the armor composed of spirally arranged old leaf bases. In existing cycads the regular order in which the leaves appear is not disturbed except by the appearance of terminal cones. In Cycadeoideæ, on the other hand, disturbance of this arrangement is very common and is due to the emergence of numerous large and laterally borne fructifications, and to the particular level at which periderm formation takes place. In consequence of these disturbances, the leaf arrangement can not be used for either generic or specific distinctions. A much more definite and constant feature is to be found in the particular grouping of the vascular bundles in the leaf scars.

Special interest centers in the character of the inflorescence. A study of the ovulate cone of *Cycadeoidea wielandi* shows a structure which arises from between the old leaf bases or else from their axils in part, at any point between the base of the trunk and the youngest series of leaves. In structure they present the type exhibited by *Bennettites gibsonianus* and *B. morierei*. But it is found that in all the strobili, situated about the lateral bract-bearing surface of the peduncle, and just beneath the terminal ovulate cone, there is an annular shoulder which bears distinct traces of some earlier, dehiscent or abortive or wilted disk. This disk is seated on the receptacle above the bracts, and vascular bundles pass out to it from the woody cylinder of the peduncle. This disk is interpreted as a staminate receptacle, and its presence in all the specimens from the Black Hills is held to signify that all these species were bisporangiate. In some cases the inconspicuous character of the disk leads to the inference that the inflorescence was homosporous. The evidence presented by the great majority of cones studied supports the conclusion that all the known Cycadeoideæ are descended from bisporangiate forms, and that of all the considerable number of fruits of *Cycadeoidea* and *Bennettites gibsonianus,* or allied species, far the larger portion were actually bisporangiate and discophorous. That this conclusion has not resulted from previous studies, and that it has only rarely been suggested in a modified form, is ascribed to the imperfection of the longitudinal sections of cones.

The orthotropous seeds are about the size of a small grain of rye, and each is produced on a separate pedicel. Only one coat encloses the nucellus. In this respect *Cycadeoidea wielandi* is comparable with *Bennettites morierei* from which, however, it differs in detail to such an extent that the two can not be homologized with certainty. The seed coat of the former is nevertheless exactly comparable with *Lagenostoma,* which, of all the existing and extinct forms thus far discussed, affords the most striking structural parallels with American *Cycadeoidea* seeds.

A further parallelism between the American *Cycadeoidea dacotense* and *C. wielandi,* and the European *Bennettites gibsonianus* and *B. morierei,* is to be found in the presence of well-marked dicotyledonous embryos which more or less nearly fill the entire space and indicate a nearly, if not complete, exalbuminous condition. These embryos are strikingly like those of *Gingko.* Evidence has also been obtained with respect to the existence of an earlier or preembryonic stage which has never been found preserved in any other specimen or hitherto observed in any other fossil gymnosperm or other plant. The evidence points to the replacement of the oospore by a homogeneous tissue and the absence of a suspensor. The embryo was therefore formed directly through growth of the oospore which thus represents the proembryo or protocorm. The suggestion arising from these facts is an analogy with *Ginkgo* in which there is a much more simple form of embryogeny than in other gymnosperms.

One of the most striking facts revealed by the studies so far completed, is that the hiatus between the two great Cycadean lines is of a two-fold character. In existing cycads great complication of the cortical bundle system has developed, while the reproductive organs are relatively little changed and primitive. Conversely, in the Cycadeoideæ there is a retention of the primitive cortical system together with the most surprising reproductive changes leading up to the bisexual flower which mimics that of the angiosperms. It is therefore natural to ask if two groups so related shall be included in one greater class, the Cycadales, or the Cycadeoideæ be excluded from the true Cycadales, as Bennettitales or Cycadeoidales? After a careful review of the positions taken by Scott, Zeiller, Potonié and Count Solms, and of the evidence afforded by the paleontological record, it is held that the Cycadeoideæ find their appropriate place amongst the true Cycadales.

An interesting summary of the fern-cycad relations, together with suggestions bearing upon analogies of the ferns and angiosperms, closes a very able treatment of a difficult but intensely fascinating problem. The general tendency of the evidence is to greatly strengthen the current views respecting the marattiaceous origin of the cycads; or, in the pregnant words of the author "The preceding résumé of the principal characters of the two great cycad groups as combined and showing their descent from marattiaceous ferns of the Paleozoic, is not merely conclusive, but one of the great cornerstones upon which the conception of evolution can rest secure."

D. P. Penhallow

Montreal,
May, 1907

SCIENTIFIC JOURNALS AND ARTICLES

The April number (volume 8, number 2) of the *Transactions of the American Mathematical Society* contains the following papers:

E. Kasner: 'Dynamical trajectories: the motion of a particle in an arbitrary field of force.'

W. R. Longley: 'A class of periodic orbits of an infinitesimal body subject to the attraction of n finite bodies.'

E. B. Van Vleck: 'A proof of some theorems on pointwise discontinuous functions.'

L. E. Dickson: 'Invariants of binary forms under modular transformations.'

E. J. Wilczynski: 'Projective differential geometry of curved surfaces (First memoir).'

J. I. Hutchinson: 'A method for constructing the fundamental region of a discontinuous group of linear transformations.'

E. B. Wilson: 'Oblique reflections and unimodular strains.'

C. N. Moore: 'On the introduction of convergence factors into summable series and summable integrals.'

The May number (volume 13, number 8) of the *Bulletin of the American Mathematical Society* contains: Report of the February meeting of the San Francisco Section, by W. A. Manning; Report of the Fifty-seventh Meeting of the American Association for the Advancement of Science, by L. G. Weld; 'On a Final Form of the Theorem of Uniform Continuity,' by E. R. Hedrick; 'The Groups Generated by Three Operators Each of which is the Product of the Other Two,' by G. A. Miller; 'A Table of Multiply Perfect Numbers,' by R. D. Carmichael; 'The Symmetric Group on Eight Letters and the Senary First Hypoabelian Group,' by L. E. Dickson; 'Double Points of Unicursal Curves,' by J. E. Wright; 'The Mathematical Tablets of Nippur,' by D. E. Smith; 'Osgood's Theory of Functions' (Notice of Professor W. F. Osgood's Lehrbuch der Funktionentheorie), by H. S. White; Shorter Notices (Arnoux's Introduction a l'Etude des Fonctions Arithmétiques, by W. H. Bussey; Neumann's Studien über die Methoden von C. Neumann und G. Robin zur Lösung der beiden Randwertaufgaben der Potentialtheorie, by O. D. Kellogg; Biermann's Vorlesungen über mathematische Näherungsmethoden, by J. W. Young; Ariès's La Statique Chimique Basée sur les deux Principes Fondamentaux de la Thermodynamique, by E. B. Wilson); Notes; New Publications.

SOCIETIES AND ACADEMIES

THE IOWA ACADEMY OF SCIENCES

The twenty-first annual session of the Iowa Academy of Science was held at Drake University, Des Moines, Iowa, on April 26 and 27. The meeting was well attended and much interest was manifested in the papers presented. In addition to the regular program, illustrated lectures were given on the evening of the twenty-sixth by Professor H. L. Russell, of the University of Wisconsin, on 'Recent Discoveries with Reference to Insect-borne Diseases,' and by Professor W. W. Campbell, director of the Lick Observatory, on 'The Solar Eclipse in Spain.'

The officers elected for the ensuing year are:

President—John L. Tilton, Simpson College.

First Vice-President—C. L. Von Ende, State University.

Second Vice-President—Nicholas Knight, Cornell College.

Secretary—L. S. Ross, Drake University.

Treasurer—H. E. Summers, Iowa State College.

The following program was presented:

The Influence of Science in Forming Ideals. President's Address: C. O. Bates.

Exposures of Iowan and Kansan (?) Drift, East of the Usually Accepted Boundary Line of the Driftless Area: Ellison Orr.

(*a*) *Volcanic Phenomena around Citlaltepetl and Popocatepetl, Mexico;* (*b*) *Physiographic significance of the Mesa de Maya;* (*c*) *Tertiary Terranes of New Mexico:* Charles R. Keyes.

A Visit to the Panama Canal (illustrated): Grant E. Finch.

An account of three weeks of observations on the Canal Zone during the summer of 1906. Impressions of climatic conditions and of problems and progress in the canal enterprise.

(*a*) *The Channel of the Mississippi between Lansing and Dubuque* (illustrated); (*b*) *The Unconformity at the Base of the Saint Louis Limestone* (illustrated): S. Calvin.

(*a*) *Recent Alluvial Changes in Southwest Iowa;* (*b*) *Effect of Certain Characteristics of Formations upon Rate of Their Erosion:* J. E. Todd.

(*a*) *The Loess of the Missouri River* (illustrated).

In large part a rejoinder to Professor Todd's late paper on the same subject, especial attention being given to his attempted explanation of the manner in which the shells of molluscs found their way into the deposit.

(*b*) *The Loess of the Paha* (illustrated).

The formation of loess on the Paha by wind is explained largely on the basis of plant ecology.

(*c*) *The Loess and the Nebraska Man* (illustrated): B. SHIMEK.

A brief discussion of the weakness of the evidence that the human remains found near Florence, Nebr., are in undisturbed loess.

The Orbit of the Asteroid, 1906 W. E.: E. B. STOUFFER.

A Catalogue of the Poisonous Plants of Iowa: L. H. PAMMEL and ESTELLE D. FOGEL.

The purpose of the catalogue is to enumerate the plants that are poisonous to live stock.

A Study of the Variation in the Number of Ray Flowers in Certain Compositæ: W. S. DUDGEON. (Presented by L. H. Pammel.)

A study was made of the ray flowers of the following plants: *Rudbeckia triloba, R. hirta* and *Helianthus grosseserratus.* The constant was worked out according to Professor Davenport's formula. The ray flowers of *R. hirta* vary from 2 to 28 out of 3,847 counted; 1,327 had 13 rays. The ray flowers of *R. triloba* vary from 5 to 18. The ray flowers of the sunflower vary from 7 to 25. In *R. hirta* there appear to be two well-marked forms.

Iowa Erysiphaceæ: J. P. ANDERSON.

Notes on Iowa Algæ: R. E. BUCHANAN.

Keys to groups and species of algæ and their reported distribution in the state.

The Homologies of Tissues in Ferns: H. S. CONARD.

Studies in Karyokinesis: J. E. GOW.

An account is here presented of some observations on the process of cell division in the pollen mother cells of *Trillium sessile,* in the vegetative cells of the nucellus of *Arisæma,* and in the young root tips of *Zea mays.* Attention is called to the latter as contradicting, apparently, certain accepted theories of vegetative cell divisions.

(*a*) *The Estimation of Silica;* (*b*) *The Analysis of Some Iowa Waters:* NICHOLAS KNIGHT.

The Recent Investigation of Iowa Ground Waters: W. S. HENDRICKSON.

Some Problems in Municipal Sanitation: L. H. PAMMEL.

The question of the water supply for cities and villages is a very important one. With the density of population increasing, the problem becomes more complex. All of our Iowa streams are more or less polluted. The paper discusses some of these and the supposed case of pollution when a railroad passes over water. Such pollution will not occur if proper precautions are taken.

The Physical Science Laboratory of the State Normal School (illustrated): A. C. PAGE.

Description of new laboratory presented because of possible interest to any contemplating building.

The Lateral Line System of Amphiuma: H. W. NORRIS.

Securing a Stand of Clover on the Southern Iowa Loess—A Biological Study: E. B. WATSON.

L. S. ROSS,
Secretary

THE AMERICAN PHYSIOLOGICAL SOCIETY

THE seventh special meeting was held at Washington, May 7 and 9, in conjunction with the Congress of American Physicians and Surgeons.

The sessions of the society were held in the Physiological Laboratory of the George Washington University.

The scientific program was as follows:

TUESDAY, MAY 7

YANDELL HENDERSON: 'Production of Shock by Loss of Carbon Dioxide and Relief by Partial Asphyxiation.'

J. A. E. EYSTER: 'Vagus Inhibition from Rise of Pressure in the Aorta.'

DONALD R. HOOKER: 'May Reflex Cardiac Acceleration occur Independently of the Cardio-inhibitory Center?'

WILLIAM H. HOWELL: 'The Calcium and Potassium Metabolism of the Heart during Inhibition and Acceleration or Augmentation.'

T. SOLLMANN: 'The Acute Effects of Gastric and Peritoneal Cauterization and Irritation on the Blood Pressure and Respiration.'

T. SOLLMANN: 'Perfusion Experiments on Excised Kidneys: Solutions of Electrolytes.'

VELYIEN E. HENDERSON: 'The Teaching of

Physiology in the Laboratory.' (A discussion of this paper is especially invited.)

C. C. GUTHRIE: 'Results of Removal and Transplantation of Ovaries in Chickens.'

R. S. LILLIE: 'The Influence of Electrolytes on the Osmotic Pressure of Colloidal Solutions.'

THURSDAY, MAY 9

Joint session with the American Society of Biological Chemists.

REID HUNT: 'Notes on the Thyroid.'

WALTER JONES: 'On the Occurrence of Ferments in Embryos.'

C. G. L. WOLF and PHILIP A. SHAFFER: 'Metabolism in Cystinuria.'

C. G. L. WOLF: 'Protein Metabolism in the Dog.'

A. B. MACALLUM and C. C. BENSON: 'The Composition and Character of the Hourly Excretions of Urine.'

S. P. BEEBE: 'The Parathyroid Gland.'

V. C. VAUGHAN: 'Proteid Susceptibility and Immunity.'

WALDEMAR KOCH: 'The Distribution of Sulphur and Phosphorus in the Human Brain.'

A. D. EMMETT and WILLIAM J. GIES: 'On the Composition of Collagen and the Chemical Relation of Collagen to Gelatin.'

LAFAYETTE B. MENDEL: 'Embryo-chemical Studies.'

Joint session with the Association of American Physicians.

Symposium upon acidosis. The discussion was introduced by Dr. E. P. Joslin, representing the Association of American Physicians, and Dr. Otto Folin, representing the American Physiological Society; and followed by a discussion, in which Dr. Graham Lusk, Dr. Lafayette B. Mendel, Dr. A. E. Taylor, Dr. L. F. Barker, Dr. W. S. Thayer and others took part.

The following resolutions were adopted by the society, in association with the American Society of Biological Chemists at the joint session: "This society approves of the movement represented by the Committee of One Hundred of the American Association for the Advancement of Science to increase and coordinate the present activities of the federal government in matters pertaining to public health. This society therefore urges upon the President of the United States and members of Congress the favorable consideration of such legislative measures as are best adapted to secure this result."

The society decided to hold the next annual meeting in Chicago during Convocation week, December, 1907.

LAFAYETTE B. MENDEL,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 429th meeting was held April 6, 1907, with President Stejneger in the chair.

Under short notes Dr. Hopkins called attention to the influence of the recent abnormal warm weather on the opening of the buds of certain forest trees, stating that certain early varieties of American linden trees on B Street, southwest, were fourteen days earlier this spring than last, but that the buds on the late varieties of the same species were not influenced, thus indicating a method of locating varieties of forest trees and the determination of the range of a given periodical phenomenon within a species as influenced by normal and abnormal seasonal conditions. Phenological data collected during the past ten years show quite conclusively that the average time of the beginning of seasonal activity of certain species and varieties of indigenous plants and animals, that remain dormant during the winter, may be utilized as an index or guide to the dates each season when, at different latitudes and altitudes, the conditions are most favorable for action against certain insect pests, plant diseases, etc. The same records show that there is a normal variation in a given phenological phenomenon of about four days for a difference of four hundred feet of altitude and four days for a difference of one degree of latitude, thus it has been shown that within a state like West Virginia there may be a variation of thirty days on the same degree of latitude, due to a difference of 3,000 feet altitude, which is equivalent to a difference of about seven degrees of latitude at the same altitude. Thus, the normal variation between two localities may be calculated approximately, but the response of life activity in certain index forms of plants and animals, to general and local climatic and other influences, will not only give quite positive evidence of the actual variation between localities, but will serve as most reliable guides to the solving of certain economic problems, as, for instance, the control of certain insect enemies of forests which require remedial action within a short period in their seasonal history.

The practical application of the principle outlined in these remarks has been referred to by Dr. Hopkins in Bulletin 50, West Virginia Agricultural Experiment Station, 1898, pp. 17–18, Bulletin 67; ibid., 1900, pp. 241–248; and Bulletin No. 58, Part III., Bureau of Entomology, U. S. Department of Agriculture, 1907, p. 32.

Dr. Gill, apropos of his recent Smithsonian article on 'Parental Care among Freshwater Fishes' and the numerous cases of oral gestation and harboring the young in the mouth, called attention to an article in a Swiss journal (*Bibliothèque universelle*, Geneva, 1905) by Dr. Fuhrmann, announcing that an osteoglossoid fish of Borneo (*Scleropages formosus*) also took the young into the mouth after hatching; the data given, however, were scanty and it was not stated whether the egg-carrier was the female or male.

The regular program consisted of an address by Dr. George A. Soper, of New York, on 'A Chronic Typhoid Fever Producer,' and discussion following.

The speaker, after introduction by Dr. L. O. Howard, gave a detailed account of his investigation covering several months into the source of a household epidemic of typhoid fever occurring in Oyster Bay, N. Y., during the summer of 1906. Of eleven persons six developed positive cases of typhoid between August 27 and September 3. Several suspected sources—water, milk, vegetables, fruit and soft clams—were excluded by careful study and examination. Repeated sanitary analysis of the water supply and failure to detect subsoil pollution by fluorescein tests of the drainage showed the infection was not water borne. Typhoid was unusual in Oyster Bay and there were no cases immediately preceding or following those under consideration. The milk and food supply of the infected household was common to others of the village without the occurrence of other cases. None of the patients had been absent for several weeks prior to the outbreak and they therefore had acquired it on the premises. The house and surroundings were in an entirely hygienic condition. The investigator inferred the occurrence of some unusual event prior to August 20, and found it in a change of cooks August 4. The new cook's term of service with this family covered a period three weeks prior to and three weeks subsequent to the outbreak. She refused to give any information tending to connect her with the cases, but an independent investigation of her previous service disclosed a startling and significant history of typhoid. Despite the fact that her record for nearly two of the past five years is yet unknown, twenty-six cases of typhoid fever, including one death, were associated with her service in seven families during this time. The cases were almost entirely among the servants and the initial case frequently occurred soon after the arrival of the cook. She did not directly admit having herself suffered from typhoid, but to three persons she is said to have previously testified to a mild attack.

The evidence indicating the cook to be a competent cause of typhoid, she was taken into custody by the New York City Department of Health, March 11, 1907, and at the detention hospital a bacteriological examination was made. She was a large healthy Irishwoman, single, forty years of age. The urine was free of typhoid bacilli, but the stools showed great numbers nearly every day for the several weeks of observation. The blood gave a positive Widal reaction. Thus a healthy and vigorous subject was shown to be a chronic typhoid-fever producer. As the typhoid organism is known to persist for years in the gall bladder, this is the presumed source of the infection, removal of which requires the consent of the subject.

The speaker called attention to recent papers by Dr. Robert Koch and others on the important investigations in western Germany of typhoid outbreaks by the aid of portable or 'flying' laboratories. To the Germans the dangers of bacillus-carriers were well known. Stress was laid on the importance of contact in transmission and on the analogy in this respect between typhoid and diphtheria and tuberculosis. A careful campaign is necessary to discover bacilli-carriers once they escape the physician's care. The rigorous measures of isolation and disinfection adopted in Germany are perhaps impracticable here at the present time, except during epidemics.

In the discussion which followed, Dr. W. C. Woodward, health officer of the District of Columbia, commended the accuracy and the scientific spirit of Dr. Soper's work and spoke of the desirability and the difficulty of procuring such accuracy in the routine work of a health department and in obtaining for such routine work men imbued with the same scientific interest as that displayed by Dr. Soper. The extreme difficulty of detecting bacillus carriers and of enforcing upon them proper isolation would constitute a serious obstacle in the way of preventing the spread of disease through them. The public should guard, however, against forming exaggerated ideas of the danger from this source. Among the many hundreds of patients suffering from typhoid fever who have come to the knowledge of the Health Department during recent years, many were housewives and other persons who, upon convalescence, necessarily took an active part in the ordinary affairs of the household. And although many of such cases must have been what are now termed bacillus carriers for longer or shorter periods after convalescence, the Health Department had yet to find a case in which there was an outbreak of typhoid fever, or even a second case, in the household after the convalescence of the first case.

Dr. Woodward asked why it was that in the families in which the bacillus carrier which formed the subject of Dr. Soper's investigation, had been employed, the number of the members of the families affected by the disease was apparently relatively so small as compared with the number of persons affected among the persons employed about the household and about the premises.

Dr. M. J. Rosenau said that the typhoid problem is now magnified and the difficulty of dealing with it increased. Though complex and intricate, the situation is not hopeless. It is not necessary to imprison the bacillus carrier; it is sufficient to restrict the activities of such an individual. As for typhoid in Washington, if largely transmitted by bacillus carriers and contacts a different epidemiological picture might be expected; the disease should increase progressively, whereas it is epidemic in the summer, declining rapidly in the fall; there is little typhoid fever here in the winter and spring. Dr. Rosenau believes that when large quantities of virulent cultures are ingested the disease is frequently induced within the usual period of incubation. Ordinarily, however, persons become infected with dilute cultures or attenuated bacteria which remain in the intestinal tract awaiting lowered resistance before the disease manifests itself. This lowered resistance is largely brought about by the enervating effects of the hot weather. This explains the seasonal prevalence of typhoid fever and why it is a summer disease in Washington and many other places.

Dr. L. O. Howard, while appreciating that a charge of professional bias might lie, felt it necessary to emphasize the rôle of the housefly as a carrier. Whether its importance is secondary, tertiary, or further removed in degree, it is certainly known to transmit typhoid even under city conditions. The housefly is abundant in low quarters and near waste lots in cities like Washington. To do away with it is a simple matter. It is but necessary to oblige stable keepers to dispose of horse manure, since 99 per cent. of the houseflies in cities are bred in it. Dr. Soper's paper has emphasized the importance of this measure by showing the possibilities of healthy human subjects as carriers. Such persons of unclean habits increase the opportunities of the housefly as a transmitter of infection.

Dr. C. W. Stiles, referring to the question why bacillus carriers did not infect more widely, drew an analogy from parasitic worms. *Oxyuris* (*Oxyurias*) *vermicularis* is transmitted by the hands. As with typhoid distributors it might be expected to infect a large number of persons. This is not the case. For instance, in an orphan asylum of several hundred, perhaps only eight or ten may be infected, though the worm is transmissible from hand to mouth. The dwarf tape worm, *Hymenolepis nana,* is another case in point. Personal cleanliness has an important bearing.

Mr. K. F. Kellerman said that Doctor Soper's typhoid investigations have shown weighty reasons for the sterilization of sewage. Chemical sterilization is practicable at low cost, by nascent chlorine or one of the heavy metals, and should be resorted to when sewage is discharged into streams which in a short time are used as the sources of drinking water. He seconded Doctor Howard with respect to the importance of flies. At Panama typhoid is rare, and the few cases are confined to the lowest classes of negro laborers who eat in the open where flies are abundant instead of in screened dining-rooms such as are used by the Americans.

Dr. J. Goldberger gave some figures on the frequency of bacillus carriers. One thousand seven hundred cases examined at three of the laboratory stations in Germany showed 3 per cent. to be chronic carriers. It is possible to calculate the number of carriers for a given district and the probable danger to non-immunes.

Dr. G. Lloyd Magruder recalled the early typhoid investigations in Washington and the surrounding country which cited the water supply, cesspools, manure piles and milk as carriers. The city wells and water supply of many dairy farms were found contaminated. It is difficult to enforce sanitary precautions in the city and almost impossible in the country. The farm is an important source of city typhoid and the fly an important carrier on the farm.

Dr. Soper in closing replied to questions and amplified certain points. With respect to the cost of his investigation he preferred to give no figures. It had cost him personally more than he had agreed to charge at its beginning. The investigation would hardly have been undertaken in this particular instance, save for its scientific interest. His typhoid work was generally done for cities, as at Ithaca and Watertown. As to why more persons other than the servants were not attacked in the families served by the cook, he believed the members of the family were protected in large measure by the sterilizing effect of cooking, the food being chiefly handled after cooking by butlers and waitresses or servants other than the cook. The cook never handled fruit, salads and other things eaten raw by the family. Servants newly attached to the household were more apt to take the disease than those long associated with the cook. Possibly an acquired immunity explains this. Hand infection is important and the hands should receive more attention than they do. It would be well if cooks could be selected only after careful assurance concerning their histories and personal habits. In general, scrupulous cleanliness is an important safeguard against typhoid. Increase in knowledge of bacillus carriers should be looked upon as encouraging rather than otherwise, since it is only by a knowledge of the facts that preventive measures can be accurately applied and transmission can be prevented. As to the situation at Washington, the speaker preferred to say nothing until the official report of the exhaustive investigations of the Public Health Service had been made public.

At the conclusion of the meeting the chair tendered the society's thanks to the speaker for his address.

M. C. MARSH,
Recording Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 190th meeting of the society, held at the Cosmos Club, on Wednesday evening, March 27, 1907, under informal communications, Mr. F. E. Wright showed a new double screw micrometer ocular by the use of which the optic axial angle of a bi-axial mineral can be determined on any section showing in convergent polarized light an optic axis within the field of vision.

The regular program consisted of an exhibition of the geologic relief map of the Southern Appalachian Province prepared for the Jamestown Exposition. The following description of the territory covered by the map was given:

The Appalachian Mountains and Valleys: Mr. ARTHUR KEITH.

The four main geographic divisions of the Appalachians are typically shown in this area.

These are the Piedmont Plateau on the east, the Appalachian Mountains, including the Blue Ridge and joining the Appalachian Valley northwest of the mountains, and, still farther northwest, the Appalachian Plateau, including the Cumberland and Allegheny plateaus.

Southeastward from the Blue Ridge the streams flow directly to the Atlantic in most of the area, northwestward from it they flow into the Appalachian Valley, and southwestward along the valley into Alabama. Most of the drainage of the Appalachian Plateau is dendritic and flows northwestward into the Ohio River from the eastern margin of the plateau.

The characteristic topography of the Piedmont Plateau is a smooth, even-topped upland into which the stream channels are rather deeply dissected. The Appalachian Mountains, from maximum heights of 6,600 and 6,700 feet in western North Carolina, become gradually lower toward Alabama and Virginia. Around their southern end the Piedmont Plateau merges with the Appalachian Valley. The same is true in less degree in Virginia. The Mountains are rugged and deeply dissected, especially where they rise abruptly from the Appalachian Valley. The great Appalachian Valley is a composite of many small valleys, separated by sharp linear ridges and mountains. The valleys follow the beds of soft rock and the ridges the hard sandstones. The height of the valley is greatest (about 2,000 feet) in southern Virginia and descends in either direction to about 500 feet in Alabama and northern Virginia. The Appalachian Plateau in Tennessee and Alabama are typically flat, table-topped mountains, more or less dissected by stream gorges and narrow valleys. They are preserved from erosion by beds of hard sandstone. In Kentucky and farther north the sandstones are less prominent and the region is extensively dissected into a network of hills and knobs. The summits of these, however, fall in general into planes.

Only the larger rock divisions were shown in color on the model. These corresponded in the main with the great time divisions—Cambrian, Ordovician, etc. The Carboniferous was shown in two divisions, the Pennsylvanian and Mississippian, and the Cambrian was divided into a lower siliceous and upper calcareous group. The Archean was divided into gneisses and igneous rocks. These greater divisions correspond closely with the great geographic divisions, the topographic features, in fact, being very largely determined by the progress of erosion on the different formations, according to their solubility. Thus, the Appalachian Plateau is formed mainly of Pennsylvania sandstones and shales with bordering zones of the Mississippian limestone. The Appalachian Valley is underlain in the main by narrow bands of the Devonian, Silurian, Ordovician and the calcareous division of the Cambrian rocks, while along its southeast border lie the siliceous Cambrian rocks. These also form the northwestern part of the mountains in a comparatively narrow band extending throughout the Appalachian system, with a few outliers farther southeast.

The main mass of the mountains is composed of gneisses through which have been injected igneous rocks of various descriptions —mainly granites. These rocks are, for the most part, Archean, but include also some of Algonkian age. They extend southeastward over the Piedmont Plateau in broad areas. Over the plateau there are also found large masses of later igneous rocks of approximately Carboniferous age. In this respect the Piedmont Plateau resembles the eastern part of the Appalachian province in New England. Other resemblances are seen in central Virginia and North Carolina, where sediments of Silurian age are found. Knowledge of these parts of the Appalachians is at present very limited. Over the Piedmont also are isolated basins of Triassic red sandstones and shales.

The Appalachian structures also fall into main groups similar to the geographic and geologic features. The Appalachian Plateau is underlain by rocks which are nearly flat. In the valley all of the formations are steeply folded, overturned, and, in places, faulted. The local changes in the extent and type of deformation express the differences in the character of the rocks. Open folding at the north progresses into steeper folding southwestward, then into faulted folds and overthrusts, until in southern Tennessee and northern Georgia faults are much more prominent than folds. Huge overthrusts of many miles throw extend from lower Virginia into Georgia. These have been folded and faulted by later deformation. In the mountains similar structures prevail and metamorphism is added thereto. This increases rapidly toward the southeast and in large areas has destroyed the original aspect of the formations.

These structures were produced by tremendous force which thrust the pre-Cambrian masses northwestward against the sediments. According as these masses were unequally advanced the sediments were deformed and the great bends of the Appalachian Valley produced. Most of the structures run for great distances in parallel lines, but there are many cross folds extending across the valley and mountains.

Deformation was active in pre-Cambrian time, appeared in less degree at several times during the Paleozoic, and culminated in the post-Carboniferous Appalachian revolution. The Piedmont Plateau shared to some extent in the deformation of Triassic time, but the rest of the region appears to have escaped. Still later uplifts have appeared at various times up to the Quaternary and can be traced through the topographic forms. The land was uplifted and warped in broad levels or domes.

The Plateau Region: Mr. M. R. Campbell.

The Appalachian Revolution: Mr. Bailey Willis.

Assuming that the geologic structure of the Appalachian zone is too well known to require any descriptive statement, Mr. Willis proceeded to discuss the larger problems of the nature and origin of the movement involved in Appalachian folding. He referred to the hypothesis which he had once entertained of a movement of the interior continental region from northwest to southeast, a movement supposed to be of such a character that the mass of ancient crystallines in North Carolina formed the buttress against which Paleozoic strata were folded. He gave reasons for abandoning this view and accepting that which is more generally entertained, of a movement from the southeast toward the northwest. Tracing this northwestward movement, he showed that all of the known mass of the continent southeast of the Appalachian zone had been involved in it; that we must suppose a belt a thousand miles long and several hundred miles wide to have moved northwestward between thirty and forty miles. With reference to such displacement of the continental margin, he stated his belief that it involved the expansion of a sub-oceanic sector. Developing this idea by illustration of continental compression in North America and Asia, he stated a general theory that since an early geologic date, continents have from time to time been compressed in consequence of the expansion of the material beneath the oceanic basins, and he attributed this expansion to the plastic flow of rocks considered as rigid solids, which are nevertheless not sufficiently firm to maintain their form as masses of oceanic extent and one hundred miles or more deep. This property of plastic movement would apply equally to sub-continental as to sub-oceanic masses, but in view of the greater density of the latter, the motion has been from the oceanic toward the continental areas.

Economic Conquest of the Southern Appalachian Coal Field: Mr. Geo. H. Ashley.

Mr. Ashley traced briefly the movements resulting in the populating of the district, pointing out some of the factors affecting that movement, the routes by which it took place and the relation of those routes to the physiography. He then reviewed briefly the early efforts in marketing the coal by the use of the rivers reaching from the Ohio up into the coal fields, pointing out the difficulties encountered and how these were overcome by building dams and locks. Then came a study of the gradual incoming of railroads in which he pointed out the relation of the routes chosen to the physiography, some of the difficulties encountered, and the territory opened up to export as a result.

Ralph Arnold,
Secretary

THE NEW YORK ACADEMY OF SCIENCES

At the meeting of the Section of Geology and Mineralogy, January 7, the following paper was presented:

Volcanoes of Colima, Toluca and Popocatepetl: Edmund Otis Hovey.

Toluca is the oldest of the three volcanoes. A feature of greatest interest in the crater is the dome of vitreous andesite which welled up in the crater as the latest phase of the activity of the volcano and shows a certain resemblance to the cone of Mt. Pelé, with regard to origin. The volcano of Popocatepetl shows its composite character as a strato-volcano in the walls of the crater, and streams of lava have been among the features of the most recent eruptions. The volcano of Colima is still sending up a vigorous column of steam from its central summit crater. From this summit crater there poured out, in the latest eruption (1903), streams of very frothy lava which present a strange appearance on account of the porous character of the surface blocks. The same feature characterizes the streams of the earlier eruptions and has led some observers to the erroneous conclusion that flows of lava have not occurred at the volcano of Colima.

The major portion of the evening was then devoted to an examination of the exhibits of geology, paleontology and mineralogy in the New York Academy of Sciences Exhibition, under the guidance of the committeemen in charge of those exhibits.

At the meeting, April 1, Mr. Robert T. Hill gave a discussion of the tectonic structure of the northern part of the Mexican Plateau, which was published in Science for May 3.

Dr. Alexis A. Julien then spoke on the 'Evidence of the Stability of the Rock Foundations of New York City.' The general facts were reviewed which might justify the confidence of builders in the operations of extensive construction now in progress. Two former periods of enormous seismic activity in this region were considered, as recorded by the violent faulting produced at each time. The one, connected with the foldings, slips and shattering during the great Appalachian uplift, and now revealed by the numerous pegmatite intrusions cutting irregularly across the stratum of crystalline schists, probably effected during Cambrian time. The other, after the close of the Mesozoic, during the thrust of lava sheets between the sandstones and shales of the Newark series of New Jersey, now indicated by many faults across Manhattan Island and the adjacent Palisade Ridge. The long period of cessation of uplift, of ensuing subsidence and extensive surface erosion, offers the conditions in this region which promise long stability, notwithstanding the slight tremors noted at intervals of thirty or forty years. In the absence of disturbance of the glacial striæ, everywhere abundant, which serve as natural benchmarks to record changes of level or faulting, we obtain therefore direct testimony to the established absence of tremor during the long and approximately definite period which has elapsed since the passage and withdrawal of the continental glacier. In other parts of the Hudson River valley, however, some evidences of post-glacial faulting have been observed.

Alexis A. Julien,
Secretary of Section

THE ELISHA MITCHELL SCIENTIFIC SOCIETY OF THE UNIVERSITY OF NORTH CAROLINA

The 172d meeting was held in the main lecture room of Chemistry Hall, Tuesday, April 16, 7:30 p.m., with the following program:

Professor Archibald Henderson: 'The Foundations of Geometry.'

Professor Chas. H. Herty: 'The Optical Rotation of Turpentines.'

Alvin S. Wheeler,
Recording Secretary

DISCUSSION AND CORRESPONDENCE

THE CLOCKS OF THE GREENWICH AND U. S. NAVAL OBSERVATORIES

To the Editor of Science: In Professor Eichelberger's paper, published in your issue of March 22, 1907, he gives a comparison of the performance of six clocks, at various periods from the time of Bradley in 1750.

This table is primarily intended to show the improvement made in one hundred and fifty years; but incidentally it brings out an apparently strong contrast in the performance of the clocks in use at Greenwich Observatory and the Naval Observatory, Washington. Clocks can now be made to give, under ordinary conditions, very fair and steady rates; but if greater excellence is required, than beyond the best possible mechanical construction, means more or less independent of the clock, as such, have to be devised to obtain it. For instance the Greenwich clock is kept in a room in which the temperature does not vary more than 5° F., and it has an automatic device to correct for barometric error. Thus its conditions are practically the same as those of the Washington, although attained by different means. The Washington clock is kept in an air-tight case in a room whose temperature varies say 5° C. A comparison then of the actual performances of these two clocks is of great interest. Professor Eichelberger's figures on page 451 fail to do this, and for these reasons:

1. The Greenwich Clock rates are spread over a period of one year, while those of the Washington clock only extend over three selected months.

2. The quantity $^s.015$ given in the table in the second column on page 451 is not comparable with $^s.051$ given for the Greenwich clock.

$^s.051$ is the mean deviation of the observed rate.

$^s.015$ is the mean deviation of the observed rate from one calculated from formulæ, and hence quite distinct from $^s.051$.

Professor Eichelberger, while admitting that his comparison is not valid, appears not to realize that it is altogether vitiated by dealing with periods of widely different deviation, and the fact that $^s.051$ and $^s.015$ represented two distinct phenomena seems to have escaped his notice.

It is, however, from the material he gives a simple matter to make such a comparison: the rates are taken from the table on page 451, and the column 'mean deviation' exhibits the difference of the separate rates from the mean rate $+ ^s.016$.

For the Greenwich clock the corresponding period of 1904 is first given, and in addition the same period of 1905.

It will be seen that the difference is really in favor of the Greenwich clock.

U. S. NAVAL OBSERVATORY CLOCK

	1904	Mean Daily Rate s.	Mean Deviation s.
Feb.	8–11	+ .019	.003
	11–15	— .014	.030
	15–20	+ .005	.011
March	1– 4	— .026	.042
	4– 9	— .010	.026
	9–16	— .022	.038
	16–18	— .043	.059
	18–22	— .022	.038
	22–25	— .029	.045
	25–28	+ .002	.014
	28–34	— .007	.023
April	3– 5	+ .017	.001
	5–13	+ .002	.014
	13–16	+ .026	.010
	16–19	+ .034	.018
	19–22	+ .002	.014
	22–31	+ .029	.013
May	1– 4	+ .113	.097
	4– 7	+ .082	.066
	7–12	+ .161	.145
	Mean	+ .016	± .035
	Range $0^s.204$.		

GREENWICH CLOCK

	1904	Mean Daily Rate s.	Mean Deviation s.
Feb.	8–12	+ .110	.078
	12–15	.123	.065
	15–19	.135	.053
March	1– 3	.180	.008
	9–16	.203	.015
	16–18	.235	.047
	18–22	.180	.008
	22–25	.206	.018
	27–30	.156	.032
	30–Apr. 1	.180	.008
April	1– 5	.191	.003
	5–13	.207	.019
	13–16	.223	.035
	16–19	.240	.052
	19–22	.227	.039
	24–29	.192	.004
May	1– 4	.220	.032
	4– 7	.207	.019
	7–12	+ .158	.030
	Mean	+ .188	± .030
	Range $0^s.130$.		

GREENWICH CLOCK

1905		Mean Daily Rate s.	Mean Deviation s.
Feb.	1– 4	—.243	.016
	6–12	.225	.002
	12–21	.225	.002
	21–March 1	.258	.031
March	2–12	.228	.001
	12–18	.220	.007
	18–24	.200	.027
	24–Apr. 1	.217	.010
April	1– 8	.222	.005
	8–16	.222	.005
	16–23	.178	.049
	23–May 1	.232	.005
May	1– 6	.248	.021
	6–12	—.274	.047
	Mean	—.227	±.018
	Range $0^s.096$.		

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REASONS FOR BELIEVING IN AN ETHER

SEVERAL weeks ago an article with this title appeared in SCIENCE. In it were mentioned two reasons for the belief in an ether; but what seems to me the most powerful of all arguments was not mentioned, nor is it often referred to elsewhere. It is alluded to by Maxwell in his article 'Ether,'[1] where we find these words:

> In the next place, this energy is not transmitted instantaneously from the radiating body to the absorbing body, but exists for a time in the medium.

The ether was originally invented to avoid the assumption of action at a distance; but there are no insuperable objections to action at a distance *provided it be instantaneous.* Herein lies the point of the argument. We have replaced the old question: "Can a body act *where* it is not?" by the far more searching question: "Can a body act *when* it is not?"

The energy sent out by the sun, for instance, reaches the earth after a lapse of some eight minutes. What of the energy during that time? The principle of the conservation of energy forbids our supposing that it is annihilated and recreated eight minutes later; and it will hardly be urged, I think, that it exists as a sort of disembodied spirit during that interval. There must be some medium in which it may reside during its finite time of passage from place to place.

The ether stands or falls with the principle of the conservation of energy.

PAUL R. HEYL

[1] Encyc. Brit., ninth edition, Vol. VIII., p. 570.

THE FIRST SPECIES RULE

THE article by Professor John B. Smith in the May 10 number of SCIENCE under the above title, in which exception is taken to the operation of the first species rule in the case of the lepidopterous genus *Rhynchagrotis* Smith, can hardly be considered as an argument against the use of this method of type fixing. His objection is against the selection of a doubtfully referred species as the type of a genus, a matter which is fully covered in most, if not all, codes and is *entirely independent* of the *method* of selecting types, whether by elimination or first species rule.

In the specific case mentioned by Professor Smith we fail to see that the species *chardinyi* selected as the type by Sir George Hampson was 'questionably referred,' as in the original description Professor Smith says: "The group, though placed with, and certainly very closely allied to *cupida,* yet shows so many peculiar characteristics that it would seem possible to separate it by a distinct generic term. The two species *rufipectus* and *brunneicollis* are, however, somewhat intermediate and as the species (*chardinyi*) can hardly be referred to *Triphœna,* I prefer to leave it here." Regarding this Professor Smith states in his recent article: "my reason for placing it there being that I believed it would prove to be properly referable to an exotic genus to which I did not care to risk making a synonym."

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PHILADELPHIA, PA.,
May 17, 1907

SPECIAL ARTICLES

SOME MUTUAL EFFECTS OF TREE-ROOTS AND GRASSES ON SOILS[1]

IT is commonly noticed that plants of the Gramineæ do not grow readily under certain species of trees, and while many reasons have been assigned, there does not seem to have been much systematic experimentation to determine the principal causes.

Shade thrown by the trees themselves has been commonly mentioned as an important factor. Considerable shade in itself will retard the growth of many grasses, but it is hardly probable this is important in the case of single trees; moreover, the shaded area varies with the time of day, and the grass receives light much of the time. Also, if shade is an important factor, why does not the grass live on the sunny side of the tree? Near the base of the trunk of low-branched trees this might become a controlling factor.

Another reason commonly assigned is the removal of 'plant food' by the tree, thus starving the grass. As the parts of the tree roots most active in removing soluble salts from the soil water are the newer root tips and branches, it hardly seems that these could be held responsible for the almost entire removal of plant food over the entire affected grass space. They would, at any rate, not usually be active for some distance from the tree trunk, and though roots may here be near the surface or even exposed, they play no active part in the removal of mineral constituents from the soil. As the soil solution is practically a constant as regards the amount of salts in solution, it would seem that were the removal of plant food by the tree very excessive, there would still be sufficient available for the grass owing to the nature of the solubility of soil minerals. As, however, it would be impossible to say in just what quantity and in what combination the plant food should be present for the best development of tree and grass, respectively, this factor of plant food removal is difficult definitely to rule out.

Another reason assigned, and perhaps the more logical, is the removal of water from the soil by the tree. The average sized tree during active growth transpires an enormous amount of water, especially if the season be hot and dry; but so does the grass, and it would be about as logical to blame the grass for removing so much water from the soil as to cut short the available supply for the tree. Here again, the root system for some distance from the tree does not play an important rôle in absorption of water. These possibilities of plant food and water removal would seem to be negatived by the experiments at Woburn to be mentioned later.

While all these factors working in conjunction may produce an effect on the growth of grass, there seems to be a much deeper underlying principle involved.

During some experiments carried out on a lawn in Takoma Park, Md., where a few scattered oak, chestnut and pine trees are growing, it was found almost impossible to obtain a stand of grass or clover. Beds were spaded up, stable manure applied and later artificial fertilizers added and the best of care given the plots. The grass and clover (the latter also inoculated with nitrogen bacteria) came up very well and for a time gave promise of a good stand, but in a month or two all died in spite of good care. When the plots were originally spaded up, many tree roots were encountered which were removed; the soil being shallow, these naturally live near the surface. The plots on which the grass had died were later spaded up again and found to be almost entirely filled up with young actively growing roots, the special preparation of the soil having been very favorable for their growth.

As the lawn is everywhere permeated with roots (though the trees are not close enough to form a dense shade) it was thought that these might have some malignant influence on the grass. It has been shown that washings from tree trunks and tree leaves are injurious[2] to plant growth, which might account for some of the trouble experienced in trying to obtain a stand of grass, but as the trees do

[1] Published by permission of the Secretary of Agriculture.

[2] Bull. 28, U. S. Department of Agriculture, Bureau of Soils.

not cover the entire lawn area, this cause could not be the only one. The converse effect, *i. e.*, a deleterious effect of grass on trees, was found by the Duke of Bedford and his co-workers.[3] In 1897 they began to notice the peculiar effect produced by grass upon their fruit trees, especially apple and pear trees. The soil on this farm is shallow, eighteen to twenty-four inches of soil overlying an impervious calcareous subsoil.

Their first supposition was the removal of plant food, and so they inaugurated experiments to determine if this assumption was correct, but all their experiments answered the question in the negative. They then tried if the removal of water by the grass was the cause, but here again they received a negative answer. They tried the effect of carbon dioxide on the tree roots, thinking this might be given off in such large quantities by the grass as to be harmful. This not proving to be the cause, they tried the effect of the exclusion of oxygen, and also of the effect of packing imitating the impervious sod, but in all cases they were baffled, finding no evident effect of any of these factors on the trees.

Having ruled out all the above-mentioned factors, they found by other experiments that only the most actively growing portions of the tree root system was affected by the grass. A circular sod of a few feet in diameter around the tree had no effect, but as the circle was increased, the tree began showing the detrimental effect, viz., premature falling of the leaves, and entire change of the normal ripe color of the fruit, from green to red, and a dwarfing of the tree. In very many instances the trees were killed outright. They also found by excavating the ground around the trees and by removing the root system, that the pernicious effect of the grass was strongly marked even when only one thousandth to two thousandths of the root system of the tree was exposed to the action of the grass.

They finally, after about seven years' work, concluded the pernicious effect of the grass could be due only to some poisonous substance formed in the soil around the tree roots, leaving the question open as to whether these substances were due to direct excretions from the grass or to a changed bacterial action in the soil induced by the presence of the grass.

Jones and Morse[4] have described a similar relation existing between the shrubby cinquefoil (*Potentilla fructicosa*) and the butternut tree (*Juglans cinerea*), the latter killing the former for an area equal to and often much greater than that of the tree top. Excavations showed in every case of the dead or dying cinquefoil that the butternut tree roots were in close physical relation with those of the shrub. Young birch, beech, maple, cherry, apple and pine trees growing among the cinquefoil in the same field had no such influence on the latter. More recently an antagonism between peach trees and several herbaceous plants, commonly used as cover crops in orchards, has been reported by Hedrick.[5]

In work done in these laboratories, Reed found unquestionable evidence that plants do produce toxic conditions in the medium in which they grow. Agar in which wheat had grown was decidedly toxic to a second crop of wheat. Agar in which corn or cowpeas had grown was scarcely, if at all, toxic to wheat, Agar in which oats had grown was quite toxic to wheat, but not as toxic as that in which wheat itself had previously grown. Apparently excretions from the roots of a given plant, or its near relatives, are more toxic to that species than the excretions from plants belonging to more distantly related species.

It was decided to try the effect of tree seedlings on the growth of wheat under control of external factors, and accordingly a number of tree seedlings were dug up in the forest in the early part of June, 1906. The species gathered were pine, tulip, maple, dogwood, and cherry, and varied in height from about 15 to 40 cm., care being taken to get the entire root system. These were planted in paraffined wire pots,[6] using soil already made

[3] Woburn Experimental Fruit Farm, 3d Rept., 1903, and 4th Rept., 1904.

[4] Rept. Vt. Expt. Sta., 16 (1903), 173–190.

[5] *Proc. Soc. Hort. Sci.*, 1905, 72.

[6] This method of pot culture is fully described in Cir. No. 18, Bureau of Soils, U. S. Dept. of Agriculture.

up to optimum moisture condition. The water content was kept up subsequently by frequent watering. These pots, along with two controls, were put into the greenhouse and left standing for about two weeks before planting to wheat in order to enable the tree roots to become established in the soil. At the end of this time, all pots were planted to wheat, putting the same number of germinated wheat seeds in each pot.

The first crop of wheat was cut down at the end of about three weeks and weighed and the pots immediately replanted without disturbing the soil. The wheat was similarly planted and harvested every two or three weeks until the middle of December, by which time nine crops had been removed. In each crop the average green weight of the plants in the controls was considered 100, and the relative weights of the others calculated on this basis. The accompanying table shows the tabulated results of the successive crops. There is plainly a decrease in the green weight of the plants grown in the pots with the trees. This can not be due in any way to shade, as the seedling trees were not large enough to interfere in this way, and the pots were all arranged in a row, so all had an equal amount of light. The water content can not be a factor, either, as all were watered every day or two during the hot summer and every three or four days during the cooler autumn. The 'plant food' removed can hardly be considered a serious factor in the case of such small seedlings, especially as the crops increase again, as will be pointed out below, and there were of course no leaf washings from the trees to affect the wheat.

It seems, therefore, that the presence of the roots must have had some other effect on the growth of the wheat, as the size of the pots made it necessary for the two kinds of roots to be in close physical relation. That the retarding effect is due to substances excreted by the tree roots seems probable, and a closer inspection of the table shows an evident increase in yield toward autumn when the physiological activities of the trees were diminished by their entering upon their seasonal rest. It was also noticed that the tree pots that produced as much wheat growth in November as the controls were the ones in which the trees showed the earliest signs of winter rest. Attention should be called to the fact that if the tree seedlings removed sufficient plant food to starve the wheat plants in the summer period, the increase in yields toward autumn would hardly be looked for.

The increase in wheat growth in the various pots toward autumn is more clearly brought out in the last two columns. The first shows the average of the preceding six crops, which brings the time up to the middle of October, about the time the physiological activities of the trees would be decreasing, as the crop harvested October 13 was not planted until about September 20. The second of these columns shows the average of the last three crops.

It will be seen that there is a decided increase in the average yield of these three crops over the average of the preceding summer crops, except in the case of the dogwoods, and they were, excepting the pine, the last to drop their leaves, having only dropped them

RELATIVE GREEN WEIGHTS OF WHEAT CROPS, GROWN IN ASSOCIATION WITH TREE SEEDLINGS

Date of Harvesting.	6/29	7/12	8/1	8/22	9/6	10/13	10/29	11/19	12/8	Average of First 6 Crops (Summer)	Average of Last 3 Crops (Autumn)
Control	100	100	100	100	100	100	100	100	100	100	100
Maple 1	76	65	86	68	67	86	92	91	96	74 }	93 }
Maple 2	44	86	75	59	71	89	90	75	109	71 } 72	91 } 92
Maple 3	21	83	72	72	79	84	81	103	92	70 }	92 }
Dogwood 1	92	96	76	84	71	65	83	68	115	81 } 79	89 } 91
Dogwood 2	86	79	63	86	75	73	84	107	88	78 }	93 }
Cherry	81	91	102	91	71	94	88	102	93	88	94
Tulip	21	106	82	77	68	100	77	109	103	76	96
Pine	55	69	68	52	54	80	62	83	60	63	68
Pine (dead)	62	96	85	91	80	89	97	96	67	84	87

when the last crop of wheat was removed on December 8.

An interesting case is that of the two pine seedlings. During the growth of the first crop one of these died, and the pot with the dead seedling left intact was carried on in the set and treated in the same way as the other cultures. The greater yield in this pot over that in the pot containing the live pine is clearly evident.

Another feature is the variation in yield obtained in the pots with different species of trees. It would appear that the cherry was least active in checking growth of wheat, the dogwood next, followed by the tulip, then maple, and most of all, live pine, although it would not be safe to assume this same order would obtain in the field.

It should be mentioned that in replanting the wheat, the soil was disturbed only enough to accomplish this, so the organic matter left by the wheat roots would act as a light application of green manure, although it is well known that wheat is not very effective as green manure. This would perhaps help slightly to counteract the deleterious effect of the tree roots on the wheat, but the aim was to leave the soil undisturbed.

Summarizing the foregoing, we find that seedling trees of tulip, dogwood, maple, cherry, and pine retard growth of wheat when the latter is grown under conditions making it necessary for the wheat roots to be in close physical relation with the tree roots. That this retarding effect differs with different species of tree seedlings, that the checking of wheat growth is greatest during the season when the tree seedlings are most active physiologically, and this checking effect becomes less as the season of physiological inactivity of the trees is approached. That in the case of pine, at least, the live pine is much more detrimental to wheat growth than the dead pine.

This injurious effect of trees on wheat appears to be due to the excretion of substances by the trees, toxic to wheat growth.

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NOTES ON ORGANIC CHEMISTRY

FORMATION OF FUSEL OIL

THE production of fusel oil during the course of the ordinary alcoholic fermentation involves grave practical difficulties to the manufacturer of distilled spirituous beverages, because the removal of this constituent entails a considerable expense. To the pure chemist also, this formation of fusel oil is of importance because it, apparently, complicates the chemical changes involved in the course of fermentation. The conversion of grape sugar, $C_6H_{12}O_6$, into alcohol, $2C_2H_6O$, and carbon dioxide, $2CO_2$, is very simple, but to account for the production of small, variable amounts of amyl alcohol and similar substances compels the use of quite complicated equations. The difficulties of both the brewer and the chemist will be lessened, or wholly removed by some highly interesting work which Felix Ehrlich[1] has carried out in the Berlin Institution of Sugar Industry. He has fermented pure sugar solutions with pure yeast cultures and obtained, on an average, about 0.4 per cent. of fusel oil. The addition of *l*-leucine,

$$\begin{matrix}CH_3\\CH_3\end{matrix}{>}CHCH_2CH(NH_2)CO_2H,$$

or of *d*-isoleucine,

$$\begin{matrix}CH_3\\C_2H_5\end{matrix}{>}CHCH(NH_2)CO_2H,$$

to the fermenting material immediately raised the content of fusel oil to 3 per cent. The former compound gave inactive amyl alcohol,

$$\begin{matrix}CH_3\\CH_3\end{matrix}{>}CHCH_2CH_2OH,$$

and the latter, optically active, dextro-rotatory amyl alcohol,

$$\begin{matrix}CH_3\\C_2H_5\end{matrix}{>}CHCH_2OH.$$

On comparing the formulæ it will be observed that the alcohols can be formed from the leucines by the addition of the elements of water and the elimination of ammonia and carbon dioxide.

The latter substance is, of course, evolved, and the question arises as to the fate of the ammonia. Special experiments showed that the fermenting liquid and the gases issuing from it were free from ammonia and nitrogen,

[1] *Ber. d. chem. Ges.*, 40, 1027 (1907).

and that the amount of amyl alcohol formed was equivalent to the quantity of leucine which disappeared in the course of the reaction. It follows, therefore, that the ammonia must be absorbed by the yeast as rapidly as it is produced and be converted into insoluble albuminoid material.

Evidently the formation of fusel oil is dependent on the assimilation of nitrogen by the yeast and it was found, by further experiments, that the addition of asparagine, $H_2NCOCH(NH_2)CH_2CO_2H$, or of certain ammonium salts such as the carbonate or sulphate, all of which liberate ammonia far more readily than the amino-acids, such as the leucines, almost completely prevents the formation of fusel oil. The results were the same irrespective of the kind of yeast employed and of the presence or absence of leucine from the mixture. Solutions of ordinary molasses behaved like those of pure sugar. The formation of fusel oil under industrial conditions appears, therefore, to be due essentially to the presence of amino acids in the mash, and not to those which the yeast contains. It is also obvious that the removal of these acids is not necessary for the prevention of the formation of fusel oil.

The same chemist has also carried out a number of experiments on the production of certain higher and more complicated alcohols from amino acids, in the presence of fermenting sugar. He finds that the action is a general one and that it appears to resemble certain activities in plants. Thus, from phenylalanine, $C_6H_5CH_2CH(NH_2)CO_2H$, he obtained phenylethyl alcohol, $C_6H_5CH_2CH_2OH$, which is the chief constituent of the odoriferous material of the rose.

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BOTANICAL NOTES

HOW TO STUDY THE FUNGI

GEORGE MASSEE, the well-known mycologist of Kew, has brought out a useful 'Text-book of Fungi' (Duckworth and Company, London), which is intended to serve as an introduction to those new lines of research included in the morphology, biology and physiology of the fungi, 'and also to indicate where fuller information may be obtained.' The reader will observe that the book is not, like some of its English predecessors, a book of *information,* only, but it is intended to foster investigation and research, in accordance with present-day ideas as to the proper function of a text-book for advanced students. For it must be remembered that this is no elementary presentation of the subject for children in the secondary schools, or others who have not had a good preliminary training in the 'general botany' courses in the college or university. It is, on the contrary, a book for the college or university student who has already acquired a good general notion of the fungi, and their relations to other members of the vegetable kingdom, and who is now ready to take up their particular study.

The book is roughly divided into three parts: I., Morphology, Physiology, Biology, etc. (195 pages); II., Pathology (36 pages); III., Classification (183 pages). In the first, such topics as the cell, anatomy of fungi, formation of spores, sexual reproduction, asexual reproduction, effect of light, effect of low temperature, respiration, transpiration, enzymes, parasitism, symbiosis, heteroecism, mycoplasm, chemotaxis, geographical distribution, ecology, phylogeny, etc., are taken up at greater or less length, and it is safe to say that any properly prepared student who carefully goes over this part of the book will do so with great profit, and will get a very good modern understanding of these plants. In the second part the student finds helpful discussions of the diseases caused by fungi, the spread of disease by means of hybernating mycelium, legislation against disease-producing fungi, etc. The third part opens with a discussion of the classifications of the fungi, followed by a systematic account of the orders and families. The author arranges all fungi under six orders, namely; Phycomycetes, Hemiascomycetes, Ascomycetes, Hemibasidiomycetes, Basidiomycetes, Deuteromycetes. The text is illustrated with 141 figures, which add much to the usefulness of the book.

A NEW EXPLANATION OF THE TOLERANCE AND INTOLERANCE OF TREES

BEFORE a recent meeting of the Society of American Foresters Mr. Raphael Zon read a paper presenting the new explanation of the tolerance and intolerance of trees.

The theory of tolerance as formulated by Pfeil and Gustav Heyer and the classification of trees into light-needing and shade-enduring is the foundation upon which the forester bases all his practical work in silviculture. Many biological, ecological and silvicultural facts have, however, been accumulating which tend to show that the increased growth of trees after thinning or the possibility of securing reproduction of certain species only in full light or after heavy thinnings, are due chiefly to temperature and moisture condition, and, not, as has been supposed, to light requirements. It has been found, for instance, that the same structural differences which occur between trees growing in the open and trees growing in the shade, such as the reduction of leaf surface, the diminution of intercellular spaces, the lengthening of the palisade cells, etc., occur also between trees growing in dry or in moist situation, or in a dry or humid atmosphere. It has also been observed that trees within the same climatic region are more tolerant of shade when grown in fresh or moist soils, than when grown on dry or poor soils.

Fricke, a German silviculturist, has proved recently by a number of very interesting and convincing experiments that the failure of Scotch pine to grow under the shade of mother trees was not due to lack of light, but to deficiency of moisture, which is a result of competition with the roots of the larger trees.

In a stand of pine about one hundred years old, with a crown density of 0.7, growing on poor sandy soil, where the light requirements of pine are greatest, a number of isolated groups of suppressed young pines were located. The young pines were ten years old and about a foot and a half high. These groups were surrounded by ditches so that the roots of the neighboring large trees were cut through to a depth of ten inches. The little trees within the ditched areas immediately responded. The needles had double the length of the preceding summer, the terminal shoots became longer and the growth generally thrifty and has continued so, while the young growth not surrounded by ditches retained the same suppressed character. Subsequently he determined by analyses that the soil moisture content in the areas surrounded by ditches was from 30 to 40 per cent. higher than that on contiguous areas not ditched and penetrated by the roots of living trees.

It must, therefore, be conceded that the moisture conditions play an extremely important part in determining the behavior of trees growing in shade and in light, and that the rôle which light plays is by no means an all-important one. The theory of the tolerance and intolerance of trees as hitherto understood is not tenable and must be thoroughly revised.

A LABORATORY MANUAL

SOME time ago Bergen and Davis's book entitled 'Principles of Botany' was noticed in these columns (SCIENCE, January 25, 1907). We have now a companion or supplementary volume in the 'Laboratory and Field Manual of Botany' (Ginn) by the same authors, intended for the use of pupils in high schools, and perhaps in the smaller colleges. It includes ten or eleven chapters on such topics as laboratory methods and equipment (full of excellent suggestions); structure and physiology of seed plants (accompanied with forty-two suggestive experiments); type studies, preceded by the study of the plant cell (beginning with simple forms and passing regularly to higher and higher types); ecology (accompanied with many studies); botanical microtechnique (full of excellent advice); culture methods (containing much of great practical value); material, apparatus, and supplies (a most helpful chapter for the teacher); bibliography (carefully classified); glossary, etc. The book is so evidently the result of years of experience by the authors in the supervision of botanical laboratory work that we are not surprised at its air of practicality and workableness. It must prove most useful in the better class of high schools.

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LINNÆUS AND THE NEW YORK ACADEMY OF SCIENCES

The commemoration on May 23 by the New York Academy of Sciences of the two-hundredth anniversary of the birth of Linnæus took place in accordance with the program that has already been printed in Science. In the morning exercises were held in the American Museum of Natural History, where there was an exhibition of animals and minerals known to Linnæus; a presentation of letters by delegates from other societies, and an address by Dr. J. A. Allen on 'Linnæus and American Zoology.' After luncheon had been served to invited guests, the exercises were resumed in the New York Botanical Garden, where there was an exhibition of American plants known to Linnæus; an address by Dr. P. A. Rydberg on 'Linnæus and American Botany,' and lantern slides of American flowers known to Linnæus were exhibited by Dr. H. H. Rusby. In a walk through the garden, trees known to Linnæus were pointed out by Dr. W. A. Murrill. Later the bridge over the Bronx River on Pelham Parkway between the Botanical Garden and the Zoological Park was dedicated in memory of Linnæus and a bronze tablet was unveiled. The tablet was presented by Dr. N. L. Britton, president of the New York Academy of Sciences, and was accepted by the commissioner of parks of the Bronx. Addresses were also made by Dr. George F. Kunz, president of the American Scenic and Historic Preservation Society, and by Mr. Emil F. Johnson, president of the United Swedish Societies of New York. There was singing by the American Union of Swedish Singers, and the Swedish minister to this country and the Swedish consul of New York City were present. Subsequently there was an exhibit of animals known to Linnæus in the New York Zoological Park. In the evening the exercises were continued at the Museum of the Brooklyn Institute of Arts and Sciences, addresses being made by Mr. F. A. Lucas and Mr. E. L. Morris. There was a reception at the New York Aquarium given by the New York Zoological Society to the New York Academy of Sciences. The committee of the New York Academy in charge of the celebrations was: Nathaniel L. Britton, Hermon C. Bumpus, William T. Hornaday, Frederic A. Lucas, Charles H. Townsend, William Morton Wheeler.

SCIENTIFIC NOTES AND NEWS

M. Douvillé has been elected a member of the Paris Academy of Sciences in the section of mineralogy in the place of Bertrand.

Professor A. S. Warthin, of the University of Michigan, has been elected president of the American Association of Pathologists and Bacteriologists.

Professor Charles F. Burgess, of the department of chemical engineering of the University of Wisconsin, was elected to the presidency of the American Electro-chemical Society at the fifth annual meeting held in Philadelphia.

On the occasion of the centennial of the University of Maryland honorary degrees will be conferred as follows: Doctor of science—Dr. Henry D. Fry, Baltimore; Dr. Alexander C. Abbott, University of Pennsylvania; Dr. Henry J. Berkley, Johns Hopkins University; Edwin S. Faust, Strassburg; Dr. Isaac Stone, Washington, D. C.; Dr. Charles P. Noble, Philadelphia; J. Homer Wright, Harvard University; Dr. J. Whitridge Williams, Johns Hopkins University; Dr. N. G. Keirle, Baltimore. Doctor of laws—Dr. Wm. T. Councilman, Harvard University; Major-surgeon James Carroll, U. S. A., Washington, D. C.; Dr. Simon Flexner, Rockefeller Institute for Medical Research; Professor W. D. Halliburton, King's College, London; President G. Stanley Hall, Clark University; Dr. Francis L. Patton, Princeton, N. J.; Judge James McSherry, Frederick, Md.; Surgeon General Walter Wyman, U. S. N., Washington, D. C.; Dr. S. J. Meltzer, New York City; Professor William T. Porter, Harvard Medical School; Dr. William J. Mayo, Rochester, Minn.; Professor William T. Howard, Baltimore; Professor C. A. Ewald, University of Berlin; Professor Samuel C. Chew, Baltimore.

Under a grant from the Smithsonian Institution, just approved by Secretary Walcott, Mr. Bailey Willis, of the United States Geological Survey, will be sent to Europe this summer to study the types of geological structure involved in the Alps. Mr. Willis will leave on the first of August for a two months' trip, during which he will make observations at the most important spots in Italy and Switzerland, and confer with a number of geological experts. On his return he will formulate his investigations into a report which will be published by the Smithsonian Institution.

Dr. David Eugene Smith, professor of mathematics of Teachers College, Columbia University, has next year his sabbatical year of absence, which he will spend mainly in Asia, making collections of manuscripts and instruments illustrating the history of mathematics.

Dr. C. R. Weiland, of the Peabody Museum, Yale University, has left for a stay of five months in Europe where he will visit the plant collections of northern and southern Europe for a special study of cycads. The results of his investigations will be published in his second volume on cycads.

Professor Liveing, for forty-six years professor of chemistry at the University of Cambridge, expects to retire next year.

Dr. Werner Janeusch has been appointed curator of the Geological and Paleontological Institute of the University of Berlin, and Dr. Otto Schneider has been given charge of the collections of the Geological Institute.

Professor Frederick C. Shattuck, of Harvard University, will give the annual address before the Medical School of Yale University on June 24, his subject being the 'Art and Science of Medicine.'

From May 6 to May 10 Professor R. H. Chittenden, director of the Sheffield Scientific School of Yale University, lectured at the University of Illinois on the general subject of 'Nutrition.' In the course of six lectures Professor Chittenden outlined our present knowledge of the physiology of nutrition, and gave a historical and critical discussion of dietary habits and experiments bearing on true food requirements. The general trend of the lectures was toward the conclusion that the most advantageous diet for man is one that includes a minimum of proteid material, *i. e.*, the amount actually needed to meet the physiological requirements of the body. A reception in honor of Professor Chittenden was held at the conclusion of the opening lecture of the series.

Mr. Charles F. Scott, consulting engineer of the Westinghouse Manufacturing Company, will give the commencement address on June 13 at the Worcester Polytechnic Institute, his subject being 'Some Aspects of Electrical Development.' The annual commencement lecture will be given on June 11 by Mr. A. S. Ritchey, a professor of electrical railway engineering, on 'The Electric Railway.'

The fifteenth 'James Forrest' lecture before the Institution of Civil Engineers, London, will be delivered by Dr. Francis Elgar, F.R.S., on June 18, his subject being 'Unsolved Problems in the Design and Propulsion of Ships.' The fourth engineering conference will be held on June 19, 20 and 21, and the annual *conversazione* will be held on the evening of June 20, at the Royal Albert Hall.

Beginning on May 23 Sir James Dewar is giving three lectures before the Royal Institution on Chemical Progress-work of Berthelot, Mendeléef and Moissan.

The fund for a memorial to the late Robert Henry Thurston, director of Sibley College, is now complete. This fund, started by the four classes in college at the time of Dr. Thurston's death, but since added to by other Sibley students, amounts to about $1,600. The memorial will be a bronze bust of Dr. Thurston by Herman Atkins MacNeil, of New York, formerly instructor in Sibley College. The bust will be placed in the central Sibley building.

The two-hundredth anniversary of the birth of Linnæus was celebrated at Augustana College on May 13, the program being as follows: Biographical, J. A. Udden, Ph.D., of Augustana College; 'The Place of Linné in the Scientific World,' Charles E. Bessey, Ph.D., of the University of Nebraska; 'Vårsång' (spring song) by Prince Gustaf, the Wennerberg Chorus; 'Linné and the Love for Nature,' E. K. Putnam, A.M., of Davenport Academy of Sciences; remarks by Josua Lindahl, Ph.D., of Cincinnati Museum of Natural History, and P. A. Rydberg, Ph.D., of the New York Botanical Garden.

A SECOND series of tablets was unveiled in the Hall of Fame of New York University on Memorial Day, May 30. Addresses were made by Governor Hughes of New York and Governor Guild of Massachusetts. Among the twelve tablets unveiled was one in memory of Maria Mitchell, the astronomer, and one in memory of Louis Agassiz. The tablet in honor of Agassiz was unveiled under the auspices of the American Association for the Advancement of Science with brief addresses by Dr. Charles D. Walcott, secretary of the Smithsonian Institution, and Dr. Edward S. Morse, director of the Peabody Institute of Science.

THE Brooklyn Institute of Arts and Sciences commemorated the hundredth anniversary of the birth of Agassiz by a meeting on May 28. The principal address was made by Dr. Franklin W. Hooper, director of the institute.

SIR BENJAMIN BAKER, F.R.S., the eminent British engineer, known among other important works for the Forth Bridge in Scotland and the Assouan Dam, died on May 19, at the age of sixty-seven years.

DR. ALEXANDER BUCHAN, F.R.S., the eminent Scottish meteorologist, died on May 13, at the age of seventy-eight years.

LIEUT. GEN. ZACHARIAE, vice-president of the International Geodetic Commission, died at Copenhagen, on May 16, at the age of seventy-two years.

DR. GUY DAVENPORT LOMBARD, instructor in histology in Cornell Medical College, died in New York on May 22, at the age of thirty-five years.

GUY WARNER EASTMAN, assistant in physics in the Massachusetts Institute of Technology, was killed by a train in Boston on May 17. Mr. Eastman was engaged in researches under Professor A. A. Noyes and completed the work for the degree of doctor of philosophy. He was twenty-six years of age.

THREE Paris medical societies—the Société de Médecine et de Chirurgie Pratique, the Société de Médecine de Paris and the Société Médico-Chirurgicale—have combined together under the title of Société de Médecine. The first meeting of the new society was held on May 14 under the presidency of Dr. Paul Coudray.

THE Seismological Observatory, erected at the expense of the pope, was inaugurated on May 19 at Valla di Pompeii, near Naples. Father Alfani, director of the Florence Observatory, made the inaugural speech. Director Hagan and Vice-director Mannucci of the Vatican Observatory were present.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of Maryland will celebrate from May 30 to June 2 the centennial of its foundation. On Thursday, May 30, there will be a reception of representatives from other universities, alumni and invited guests and in the afternoon an inspection of the buildings. On the following day there will be addresses by President Francis L. Paton, of Princeton Theological Seminary, and by President G. Stanley Hall, of Clark University, followed by the conferring of regular and honorary degrees. On Saturday there will be a reception at St. John's College, the academic department of the University of Maryland, when a large shield with the seals of the two institutions and the coat of arms of the University of Maryland will be presented to the college.

THE Michigan Agricultural College celebrated the semi-centennial of its foundation from May 26 to 31. On May 28 and 29 the annual meeting of the American Association of Agricultural Colleges and Experiment Stations was held at Lansing. On the twenty-ninth addresses were made on the 'College and the State,' including an address by the governor of the state. The afternoon session on 'Builders of the College,' included addresses by Professor Charles E. Bessey, of the University of Nebraska, and by Professor W. J. Beal, of the college. The exercises on Thursday were under the auspices of the American Association of Agricultural Colleges and Experiment Stations and were presided over by Professor L. H. Bailey, director of the College of Agriculture, Cornell University. Addresses on agricultural and engineering education and research work in the land grant colleges were given by Dr. Elmer Ellsworth Brown, commissioner of education; President W. E. Stone, Purdue University, and Dr. W. H. Jordan, director of the Geneva Experiment Station. Memorial Day exercises took place on May 30. On Friday morning congratulatory addresses were presented from institutions and learned societies, followed by addresses from the Hon. James Wilson, Secretary of Agriculture; President James B. Angell, University of Michigan; President Henry Clay White, College of Agriculture and Mechanic Arts of the University of Georgia; President Benjamin Ide Wheeler, of the University of California, and President Edmund J. James, of the University of Illinois. In the afternoon there was a procession of delegates, state officials, members of legislature, public school officials, alumni, faculty and students, followed by an address by the president of the United States and the conferring of regular and honorary degrees.

The Hanna chair of political science and the Selah Chamberlain chair of sociology, recently established at Western Reserve University, have been filled by the appointment of Professor A. R. Hatton, of the University of Chicago, and Dr. James E. Cutler, of the University of Michigan.

Nearly one hundred representatives of the University of London, including the vice-chancellor, the member for the university, the principal, deans of faculties and members of the professorial staff, visited Paris this month for a three days' stay, on the invitation of the University of Paris.

Dr. Sidney Dean Townley, astronomer in charge of the International Latitude Observatory at Ukiah, California, and lecturer in astronomy in the University of California, has been appointed to an assistant professorship in the department of applied mathematics at Leland Stanford Junior University. Dr. Townley will assume the duties of his new position with the beginning of the next academic year in August.

Dr. C. H. Mathewson, Ph.D., Gottingen, now of the Massachusetts Institute of Technology, has been appointed instructor in chemistry and metallurgy in the Sheffield Scientific School of Yale University. At the same institution Frank L. Cooper, who receives this year his doctorate of philosophy from the Johns Hopkins University, has been appointed instructor in physics.

G. F. Kay, of the University of Kansas, has been appointed professor of mineralogy and economic geology in the University of Iowa.

Appointments at McGill University have been made as follows: Dr. A. G. Nichols, lecturer in clinical medicine; H. M. McKay, associate professor of civil engineering; Dr. A. D. McIntosh, associate professor of chemistry; Dr. N. Evans, associate professor of chemistry; Professor Paul T. Lafleur, professor of comparative literature and associate professor of English. Dr. John Brittain, professor of chemistry of New Brunswick University, has been appointed to the chair of nature study in the new Macdonald Agricultural College of Ste. Ann, in affiliation with McGill University.

At the University of Manchester Mr. H. Bateman, at present assistant lecturer in mathematics in the University of Liverpool, has been appointed to the newly-instituted post of reader in mathematical physics. Mr. Bateman was senior wrangler in 1905. Mr. C. A. Edwards, Jr., assistant in the metallurgical department of the National Physical Laboratory, has been appointed demonstrator and research fellow in metallurgy.

Dr. August Bier, of Bonn, has been called to the chair of surgery at Berlin, vacant through the death of Professor von Bergmann.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

SOME PHYSIOLOGICAL VARIATIONS OF PLANTS, AND THEIR GENERAL SIGNIFICANCE[1]

IN a survey of the domain of the biological sciences in recent years, one of the most significant facts is found in the extent to which physiology has invaded those fields of this domain which, in the earlier stages of development, seemed entirely apart from and independent of physiological relations. When species of plants were supposed to have been created at the beginning just as we find them to-day, and to transmit their original characters unchanged to their remotest possible descendants, there was no physiological question as to the variations within species, and none as to the relation of species to each other nor as to the origin of new species. In that view there could be no origin of new species. They were all created at the beginning, and then the Creator rested.

When botany first began to be a science it was merely an attempt to classify plants, that is, to discover the characters of species as they were originally created, to group together those that were most alike and to separate those that were unlike. The characters used in the first attempts at classification were more or less superficial, and systematic botany was merely a study in formal external morphology.

But a change has come; and this change began with the general acceptance, among biologists, of the view that species are not

[1] Presidential address delivered before the Michigan Academy of Science at Ann Arbor, Mich., March 28, 1907.

entities with *necessarily fixed characters.* Even though *some* species of plants have persisted with constant characters ever since their earliest records were inscribed upon the rocks, no biological theory has received more certain confirmation in recent work than the theory that species are even now in process of creation. The creative power is not resting, never has rested. Species are appearing before our eyes. We have only to open them and see. In short, nature has been caught in the act of originating new species.

I refer, of course, to the work of de Vries, who has found among the evening primroses species which, every year, are giving rise to forms among their offspring sufficiently different from their immediate parents to be regarded as elementary species. With some of these new forms the characters which distinguish them from their parents are constant when propagated by seed. This is not to be regarded as the inheritance of characters acquired by the parents of these new elementary species, but rather as the appearance of new characters in the race, not by a gradual modification of parental characters, but by a sudden transformation to which de Vries has given the name *mutation.* These new characters can not be ascribed to the direct influence of external factors on the adult or developing forms in which the new characters appear, since they appear and persist in the same conditions of life in which the parental type is continued. De Vries offers no explanation as to how these new characters are produced, but following his work, MacDougal has succeeded in producing new modifications by artificial means, using as the subject of his experiments species which are closely related to those with which de Vries obtained his notable results. MacDougal injected various substances, radium preparations, sugar solutions, calcium nitrate and zinc sulphate, into the capsules of the plants experimented upon, before the eggs were fertilized by the nuclei from the pollen grain. From the many capsules used, a few furnished seeds which, on planting, produced plants notably different from the type of the parent plant. The flowers of the new type were closely guarded to prevent cross fertilization, and their seeds when planted gave a few plants which conformed in every particular to the new type.

If there is no mistake about MacDougal's results, and I see no reason for supposing there is, at least one very important conclusion seems to be well founded, namely, that in an early stage of development of the plant egg, before it has been fertilized, it may be so profoundly modified that the adult plant resulting from it is decidedly different from what it would have been had the egg not been so modified, and the modifications thus produced are transmitted to the next generation through the seeds. Taking the results of both de Vries and MacDougal, we may conclude that the necessary modification of the egg is sometimes produced in nature, and may also be induced by means under the control of the experimenter.

There is one question which will probably be both affirmed and denied by different biologists for some time in the future. It is this: Are the new types which appear by sudden leaps to be considered new species or only varieties of the parent species. The debate on this question will be all the more acrid and prolonged because of the impossibility of giving a satisfactory definition of the term species.

One ought to ask pardon perhaps for quoting *authority* in science, but a high botanical authority has said that he believes no better definition of species has ever been invented than this: "A species is a perennial successon of like individuals." An equally high botanical authority has said that a species is a judgment. And this also is true. Species and other categories of classification are more or less arbitrary distinctions, made for convenience in classification of our knowledge. Hence in a given case, the question whether two different forms are to be regarded as two different species or not, is in part a matter of individual judgment. If Darwin's view is correct, that new species may originate by the gradual accumulation of exceedingly minute differences, there could be no line of demarcation between species provided we could have all the transitional forms. Only where the transitional forms had disappeared, or the new forms had migrated to a new region, could we have sharp lines of distinction between species. Even in the case where the new form had migrated to a region not occupied by the old, the transitional forms would be disclosed on studying the species in all its range. Distinctions of species in such a case must necessarily be more or less a conventional matter. But if species originate by the sudden production of entirely new characters, that is by mutations, as de Vries believes, then there are no transitional forms connecting the new to the old. The condition in nature in this case would be similar to that in which there has been an extinction of the transitional forms between two different types derived from a common ancestor by gradual modifications. In either case there is room for individual judgment in the delimitation of species, according as the differences between the two types are greater or smaller. We say, "A species is a perennial succession of like individuals." But how nearly alike must they be? No two individuals are exactly alike, and the extreme differences possible between two individuals of the same species may be greater than those between two individuals of different species. In other words, the differences within the species may be greater than the differences between species, as de Vries has pointed out. How then are we to decide whether two individuals comparatively different from each other, and yet alike, belong to the same or different species? It has been found that for any given character the variations within the species may be expressed numerically by an average with deviations, both above and below that average. For instance, the average height of the stem in a given species of plants may be two feet. Most of the plants composing the species may vary only slightly from this average, say from one to three feet. But the greater the number of individuals examined and measured the more certain it becomes that we shall find a few individuals which differ far more widely from the average. In our supposed case we might find that the extreme limits of size were six inches to ten feet, while the average was only two feet. These deviations from the average of the species are called the fluctuating variations. They are largely determined by the external conditions in which the species grows. Professor George Klebs has shown that when plants are subjected to extremes of variation in the external conditions of light, heat, moisture and food supply, the deviations from the average of the fluctuating variations become far greater than are usually found in a state of nature. Klebs's results with Sempervivum were truly remarkable. He produced variations that are not found in a state of nature in the species with which he worked, changes in the color, size and shape of the flower, great variations in length of the stem and its mode of branching, the size, shape and arrangement of the leaves. As the result of this kind of work, carried on for a considerable number of years, Klebs has given us a definition of a species which expresses the dependence of the form of the plant upon the environment. According to Klebs we must say: "To a species belong all individuals which, propagated vegetatively or by self-fertilization, under like external conditions, show the same characters through many generations." If two plants under these conditions show a noticeable difference, they are to be regarded as belonging to two species, even though they have descended from a common ancestor. Gaston Bonnier has shown by experiment that plants transplanted from the region of Fontainebleau near Paris to Toulon in the Mediterranean region show in a few years adaptations both of external form and internal structure which cause them to resemble the species characteristic for the Mediterranean region. The same investigator found similar results on transplanting from the plains to Alpine regions. Knowing the origin of such widely variant forms we do not call them two species, but merely extremes in the fluctuating variations of the species. It is conceivable, however, that nature might perform this same experiment on such a scale and in such ways as to make it difficult or impossible to recognize the common origin of two such different types. In that case the botanical collector or systematist, finding the two types in widely separated regions, would describe them as two species of plants. If the distribution of the species was continuous from one of these extreme regions to the other the connecting intermediate forms would show that we had to do merely with extreme fluctuating variations brought about by extremes in soil, moisture, heat and light. If, however, the geographical continuity of the species had been interrupted in any way, it would be impossible to determine by observation alone that the two extreme types were only fluctuating variations of one species. That could be determined by the experimental method as followed by Klebs and Bonnier. Plant the two types in the same region, grow them under exactly the same conditions, and if after many generations they continue to exhibit constant differences they are to be regarded as two species. On the contrary, if they show the same characters under the same conditions, they are one species. Such a method of determining whether one has a new species or not involves an enormous amount of labor, and a great deal of time. It is not in favor with the systematists who work with the higher plants. Nevertheless, there is an increasing recognition among botanists of the necessity of physiological work even in those fields of research that have in the past been dominated by morphology alone.

Such experiments might help to decide the question whether the so-called alpine species have been constant since the glacial period, as de Vries supposes they must have been, or whether, as seems possible, similar combinations of climatic conditions, operating in widely separated regions such as the alpine region of central Europe and the high latitude of Norway, have produced species of similar form. It does not even seem necessary to assume that the parent species of the alpine forms has been the same in these widely separated regions. De Vries has pointed out that species sometimes overlap by what he calls *transgression variations*. Klebs has shown that in one species of Sempervivum he could produce nearly all the characters found in the other species of the genus. Is it not therefore possible that the continuation of conditions of soil, temperature, moisture and light characteristic of the alpine regions, could produce a type varying about a new average, which lies near one of the extremes of the fluctuating variations of the parent species.

If this new average should be established within the limits of the transgression variations of two species one of which existed in northern Europe and the other in central Europe, we should have the production of similar types, the alpine and arctic type, in widely separated regions and from different parent species. The characters of the new type are not 'fixed' in the sense of being due to inheritance, but only in the sense that they are a response to a particular combination of external factors, and this combination is constant in the given regions. Such a view of the origin of alpine types is not merely of theoretical interest, since the application of the physiological method gives the means of reaching more or less definite conclusions.

De Vries and others have pointed out that the species of the manuals and the systematic botanists are in large part composite or collective species and not simple or elementary species. In his view the latter differ from their parent species by *new* characters, not by modifications of old ones. The new characters are inheritable as soon as they appear, and are not regulated by the external conditions in which the adult plant lives.

If MacDougal's work stands the test of repetition, physiological experiment may open up a new field in investigating the *origin* of species. One method of applying physiological experiment to determining the *limits* of species has just been discussed. But other applications of this method are possible. It is well known that cross fertilization generally takes place only between closely related species of plants, rarely between genera. When attempts are made to cross species remotely related, either the pollen does not grow upon the stigma of the strange species, or fertilization of the egg does not take place, or if seeds develop the resulting hybrid is sterile, not being able to produce seeds for its propagation. What lies at the basis of these physiological differences is still obscure. It is probable that enzymes, toxins or other chemical substances play a part. But whatever the explanation, the fact may be used in determining the nearness or remoteness of the relationship between forms. This possibility has been recognized by many investigators, and biologists have proposed using the degree of fertility of hybrids as the means of distinguishing genera, species, and varieties. Though this has been found not to be reliable in all cases, de Vries has suggested it as a means of distinguishing his elementary species from varieties. If on crossing two forms the resulting hybrid is constant in regard to a given character, when guarded against further crossing, the two forms were different species. But if, on crossing, the descendants of the resulting hybrid followed Mendel's law of hybrids, according to which one fourth of the offspring of the hybrid in each succeeding generation resembled one parent in respect to a given character, one fourth resembles the other parent as regards the corresponding character, while half are like the original hybrid, then the parent forms of the hybrid were one and the same species.

Whatever the limitations of this method in its practical application, the significant fact is the extent to which physiological conceptions have invaded a realm that was purely morphology. We may use the experimental method in studying the origin of new species and varieties. We may apply physiological methods in determining the range of the fluctuating variations within the species. We may use physiological affinities as the test of the degree of relationship existing between different forms found in nature.

The foregoing discussion has had special reference to the higher plants. But among the lower forms of plant life physiological methods are far more applicable, indeed necessary, in determining the characteristics of species. In all that group of plants known as bacteria, species can be distinguished only by physiological means. These organisms are so simple in structure, their morphological characters are so few, it is utterly impossible to classify our knowledge of them even from a systematic point of view without using physiological means as the basis of species distinctions. The most important relations which the bacteria bear to the organic world in general, and to the human race in particular, are physiological in their nature. Some of them have the power of invading the animal body and producing there substances which we call toxins, and which may be so exceedingly poisonous that the result may be fatal in an extraordinarily short time. Fortunately the animal body has the power to vary its ordinary physiological processes in such a way as to produce antitoxins which neutralize the action of the toxins. A given organism may vary in its virulence at different times. An epidemic due to an organism in the so-called attenuated state, produces a mild form of the disease. A given animal or plant may be especially resistant to the toxin of one species of bacteria, as the horse is to diphtheria toxin, or it may be very susceptible to a given toxin, as the human body is to the toxin of tetanus, or lockjaw. Also the same organism shows different powers of resistance, or immunity, at different periods. It is well known that any conditions of life that produce a low state of vitality in a given individual, make that individual far more susceptible to disease, that is, to the toxins of other organisms. Not only are plants and animals susceptible to the toxins produced by other plants and animals, but each organism produces substances which are toxic to itself. This is true not only for the lower organisms, but at the present time a discussion is being carried on as to whether the necessity of the so-called rotation of crops of higher plants is more dependent upon the partial exhaustion of the soil in elements necessary for a given crop, or upon the gradual accumulation in the soil of substances detrimental to the kind of plants that produced them. The physiological variations of organism, and the physiological relations of one kind of organism to another, form a series of the most fascinating as well as the most difficult of biological problems. The small size of the bacteria and the rapidity with which they multiply make them very favorable subjects for experiment along the line of the fundamental biological processes. An organism that requires several hundred years to complete its life cycle is obviously not a favorable subject for an experiment that requires the study of several generations. But if, as in the case of some of the bacteria, a new generation may be produced every fifteen minutes, it is possible to obtain within a few hours hundreds of generations and millions of individuals.

There is another group of organisms about which I wish to speak, not so simple as the bacteria in structure, but far inferior in that respect to the highest plants. I refer to the filamentous fungi, and I wish to call your attention to some facts that again have to do with the question: What is a species? As in the case of higher plants, the first attempts to classify these organisms were upon a purely morphological or structural basis. But a deeper knowledge of their life histories and physiological variations makes it more and more apparent that here, as among the bacteria, it is necessary to use physiological means of distinguishing, shall we say species? For the present we can avoid making the decision, and say forms or races, yet at the same time we can hold our minds open to evidence as to whether these forms or races are not, after all, incipient species. Two groups of these fungi especially force themselves upon our attention from the point of view we are considering. One of the groups has been called the Uredineæ or rust fungi, and the ordinary rust of cultivated cereals is a typical example. The other group is known commonly as the mildews, or more technically the family Erisyphaceæ. The rose mildew and the grape mildew are common examples. In both of these groups it has been found necessary to distinguish what have been called biologic forms or races, distinguished from each other only by the fact that they differ in capacity to infect different species or genera of the host plant. Working with the wheat rust, which was formerly supposed to be the same on any of the cultivated cereals and wild grasses, Eriksson has found that there are numerous races adapted more or less closely to the species of single genera, and they are able to infect species of other genera either with difficulty or not at all. Their forms can not be distinguished morphologically, and yet the infection experiments show that physiologically they are decidedly different from each other. In trying to conceive the origin of these forms, there seem to be three possibilities. First, these biologic forms may have had an origin from different species growing on a narrowly limited group of host plants. There seems to be little evidence for this view. Second, they may have been derived from one species, by sudden physiological changes in the fungus alone, without any influence of the host. This would be similar to the origin of elementary species by mutation, as found by de Vries among the evening primroses. There seems to be no direct evidence for this view. Third, a group of biological forms which can not be distinguished morphologically may have originated from one species which at first grew on a wide range of host plants, but when a strain or race is propagated continuously on the same species of host, there is a special adaptation of the fungus to that species of host, and it becomes able to infect that one more readily, and others less readily, and at last not at all. For this view there is some direct evidence. A form of rust which was capable of growing on four genera of host plants, was propagated for ten years continuously on only one of the four. At the end of the ten-year period it could infect that one genus strongly and the other three weakly or with uncertainty. If this experiment indicates the way in which the biological forms have come into existence, they have originated, not by mutation, but by adaptation. The differences they exhibit have come about by the gradual accumulation of imperceptible modifications.

Among the mildews there has been found an adaptation of forms even closer than among the rusts. Experiments of Salmon on the mildews of grasses disclosed the fact that adaptation is not only to one or few genera, but in many cases actually to one or a few species within the genus. The mildews exhibit the phenomena of adaptation carried much farther than it is carried among the rusts.

The question remains, can these biologic forms or adaptive races ever rise to the dignity of true species? Again the direct evidence is lacking. But if these fungi are as variable in their morphological character as Klebs found even the flowering plants to be under different physiological conditions, we might expect the same causes which bring about the physiological adaption to be able to produce morphological differences as well. But even if no morphological differences appear, are we not justified in making physiological characters the basis of species among the fungi as is already done among the bacteria? The speaker is inclined to answer this question in the affirmative. It seems certain that, for practical purposes at least, it is becoming absolutely necessary in other groups of fungi, as well as in the rusts and mildews, to make distinctions on physiological grounds, not to the exclusion of morphology, but in addition to it. Whether you call the groups of individuals so distinguished species or not, matters very little. The important thing is that the distinction must be made.

It is impossible to apply de Vries's test for species and varieties among the fungi. For most of them there can be no such thing as cross-fertilization. For many there is no fertilization at all, and even where present, it is generally strictly self-fertilization. Naegeli long ago pointed out that where plants were propagated only vegetatively or by self-fertilization and it may be added parthenogenetically, individual peculiarities were perpetuated in the descendants, while with open or cross-fertilization the peculiarities of one individual may be modified in the next generation by mingling with another line of inheritance representing peculiarities of another individual opposed to those of the first. Open or cross-fertilization therefore tends to keep the species homogeneous by neutralizing extreme individual variations. While in those plants which are propagated by parthenogenesis, that is where the eggs develop without fertilization, or by self-fertilization, or by non-sexual spores, or by vegetative means, the species tend to become heterogeneous. They are made up of many lines of descent which are never mingled, individual peculiarities tend to become extreme, and species limits are particularly difficult to determine. Among flowering plants the hawkweeds furnish an example of the results of reproduction by parthenogenesis. In this genus, *Hieracium,* it is said that of two noted men who had made a special study of the species of this genus, neither could identify the species by the other's descriptions. The same result is apparent among the fungi, in the development of the biologic forms or adaptive races.

Individual adaptation to a given host is not neutralized by fertilization from a plant with a different adaptation, but is continually accentuated. The practical importance of many of these adapted forms compels us to recognize them as distinct entities, and to give them names. For practical purposes then they are species, even though they can be distinguished only physiologically.

This capacity for physiological variation or adaptation on the part of fungi is significant in another direction. It is certain that among the fungi as well as among the bacteria, forms that for the most part live only on dead organic matter, that is, as saprophytes, may under certain special conditions become adapted to a parasitic life. They thus become the producers of new diseases. Though for the most part supposed new diseases are only a wider distribution of old diseases, it is entirely possible for new diseases actually to originate by physiological adaptation. This has been proved in the production of plant disease experimentally.

But if this kind of variation has its somber side, there is also an obverse side. Physiological variation enables us in many cases to select and propagate cultivated plants that are particularly resistant, and sometimes completely immune, to a given disease. The same phenomenon may be observed here as in the human family. In any given epidemic there are always certain individuals who never contract the disease. They have a certain natural immunity to that particular disease, and this immunity is due to some physiological peculiarity. So in a field of rusted or mildewed wheat some individual plants show themselves more resistant than their fellows to the species of rust fungus found upon that species of host. By selecting and propagating these immune individuals we may develop an immune race or strain. The problem is not always so simple as here stated. It may happen that a race immune to one disease may be very susceptible to another, or immunity may be accompanied by other qualities altogether undesirable. One might be led to suppose, on reading certain popular articles intended to show how new forms of plants are produced, that it is only necessary to imagine an ideal plant and then set to work to create it. Nothing is farther from the truth than this. Nature does sometimes produce something new, as a stoneless plum, or a nectarine on a peach tree. But man must take the materials furnished by nature, combine them in new ways, or modify them within limits which are usually soon reached. He can not create a wheat plant immune to rust, nor a watermelon resistant to the wilt fungus. But if nature furnishes a few individuals with the desired qualities, man can propagate the individuals possessing those qualities, and by rigid selection maintain the qualities to a high degree. If it is possible to cross the plants with other species or with varieties of the same species, he may be able to combine in the same individual a number of desirable qualities. Having obtained these qualities in one individual, he can best conserve them by vegetative propagation, such as by grafts, cuttings, bulbs or tubers, according to the habit of the plant propagated. He may care nothing whatever about the limits of species or varieties except in so far as their physiological relations help or hinder his combinations. Following MacDougal's method, it may be possible to produce in plants some new characters. But even if it were possible to produce in this way really new species, it is hardly within the range of possibility that we could choose beforehand the kind of a species we would produce. It would be a case of 'cut and try.' If the result be a form with desirable qualities, let it be preserved, but if it be worthless, let it die. Nature has repeated this experiment ten thousand times. If we would imitate her we must search out her secrets in the physiological realm. She conceals them well, but is not unwilling to reveal them to him who questions her with a hearing ear, a seeing eye, and a thinking brain, tools which she herself has given him.

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A PLEA FOR THE STUDY OF THE HISTORY OF MEDICINE AND NATURAL SCIENCES[1]

FOR a number of years a new current of thought has been gradually coming to the front in the minds of scientific thinkers of the times. The nineteenth century, the mental development of which is now assured, has of late been severely criticized for its unhistorical character, and perhaps not without reason. Over this inheritance from the preceding generation a certain dissatisfaction is being more and more keenly felt in the most diverse branches of science. The main trend of the last century was naturalistic and economic to a marked degree; so much so, that the new methods discovered in natural science, and

[1] Read before the American Anthropological Association, at the meeting of the American Association for the Advancement of Science, December 31, 1906.

the vast progress resulting therefrom, seemed to foreshadow an entirely unprecedented epoch in the history of science, and the generation of that age was only too eager to sever all links connecting it with the accomplishments of former ages.

The inauguration of the twentieth century presents a somewhat contrary aspect. One of its primary tendencies has been towards a restoration of our lost connection with the eighteenth century and with earlier periods, resulting in a movement of such earnest and impressive character that we can not foretell at the present moment whether the eighteenth century will not, at some day not far off, seem nearer to us than the sober prose of the nineteenth.

It is not mere chance that at the dawn of the new age the war-cry 'Historical investigations!' is sounded from all camps, and that in consequence a broader scientific knowledge is obtained through this pursuit of historical research. In nearly all lines, students had become weary of the worn and time-honored ruts, and from the dry atmosphere of specialized specializations yearned for the purer air of loftier heights; and not least among the causes of this reaction was the disappointment due to the misapplications and failures of the evolutionary theory. New ideals were thus created, and found their expression in an extended historical movement, which led to radical changes and to amplifications in literary activity, in academic instruction, and in museum policy—or rather in encouraging prognostics of a new museum era—at least, so far as Germany, Austria and Switzerland are concerned. To give a concise idea of what has been accomplished, and is being proposed to be done in this line, is the object of this paper.

To review even hastily all literary pursuits pertaining to this large field is naturally beyond the scope of my purpose. The most noteworthy, in my estimation, are the following: the journal *Zoologische Annalen,* founded in the interests of the history of zoology in 1904 by Max Braun, professor of zoology at the University of Königsberg, and the organization of the Deutsche Gesellschaft für Geschichte der Medizin und der Naturwissenschaften, in Hamburg, on September 25, 1901,—a most active and industrious society, which, now under the able leadership of Professor Karl Sudhoff, of Leipzig, has thus far published six volumes of 'Mitteilungen zur Geschichte der Medizin und Naturwissenschaften.' The pages of this journal are full of interesting original contributions and copious reviews concerning the history of anthropology, botany, zoology, geography, geology, mineralogy, chemistry, physics, mathematics, astronomy, technics, and medicine. A distinguished production of German scholarship is the 'Handbuch der Geschichte der Medizin,' established by Theodor Puschmann, the late celebrated medico-historian of Vienna, and edited by Max Neuburger and Julius Pagel. It was recently completed in three volumes, with thirty-one contributors, and embraces the history of medicine in all its departments and epochs, among all peoples of the globe, inclusive of primitive tribes. Despite its very numerous shortcomings—chiefly due to inaccessibility or want of material, especially on Asiatic medicine, but partially also to lack of historical criticism—it remains, nevertheless, a remarkable monument, but more prospective than retrospective. The recent proposed action of the Berlin Academy of Sciences in regard to the publishing of a complete edition of the Greek medical authors may also be mentioned in this connection; and the new epoch-making researches on the life, personality and works of Theophrastus Paracelsus.

In the academic institutions of Germany and Austria, broad and liberal space is now allotted to instruction and research-work in the history of medicine, natural sciences, and particularly in that of cultural plants and domestic animals. It is an officially acknowledged, fostered and encouraged subject of teaching and study; and there is now hardly any German university, however small, where it would not find a competent representative. Only a year ago (1905) an institute for the study of the history of medicine, in connection with a full professorship, was established at the University of Leipzig, the chair being occupied by Professor Sudhoff, who tells me that thus far there has been an average attendance of from fifteen to twenty-five students in his courses. This institute will regularly issue scientific publications from the beginning of next year (1907).

Berlin has two professors for the history of medicine—Pagel and Schweninger—it having been customary for many years for students of medicine to be allowed to choose their theses from this field, which has been done by many of them with evident success. Regular courses are offered there, besides, in the history of epidemic diseases, of anatomy, of chemistry, of astronomy, of cultural plants. In the last-named subject, four courses are tabulated this winter—one of general character, and three special ones relating to the cultural plants of Africa, and to those of the German colonies and the tropics, respectively. The University of Vienna has likewise two representatives of medical history (Neuburger and v. Töply), general courses and a special course on the history of physiology (Kreidl). Innsbruck possesses a specialist in the history of zoology (v. Dalla-Torre). An *extraordinariat* for the history of medicine has been founded at Würzburg (Helfreich); and courses on the subject are provided for at Bonn, Göttingen, Breslau, Heidelberg, Tübingen, Munich, Marburg, Kiel, Rostock (with even three teachers), further at Graz in Austria; and at Basel, Zürich and Bern in Switzerland.

On November 13, 1906, the cornerstone of the German Museum of Masterpieces of Natural Science and Technics, in Munich, was laid,—the last creation born from this young historical spirit. A question much ventilated now, in the circles of Germany interested, is the plan of a comprehensive museum for the history of medicine, illustrating its development, from the times of prehistoric man down to the present day, in anatomy, surgery, hygiene, endemic diseases and other phases.[2] Such a medical museum, fully deserving of the name, as yet exists nowhere. The medical faculty of the University of Paris moved a resolution to this effect some years ago, but the scheme has not yet been carried out. The only institution that has thus far made any attempt in this direction is the Germanic Museum of Nürnberg, whose very beautiful collections, however, are restricted rather to pharmaceutical than to purely medical antiquities. The first temporary exhibition relating to medical history was held in Düsseldorf in 1898, on the occasion of the annual assembly of the German naturalists and physicians; and similar tendencies developed at the Russian congress of physicians at Moscow in 1900, with greatest success.[3]

[2] See a paper by Sudhoff, 'Zur Grundsteinlegung des Deutschen Museums von Meisterwerken der Naturwissenschaft und Technik,' Begrüssende Gedanken und Ausblicke (reprint from *Münchener Medizinische Wochenschrift*, No. 46, 1906).

[3] Compare report on address by B. Reber, 'Über Notwendigkeit und Wert von Sammlungen betreffend die Geschichte der Medizin,' in report on 78. Versammlung Deutscher Naturforscher und Ärzte (reprint from *Münchener Medizinische Wochenschrift*, No. 47, 1906, p. 8).

I now venture to suggest that such a museum, representing the development of medicine, natural sciences and technics in their whole range, be established in this country, perhaps here in New York, which seems to be the most appropriate place for it; and I am under the strong impression that such an institution would be of wide and universal benefit to our public at large, and would contribute immensely towards the furtherance of science, both natural and historical, and also considerably aid the cause of anthropology. The temporary tuberculosis exhibit in this city last winter may serve as a technical example of what could be accomplished here. If a sufficient number of notable physicians of New York could be interested in the far more extensive plan just proposed—the carrying out of which would not require an exorbitant capital—its realization would seem to be within easy reach. Nothing would be more welcome to us than the sympathetic cooperation of physicians, to interest whom in the study of anthropology we must make many more and larger efforts, especially when we consider how signally anthropology, in its theoretical and practical bearings, has progressed and been advanced by medical men in Europe. One of the foremost tasks of the future American museum devoted to medical science would certainly be to represent the accomplishments of the hygiene, and technical inventions. In this way we should enlist the interest of physicians in our native population; and students of anthropology might also profit from their mode of viewing the subject or from an active participation in our work. A museum of this type, if developed on the broadest lines, may indeed lead also to new and fruitful anthropological work. I need hardly accentuate here the point that a full historical representation of all endemic and the great epidemic diseases (analogous to the idea of the tuberculosis museums), in connection with the development of hygiene, would be a matter of great public service—an undertaking which should meet with the support of all philanthropists. It goes without saying that a museum of this kind would be a scientific, social and educational potency of the highest order—an agency of social progress, not inferior in rank to art or ethnographical museums.

At the same time I may be allowed to express the wish that the study of the history of medicine and the other natural sciences be taken up in this country with the same energy as on the other side of the Atlantic. I need not dwell here on a discussion of the manifold advantages of such pursuits, as the development of all science as an emanation of human culture naturally falls under the head of anthropology.

The most obvious gain which could be derived from the carrying out of these suggestions would be closer affiliation and more intimate contact of all the sciences. In the pursuit of historical investigations, we are all on common ground, and the character of the subject necessitates mutual dependence and assistance. It logically leads to a plea for cooperation, through the efficiency of which many of our most important problems are awaiting their final solution. Allow me to recall to you the study of the history of cultural plants and domestic animals, as constituting the framework of all higher forms of human culture. These topics have engaged the attention of anthropologists to a very limited extent only, being mainly worked up by botanists and zoologists, and occasionally by geographers and economists. The leading books on the subject are little satisfactory from the historical point of view, while historical investigations already in existence suffer from the lack of botanical or zoological accuracy. There is an unharmonious dissonance between these various attempts; and a synthetical representation that should seek to reconcile the conflicting standpoints is still a vain hope. The reason is the isolation of the single sciences, each of which, being restricted to its peculiar resources and methods, is intent on solving a problem in which a goodly number of them are involved. Naturally, only one solution to a problem is possible, whether it be attacked through physical or historical research; and if the results obtained by either are mutually contradictory, this is equivalent to saying that the particular science alone is unable to solve it, and that the solution should be undertaken by a concentration of energy of all the sciences concerned in the specific case. To cite a practical example, take the origin and propagation of our cereals, or the long history of the domestication of the ox or the horse—problems around which, finally, the most ancient history of Asia and Europe centers. There is no science which, by the mere exercise of its own limited faculties, could reach a decisive solution of them; but I am fully confident of ultimate success through a cooperative combination of the various sciences involved, which, in this case, are geology, botany, zoology, archeology, history and anthropology. The individual can not master all these sciences; and, instead of dividing our strength by working singly from isolated positions, we should advocate the uniting of all available forces for the best good of the same cause. The identical observation holds for all historical studies of sciences. The students of Oriental fields, for example, whether their work be in the Egyptian, Arabic, Indian or Chinese departments, are almost daily confronted with the wonderfully rich scientific lore of these peoples referring to subjects in which they themselves are not competent; but it is on the shoulders of these very students that the accumulation of a large portion of the material rests, on which the historian of science can build. One of the most remarkable instances of this sort of cooperation which I have in mind, and which might be extended over many other lines, was the association of the Orientalist Karabacek in Vienna with the naturalist Johann Wiesner, for the investigation of ancient Arabic, Chinese and Turkestan rag-papers, the microscopical and chemical analysis of which confirmed step by step, in minutest details, every result of the history of the invention of rag-paper contributed from Chinese and Arabic sources. The result of their joint labors, carried through many years, I consider one of the greatest triumphs of modern science. But there are many more culture problems of equal importance whose solution must be achieved in a similar manner. Let me refer you only to the history of the invention of gunpowder and of the magnetic compass, both of which are still very obscure in fundamental points, and the working-up of which requires a whole force of well-trained specialists—Arabists, Sanskritists, Sinologues, and men well versed in chemistry, technology, physics and their history.

A study of some of the principal questions in this field is further of profound significance in an interpretation of the methods and results of anthropology. Allow me to exemplify this briefly from the instance of mathematical history. The relation of the concepts of mathematics to the human mind and to the development of culture is still a matter of controversy, and one of burning actuality, just at the present time. A solution on the basis of an historical method is one of the aspects of this problem. The historical position of mathematics, however, is as yet very far from being defined, and no criterions are agreed upon which will admit at the outset of stamping a mathematical thought and theorem as borrowed or independent. The most striking feature in the history of this science is the fact that the same results, even in the highest branches of it, have frequently been obtained by different peoples and at various epochs, with little or no possibility of pointing out an historical connection between such coincidences. The quadrature of the circle, for example, was made the object of correct speculation in China, even in pre-Christian times; or the rule of Horner 'for solving equations of all orders,' established in 1819, was known to the Chinese 520 years earlier, when, in an arithmetical treatise published in 1299, roots were extracted as high as the thirteenth power.[4] Paul Harzer,[5] astronomer at the University of Kiel, last year submitted the mathematical knowledge of ancient Japan to a careful and ingenious examination, and has arrived at the conclusion that the Japanese found spontaneously adequate evaluations of the ratio π, and made the independent discovery of the binomial theorem, which they utilized for obtaining important results. Modern criticism, with its aggressiveness towards the groundwork of human knowledge, towards even that which seems most secure, has recently attacked also the foundations of mathematics, generally looked upon as the most unobjectionable science, and has designated its results, like those of other sciences, as more or less conventional, not necessitated by the nature of the human mind.[6] To us, mathematics is essentially an outcome of human culture; and the question arising from an anthropological view-point is, Are the phenomena of mathematical thoughts to be considered as on an equal footing with those of language, religion or medicine, and, accordingly, capable of methodical anthropological treatment, or are they the particular productions of individual thinkers, and, accordingly, conducive only to an exclusively historical analysis? It is impossible for the present to pronounce a verdict on this intricate problem, though I should like to say tentatively, and with all reserve, that the present state of our knowledge of the mathematics of India, China and Japan would almost seem rather to favor the acceptance of the former theory. At all events, the ventilation of this question is well illustrative of the paramount importance of the study of the history of mathematics and its principal bearings on our views of the intellectual history of man.

The practical proposition which I finally wish to lay before you is, that working committees, cooperative in character, be organized, each consisting of a limited number of members selected equally from students of natural sciences and students of anthropology, especially those in Oriental fields, and pursuing given problems *viribus unitis.* Each of these unions, which need not be of an official character, but may be freely private voluntary alliances of interested students, should be in charge of a particular branch of science. Altogether, seven may be necessary—one for the study of the history of mathematics and astronomy; others for that of cultural plants, domestic animals, physics, chemistry, technology and medicine. Each committee should be so constituted that the united forces of its laborers will represent a consummate systematic knowledge of the subject in question, and take up, suggest, encourage and elaborate pending problems by the concerted action of all its partici-

[4] A. Wylie, 'Jottings on the Science of the Chinese Arithmetic,' in his 'Chinese Researches' (Shanghai, 1897), pp. 163, 184, 185.

[5] Paul Harzer, 'Die exakten Wissenschaften im alten Japan,' Kiel, 1905.

[6] Harzer, *ibid.*, p. 26.

pants. By this method of research, much time and labor would be saved, and more positive and enduring results would be secured.

In concluding, let me call your attention also to the fact that we do not yet possess a history of anthropology, and that broad-minded contributions to the history of our science are an urgent necessity. Goethe has said somewhere that the history of science is the science itself; and I believe, further, that only by a correct appreciation of the development of our science are we able to be just towards our fellow-workers and ourselves. Now that so many of our prominent leaders, like Brinton, Powell, Cushing, Virchow, Bastian and Ratzel, have passed away, whatever we may personally think of the value of their work and its influence upon future generations, it is our duty to come to an objective understanding of their activity and aspirations, and to write the pragmatic history of anthropology in the life and labors of its most conspicuous representatives.

BERTHOLD LAUFER

SCIENTIFIC BOOKS

THE MISSION FOUREAU

Documents Scientifiques de la Mission Saharienne, d'Alger au Congo par le Tchad. Par F. FOUREAU, chef de la mission. IIme fasc., Orographie, Hydrographie, Topographie, Botanique; IIIme fasc., Geologie, Petrographie, Paléontologie, Esquisse Ethnographique, notes sur la faune, Prehistorique, Aperçu Commercial, Conclusions économiques, Glossaire. Index. Atlas. Paris, Masson et Cie. 1905. 4to, 1210 pp., maps and ills.

While the Mediterranean and mid-African colonies of France have been for some time fairly well known, the efforts to connect them by a line of geographical exploration had been rendered ineffectual by the difficulties and dangers of the route through the desert, and the hostility of the natives. Several expeditions met with disaster and were exterminated by the fanatical population.

Finally the expedition organized and carried out by Foureau in 1898 to 1900 met with success. This happy result had been well earned, because Foureau had already given twenty-three years to Saharan exploration under the auspices of the Ministry of Public Instruction. In 1898 his itineraries already amounted to 21,000 kilometers, of which more than 9,000 km. were in previously unexplored country.

In 1894, M. Renoust des Orgeries left to the Société de Géographie a considerable sum of money, to be devoted to the geographical development of the French colonies. Out of this legacy the society devoted 250,000 francs to the purpose of the Sahara Mission, a sum to which the government added not only funds but an escort of 250 picked soldiers under the command of a most competent African officer, Com. Lamy, who fell during an attack by an African chief, in the very moment when the success of the expedition was assured.

With the concurrence of men of science, the commander of the expedition has prepared this report, which by the assistance of government and various scientific societies, is now published in magnificent style by the Société de Géographie, with a preface by Alfred Grandidier.

Together with the reports indicated by our synopsis of the title, the work is replete with well-executed maps, sketches, plans of towns, views and everything which could be of use to future explorers, including minute notes as to the presence, amount and quality of water, pasturage, cultivated land, wild animals, etc. If one is startled by the frequent notation, along the river, of the presence of 'oyster banks,' hundreds of miles from the sea, reflection recalls the colonies of the fresh-water *Ætheria* to which these notes undoubtedly refer.

The mass of information in this encyclopedic work, it is, of course, impossible to summarize. A few notes may, however, have interest for the reader. While a large portion of the herbarium suffered from termites and the wreck of a canoe, nevertheless a good number of plants are recorded; and numerous small photographs reproduced in half tones in the text give excellent ideas of the manner in which many of the trees and shrubs appear in their natural state. M. Bonnet has made the most of the botanical material which was saved, and the record of native names for the various plants is remarkably full.

The expedition started from Biskra and traveled almost due south about 1,260 geographical miles to the settlement of Zinder, in the Damergou country, when they diverged to the east until Lake Chad was reached, passing round it by the north and east, whence they followed up the river Chari in a southeasterly direction, crossing from its headwaters to the Obangwe, an affluent of the Congo, down which they proceeded to the sea. The rocks encountered in the northern Sahara were largely: first, cretaceous limestones of Cenomanian age; then Devonian and a little Silurian, more or less interspersed with crystalline schists. In the middle of the Sahara the schists and granites prevailed, mitigated by patches of volcanic character. To the southeast of Lake Chad, more volcanics were encountered, and, on the upper reaches of the Chari River the schists, granites and diorites again occupied the field. Excellent views of the rocky and sandy desert with its characteristic dunes abound in the text. The first discovery and recognition of Silurian rocks, with *Climacograptus,* appears to be due to this expedition. M. Emile Haug has carefully worked out the Paleozoic fossils as well as the remains of some Cretaceous vertebrates, Selachians, fishes and reptiles, including two new species of *Ceratodus,* referable to the Albian.

The native Tuaregs of the northern Sahara are well described by Foureau, who shows how their predatory habits naturally result from the state of semi-starvation in which they exist, and which leads them to travel even 700–800 kilometers to make a raid, of which the chief result for them at best can be but the securing of a few camels and a few full meals of meat. They have an alphabet and system of writing of their own, but very few of them, those chiefly among the women, know how to use it. The correspondence and business of the most wealthy among them is done chiefly in Arabic, by Arab scribes from Touat, who act as secretaries. Those who can write are fond of inscribing the characters on rocks; and few are the suitable blocks of smooth stone in the vicinity of their trails which do not bear some inscription. They are great and fluent liars, but among themselves do not steal, and, within the tribe, observe the law of the cache. They are monogamists, and their women enjoy great liberty, and possess powerful influence which they seem not to abuse.

The Tuaregs dress in voluminous garments. They cover the whole body and head, and veil the lower part of the face, usually with a strip of blue cotton, so that the eyes alone are visible. This practise, perhaps originally adopted to shield the skin from the ardent reflections from the arid soil, has become almost a religious observance. The influx of slaves and wives from the negro races to the south, the Arab and Egyptian blood derived from the east, have made these people almost incredibly mixed in blood. Careless in their observation of the rules of the Koran, regarded almost as infidels by the orthodox Mohammedan, they are, when it is a question between them and strangers, fanatically bigoted Moslems.

M. Hanay has devoted much consideration to the prehistoric remains of the stone age collected by the expedition, many of which are illustrated by admirable heliotypes. In his summary the commander of the expedition pleads for the native people that they should be allowed to practise the rites of their religion in peace; and has very little to say in favor of the results accomplished by the French missionaries, in the regions to which the latter have had access.

On the whole the work is a mine of material for the geographer, geologist, naturalist and ethnologist, and reflects the highest appreciation of the labors of those who have succeeded in bringing so arduous an expedition to a happy and successful conclusion.

W. H. Dall

The Crawfishes of the State of Pennsylvania. By ARNOLD E. ORTMANN, Ph.D. Memoirs of the Carnegie Museum, Vol. II., No. 10.

There are two groups of animals which seem destined, on account of their abundance, extensive North American range, plasticity and educational utility, to become classics in the literature of American zoology. One of these groups is the Garter Snakes (*Thamnophis*) and the other the Crawfishes (*Cambarus*). The importance and utility of these groups does not seem to be generally recognized. It is therefore of special importance that Ortmann's recent comprehensive treatment of the Pennsylvania species of crawfishes should be generally known.

In his introduction Ortmann mentions his extensive explorations of the state, in which he traveled over 11,000 miles, and discusses his methods of collecting and finally gives a historical summary of the Pennsylvania species. This section is followed by the taxonomical and chorological portion of the paper, which includes a key to the species, with detailed description of each, full records of the occurrence of each species in the state and elsewhere, and a critical summary of the geographic range of each species. The detailed character of these records makes the work of special value to students of other localities as well as those of Pennsylvania.

The section devoted to ecology and geographic distribution discusses the subject of habitat preference, geographic origin and distribution, ecological relations (burrows, chimneys, etc.), and shows that there are three types of habitat preferences: (1) Those frequenting the larger rivers; (2) those frequenting small streams; and, (3) those frequenting springs and swamps. The river species are *Cambarus limosus, propinquus* and *obscurus.* These species are not completely restricted to these conditions, but occur also in any large permanent body of water, a habitat rarely found in Pennsylvania. The small or mountain-stream species is *C. bartoni.*[1] This form avoids large streams and tends to push up stream into the head-waters, and further shows a preference for cool waters. The spring and swamp or burrowing species are—*C. carolinus, monongalensis* and *diogenes.* These species, says Ortmann, are always found at a certain distance from the open water, although often in close proximity to streams, ditches and ponds; but not in them under normal conditions. Ground water seems to be a dominant factor in their environment. It may be suggested that the amount of oxygen and carbon dioxide in ground waters, as well as a low temperature, may be intimately related to the habitat preference of these species. (This would be an interesting problem for an experimental study.) The haunts of the various species are discussed, including the shape of the burrows and the construction of the chimneys. The deeper parts furnish winter, and the upper parts summer quarters. The chimney is simply the by-product of the burrow.

The geographic relations of each species are summarized and discussed in detail; not only with regard to origin in the state, but also their general geographic origin and migration routes. The determination of these routes has required a knowledge of the history of the streams or drainage modifications in Pennsylvania and the adjacent regions. The original distribution of *limosus* seems to have been influenced by canals and mine drainage. This species is of special interest because it belongs to an isolated group both morphologically and geographically. The allied species occur in Missouri, Illinois, Kentucky, etc., and its most apparent route is through pre-glacial streams in the now glaciated region; while a northward route, from the south along the Coastal Plain is dismissed as unlikely. Ortmann concludes that *limosus* is a Tertiary type which was driven south along the Atlantic Coastal Plain, and within the interior, into Indiana and Kentucky, while the species was exterminated from the intervening region. It might be suggested that the preservation of this group of species at the extreme northern part of the Coastal Plain on the Atlantic coast, and the upper part of the Missisippi Embayment

[1] In southern Michigan (Ann Arbor) this species is abundant in small brooks flowing through open meadows and thus illustrates the habitat variability of this species.

may indicate a former southern, rather than northern connection. The southern route would give a continuous lowland habitat, and although it might mean competition with related species now, it does not follow that formerly there was such competition. Geologically speaking, both the glaciated region and the Coastal Plain are recently populated areas.

The geographic relations of *C. propinquus* and var. *sanborni*, and *obscurus* are discussed as a unit on account of their close affinities. Their history is quite involved on account of the complex histories of the streams they occupy. These closely related forms are representative of different parts of the Ohio system: *obscurus* of the upper Ohio, *propinquus sanborni* of the middle, and *propinquus* of the lower Ohio drainage. These respective sections of the Ohio are considered by physiographers as formerly parts of independent drainage systems which later became fused to form the Ohio, and consequently there was an opportunity for differentiation while the streams were separated. Even now while occupying the same system they show only a limited tendency to fuse. Their glacial preserve was apparently in the more south-central parts of the Ohio system, not far from the ice margin.

The species *bartoni* has the most extensive range in Pennsylvania, which is in decided harmony with its preference for small, rapid, and cool streams—such as abound in the Appalachians. This is apparently a preglacial species which has extended its range in postglacial times to the northeast along the Appalachians, across various drainage lines—rather than along them. This is probably due to its tendency to frequent head waters, where divides themselves frequently migrate, and on account of the tendency and ability of this species to wander overland and thus 'migrate' the divides and around obstacles in streams, such as cascades or low falls. Such facts as these clearly emphasize the need of a detailed knowledge of the ecological relations of such animals before due weight can be given to the biological evidence suggesting physiographic changes.

This species becomes dwarfed in eastern Pennsylvania, a fact of much interest. There are so many conditions which may cause dwarfing that it would be of considerable interest to know what conditions have been operative here. The variety *bartoni robustus* is larger than the typical form and in Pennsylvania is confined to the extreme northwestern part, where the two forms occur together or separately. The general relations might be expressed thus: the largest form (var. *robustus*) occurs in the extreme northwestern part of the state, associated with or separately from a smaller form (*bartoni*) which alone occupies the intermediate eastern area of the state, and a still smaller or dwarfed form occupies the eastern part of the state. Thus there is a more or less progressive dwarfing to the eastward, if these forms prove to be intimately related.

C. carolinensis is restricted to the southwestern part of Pennsylvania, is a species of southern Appalachian range, and appears to prefer the higher altitude and clay soil of the Old Tertiary baselevel.

C. monongalensis also occurs only in the southwestern part of the state. This is a subterranean or burrowing species, even avoiding small streams, and thus it is not remarkable that the Ohio and Allegheny rivers have proved a barrier to its northward extension. The Youghiogheny and Monongahela rivers seem to have been traversed indirectly by the migration of a divide during the glacial period.

The range of *diogenes* is exceptional in that it occupies a narrow strip of the Coastal Plain along the Delaware river, is absent from all the central portion of the state, and occurs again in the southwestern part. Such discontinuity certainly suggests divergence as to origin. Ortmann is inclined to consider this species of Allegheny Plateau origin. A comparison made between the present range of *diogenes* and *limosus* shows that both exhibit a discontinuity of range between the eastern and western parts of the state, and both occupy the Coastal Plain. These facts suggest to Ortmann a retreat from the north before the glacial ice. Here again it may be suggested that detailed investigation of the Coastal Plain may favor the hypothesis of a northward extension from the south. Such a possibility may be favored by a route across the Appalachians which seems to have been very generally overlooked as a highway—the Kanawha River Route—which reaches south into North Carolina. This is a stream with a remarkable history whose biological significance is worthy of detailed investigation, not only from the standpoint of aquatic invertebrates, but also of higher groups as well. (This is perhaps particularly true of birds.) In view of the antiquity of this route it seems very improbable that animals should ignore it.

In considering the crawfish characteristic of the natural physiographic regions of the state certain interesting correlations are evident. The Coastal Plain possesses two species, *diogenes* and *limosus;* the Piedmont Plateau and Great Allegheny Valley, which form a unit, are characterized by *limosus* and *bartoni;* the Allegheny Mountains by *bartoni,* with an invasion along the Susquehanna by *limosus* and an isolated colony of *obscurus*—both are due, Ortmann thinks, to the influence of man. A map of the state showing the location of past and present canals would greatly aid one in understanding the extent of this influence. The Allegheny Plateau, on account of its proximity and drainage into the great interior glacial preserve, has the most varied crawfish fauna—six species: *propinquus, obscurus, carolinensis, monongalensis, diogenes* and *bartoni.* It is thus seen that most of the species have invaded the state from the westward and that there is a marked attenuation of the fauna to the eastward.

The life histories of the species are considered in detail, the seasonal life history may be outlined as follows; a fall mating, a spring spawning season, an early summer season when the males are in the first form. The species which follow this cycle include, provisionally, *obscurus, propinquus* and var. *sanborni, limosus* and *diogenes.* These forms comprise what Ortmann calls the 'warm water' type and have a restricted breeding season. A second class includes those species which breed and spawn practically the year around, the 'cool water' type, and includes *bartoni, monongalensis* and probably *carolinensis.*

In *obscurus,* eggs are laid in April, hatch in May or June, and by September or October the crawfish are 40 to 55 mm. long, and the males are in the first form, the females are mature and copulation takes place from September to November. The winter is passed without change, and in April the females spawn and in June moult, the males having moulted in May. The fall moult ranges from August to October, and a second breeding season follows, and in the following year a third, after which they survive the winter, the males dying perhaps in early spring, and the females in June. They may thus reach the age of three years.

On account of the almost continuous breeding season of the 'cool water' species it is difficult to recognize broods and thus determine the detailed life history.

At present the known economic relations of crawfish are rather limited. They are used as food to a limited degree, especially about cities, but are very generally used as bait, hence the confusion which fishermen are likely to introduce into the problem of the original range of species. As scavengers they may be very beneficial, and occasionally dams may be injured, but these subjects have received but little attention by the public or naturalists. Their enemies include a wide range of animals, especially among the vertebrates.

The final section of Ortmann's paper is devoted to a discussion of the relation which this study of Pennsylvania species bears to the problems of evolution; mutation and isolation in particular. Ortmann criticizes de Vries's statement that species are formed only by mutation. He then considers in detail the evidence for marked variation or mutation among the Pennsylvania species and concludes that the differences between them are very constant and the variations slight, adding that "Anything that looks like a 'mutation' in de Vries's sense is entirely unknown." Even the differences between well-defined species "are so slight that they can not be regarded as representing 'mutations,' that is to say, sudden leaps in a progressive direction." Rather than mutation Ortmann looks to isolation as the important factor and summarizes his position as follows:

> 1. The normal case is when two closely allied species, possessing identical or nearly identical ecological habits occupy separated areas, which lie close together but do not overlap. * * *
>
> 2. Whenever allied species are found in one and the same locality (overlapping), isolation becomes apparent in the following forms:
>
> (*a*) The two species have different centers of origin, that is to say, they were separated formerly, but occupied the same territory subsequently. * * *
>
> (*b*) If the centers of origin are more or less identical (absolute identity is hardly possible), the two species always differ ecologically, and although living at the same localities, prefer different surroundings. * * *

So much for the general results of Ortmann's investigations, but such an outline does not throw into the foreground some of the features of the work which deserve special mention. This paper is devoted to the fauna of a limited area, and contains several of the elements which go to make up an ideal treatment of a local fauna. Not only is there an abundance of detailed facts, supplemented by a critical review of former records, but a very serious attempt is made to understand the meaning or significance of the details presented. Thus Ortmann's efforts to *interpret* and *correlate* the mass of facts is particularly commendable and is a marked departure from the usual annotated lists. He sees clearly that many of the facts to be explained involve a knowledge of the conditions which formerly existed and consequently he turns to a study of the determining conditions. The present work is also an important lesson on the value of a knowledge of the ecological relations of animals in the study of their relationships and geographical distribution, and clearly illustrates the difference between the older 'orthodox' zoogeography and the newer ecological phase.

Some suggestions may be mentioned which offer opportunities for improvement or expansion in future work. While the details of the environment of the burrowing species are described fully, the same information is needed for the brook and river habitats; and such descriptions can be supplemented to advantage by photographs of representative situations. We need detailed maps showing the occurrence of crawfishes, in a manner similar to the mapping of plant societies by ecological botanists. The composite system of mapping used is not very satisfactory and would have been greatly improved had fewer species been placed on the map, and still more, if topographic maps had been used as a base. County and stream names are of great advantage on maps devoted to detailed faunal studies, and especially when the details of distribution are not expressed on the maps.

Ortmann does not seem to recognize, in more than a general way, the need of formulating the conditions which compose the most favorable (optimum) habitat, so that throughout the geographic range of a species the modifications of the habitat may be followed as definitely as structural and functional modifications (habits, etc.), so that eventually perhaps such relations may be correlated. In this connection it should be mentioned that the ecological observations are very largely from western Pennsylvania and should be extended over a larger area. But to do this the necessary time for *field study* as well as for *collecting* must be available. The importance of the laws of habit, habitat and environmental change is so great that it is very desirable that field students and institutional authorities see the real importance of these studies.

While reading this memoir a need has been felt for definite criteria by the aid of which some estimate could be made of the degree of primitiveness or specialization of forms. Further, if the general bearing of mutation is to be tested or used to the best advantage in the interpretation of the problems of habits, habitats and geographical distribution (of both plants and animals), it seems necessary to formulate criteria by means of which mutations can be recognized in nature (at least with some degree of probability), without recourse to pedigree cultures. Here, as when attempting to determine centers of origin, great care is necessary to avoid arguing in a circle.

Much confusion, both to the students of distribution and of pedigree cultures, has resulted from this lack of formulated criteria. Some may question the possibility of such criteria.

After such detailed studies, in which special emphasis has been placed upon geographic origin, one naturally expects certain criteria, perhaps more or less peculiar to the ecological relations of crawfishes, to be formulated, but such are not stated.

Two important papers should perhaps be mentioned in this connection, as they are not listed in the bibliography: Harris, 'An Ecological Catalogue of the Crayfishes belonging to Genus Cambarus' (*Kans. Univ. Sci. Bull.*, Vol. II., pp. 51–187, 1903), and Steele, 'The Crayfish of Missouri' (*Univ. of Cincinanti Bull.*, No. X., pp. 1–54, 1902).

A certain amount of statistical data could have been used to advantage. As this method of measuring variation, used with judgment and moderation upon critical phases and at critical localities, will aid such investigations. For example, if representative lots of *bartoni* from the northwestern, central and eastern parts of the state had been measured, the rate of dwarfing could have been determined. A similar comparison between the western and eastern variation of *diogenes* would be of value. Such variations as these are very common and signify to some that very frequently the *species is too large a unit* for the study of geographic distribution, that local variations or races are of great importance and that in further investigation the forms peculiar to definite habitats should receive recognition and detailed investigation.

In conclusion it should be said that such excellent work, perhaps the most important general zoological work yet published by the Carnegie Museum, should be continued, as the subject has reached such a degree of development that to stop now would be unfortunate, to say the least. The region to the south and west should now be considered, not only because of its proximity to Pittsburg, but primarily because it is apparently in that direction that a most wonderful evolution of crawfishes has taken place, or is taking place. Then with the modification of original conditions through the 'improvement' of streams for navigation, water power and supplies, the construction of canals, contamination by industrial refuse and sewage, we have additional urgent reasons for an early continuation of such investigations so that 'vanishing data' will be preserved for future generations. The Carnegie Museum is not a provincial institution, and does not necessarily limit its activities to the state of Pennsylvania, and it is hoped that this work will be continued, as the present study has clearly shown that the most important part of the problem still awaits detailed investigation.

CHAS. C. ADAMS
UNIVERSITY OF CINCINNATI,
CINCINNATI, OHIO

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Comparative Neurology and Psychology for March contains a report of the convocation week meetings held in New York city during the winter, including abstracts of most of the papers read before the various societies in the fields of neurology and animal behavior. The leading article is a memoir on 'Light Reactions of Volvox,' by S. O. Mast. The light reactions were studied under rigidly controlled conditions in the 'light grader' devised by the author. Among other results, it was found that the direction of motion in *Volvox* is regulated by the relative light intensity on opposite sides of the colony regardless of the ray direction. Orientation is not the result of 'trial and error' reactions, as in *Stentor, Euglena* and other forms. *Volvox* colonies make no errors in this process. There is no evidence of motor reaction in a *Volvox* colony, taken as a whole. Orientation is, however, brought about by motor reactions in the individuals which constitute the colony. Weber's law holds approximately for the light reactions of *Volvox.*

The American Naturalist for April has for its leading article a discussion of 'The Geographic Distribution of Closely Related Species,' by Robert G. Leavitt. The question is considered from a botanical standpoint and the author's conservatively stated conclusions are in favor of the mutation theory of the origin of plant species. 'The Coincident Distribution of Related Species of Pelagic Organisms as illustrated by the Chætognatha' is by Charles A. Kofoid, who shows that there is a tendency for two species of a genus of this group to occur in one locality and not elsewhere, and considers that this casts some doubt on the universality of operation of isolation in the evolution of species. E. A. Andrews describes at some length 'The Attached Young of the Crayfish, *Cambarus clarkii* and *Cambarus diogenes*' and considers their bearing on the question of the evolution of the species.

The American Museum Journal for May is mainly devoted to an article by Clark Wissler on 'The Douglas African Collection' recently acquired by the museum through the generosity of some of its friends.

The Bulletin of the Charleston Museum for April contains an account, by Ezra Brainerd, of 'A Visit to the Grave of Thomas Walter,' one of the earliest of American botanists and the author of Flora Carolina. A pleasant result of this visit has been the taking of steps for the preservation and protection of the grave.

The Peabody Museum of Natural History, Yale University, has just issued Guide No. 1 on 'The Evolution of the Horse Family,' by Richard S. Lull, and based on the valuable material mainly brought together by Professor Marsh, and recently admirably arranged and labeled by Dr. Lull.

SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

The seventh regular meeting of the session of 1906–'07 was held at the Chemists' Club, 108 West 55th Street, on May 10.

The following papers were presented:

The Causes of the Corrosion of Iron and Steel: W. H. Walker.

With the ever-increasing use of iron and steel, the conditions which limit the life of structures made from these materials, assume great importance. To few subjects have been devoted so much elaborate investigation with such conflicting results. Of the many papers which have been published and theories advanced as to the cause of corrosion, the three following are of special importance: Calvert, after a series of experiments came to the conclusion that ordinary corrosion or rusting of iron could take place only when all of the three reagents, carbon dioxide, water and oxygen were present. This opinion was universally accepted until in 1903 Whitney showed that corrosion was a purely electrochemical phenomenon and would take place in water in the absence of both oxygen and carbon dioxide, although for the formation of the so-called rust, oxygen was necessary. A year or so later Dunstan and his co-workers published the results of their work from which they concluded that Whitney was at fault and that iron was not corroded by water in the absence of oxygen and carbon dioxide. They believed the action of oxygen on iron to be a direct one, with the intermediate formation of hydrogen peroxide, and that in ordinary corrosion electrochemistry does not play a part. Following this paper came one from Moody, of Kensington, England, who took issue with both Whitney and Dunstan and describes experiments which he thinks conclusively prove that no corrosion of any kind takes place in the total absence of carbon dioxide.

Work recently carried on at the Institute of Technology substantiates Whitney's claim in so far that there is a slight corrosion of iron in pure water although if oxygen and carbon dioxide be most carefully eliminated, the presence of dissolved iron can be detected only with the greatest care, and possibly if these two constituents were absolutely removed, no iron would be dissolved. There is a tendency, however, for iron to pass into solution and for hydrogen to precipitate out in a way analogous to the action of iron in a copper sulphate solution. Unless oxygen or some other substance be present to unite with the hydrogen when set free upon the surface of the iron, the action, if it starts at all, very soon ceases. To remove this hydrogen and thus accelerate the action is the function of oxygen in corrosion.

Experiments show that the rapidity of corrosion is directly proportional to the partial pressure of the oxygen in the atmosphere above the water containing the iron and therefore in the water. A reagent which indicates very clearly those portions of the iron at which iron ions are passing into solution, on the one hand, and where hydrogen is passing out of solution with the formation of hydroxyl, on the other hand, was found in ordinary tap-water containing a little phenolphthalein and potassium ferricyanide. The red and blue zones are quickly apparent when any piece of iron is immersed in this solution, and can be rendered more or less permanent if the solution be thickened with gelatin or agar-agar.

The potential difference which has often been observed between iron and iron oxide, is shown to be occasioned by the unequal condensation of oxygen upon the two surfaces. For example, magnetic oxide of iron which, under ordinary circumstances in connection with iron, shows a large difference of potential in water or weak electrolyte, indicates no difference when the system is entirely free from oxygen.

Differences of potential which can easily be observed upon different portions of an iron plate, may be also explained by the varying capacity of different portions of the iron for occluding or segregating oxygen.

The electrochemical theory is substantiated by showing that any reagent which increases the concentration of the hydrogen ions will increase the rate of corrosion, while reagents which decrease this, inhibit corrosion. The fact that bichromate and chromic acid inhibit rusting, may be explained by the formation of an enclosing film of oxygen evenly distributed over the surface in a way analogous to that which may be formed by immersion in nitric acid.

The Analysis of Chlorides and Sulphocyanate Mixtures: M. A. ROSANOFF and ARTHUR E. HILL.

To analyze chloride and sulphocyanate mixtures is to-day a difficult matter. The known gravimetric methods are laborious. The known volumetric methods are but little more rapid and far from precise. The authors have devised a new volumetric method, which is easy of execution and yields results of the highest precision. Its basal facts are as follows: (1) At the temperature of boiling water soluble sulphocyanates are readily oxidized by small quantities of nitric acid; (2) *most* of the hydrocyanic acid produced can be expelled in a short time by boiling; (3) no hydrochloric acid is lost, owing to complete electrolytic dissociation; (4) silver cyanide is somewhat soluble, silver chloride is insoluble, in moderately dilute nitric acid.

The method can be used to determine chlorides in the presence of both sulphocyanates and cyanides, and the authors are endeavoring to extend it to the determination of bromides. Details of the *modus operandi* will shortly appear in the *Journal of the American Chemical Society.*

Gasoline-Soap 'Emulsions' and their Relation to Sewer Explosions: A. A. BRENEMAN.

The 'emulsion' produced by shaking up gasoline or benzine with soap solutions gathers slowly upon the surface of the aqueous liquid forming a thick, creamy paste which is very permanent in closed vessels but disintegrates rapidly on exposure to air by volatilization of the hydrocarbon. The ordinary operation of washing the hands with soap after benzine or gasoline has been used to remove grease, carries off the light liquid as an emulsion. This same emulsion can be made in quantity by shaking up gasoline with a weak soap solution in a stoppered glass cylinder. It then rises in an hour or less to form a thick, white, creamy layer which can be drawn off and kept separately. In this condition it is very permanent, requiring many days to effect an appreciable further separation. Under the microscope it shows a mass of air bubbles studded or coated with minute globules of gasoline. The air within the bubbles is saturated with vapor of gasoline, the soap solution is indifferent to it, and vapor pressure is at an equilibrium throughout the system. The permanence of the mass in a closed vessel is therefore to be expected. In the open air it disintegrates rapidly, giving off gasoline vapor. The entrance of this material into drains and sewers where gasoline and soap are used for washing, as in garrages and factories, is sufficient to account for the liberation of much combustible vapor and hence, perhaps, for explosions. Such material separates slowly and is difficult to trap.

Professor Breneman also read two 'laboratory notes,' one relating to the magnetic quality of magnetic (iron) oxide in the hydrated state, and one upon the use of ether in the ferric sulphocyanate test.

C. M. Joyce,
Secretary

THE TORREY BOTANICAL CLUB

The club was called to order on February 27, 1907, at 3:30 P.M., at the Museum Building of the New York Botanical Garden, with Dr. William A. Murrill in the chair. Twenty-one persons were present.

The following scientific program was presented:

Tubular Glands in the Corn Embryo: C. Stuart Gager.

The literature dealing with the transformation of starch to sugar in the corn grain during germination was first briefly reviewed, and its bearing on the structural anomaly subsequently described was pointed out. This anomaly consisted of invaginations of the glandular epithelium of the scutellum into the tissue of the latter, in such a way as to form true glands of the tubular and subracemose type.

The significance of these glands, as in harmony with the theory that the scutellar epithelium is principally an organ of secretion, was also indicated. The paper was illustrated by microscopic preparations and photomicrographs, and will be published in full in the *Bulletin* of the club for March, 1907.

A brief discussion followed.

Explorations in Southern Florida: John K. Small.

The exploration was confined to the larger group of islands lying between Miami and Camps Longview and Jackson, and to a wholly unexplored section of the everglades lying between the present terminus of the Florida East Coast Railway and Key Largo, including a portion of Cross Key. This latter island, together with a parallel and almost similar formation, constitutes the only natural and approximately complete land-connection between the Florida Keys and the mainland of the peninsula. The chain of everglade keys is a miniature of the Florida Keys, both in its crescent shape and its flora, and also of the West Indies in the character of its vegetation. It is surrounded by the everglades, except where the upper islands touch Biscayne Bay at points from Miami to Cutler. Before these islands were elevated to their present altitude, they were probably surrounded by a shallow sea, just as the Florida Keys are at the present time. This being the case, the tropical American flora now inhabiting them may easily be accounted for. After sufficient elevation had taken place, the surrounding sea was transformed into the vast spring now known as the everglades. Conditions becoming favorable, the plants of the flora of northern peninsular Florida advanced southward and naturally took complete possession of the area that was formerly the sea, thus surrounding and isolating the wholly different flora of the islands. In fact, the two floras are so sharply delimited that one can often stand with one foot on plants characteristic of the high northern regions and the other on plants restricted to the tropics. It is not an uncommon experience to see colonies of plants common in Canada, such as the arrowarum (*Peltandra*), the lizard's tail (*Saururus*) and the ground-nut (*Apios*), growing side by side with tropical palms, cycads, orchids and bromeliads.

The total area of these islands is perhaps about one hundred and fifty square miles. Those that have been explored have yielded between five and six hundred species of native flowering plants, surely a very large number considering the fact that the solid rock is exposed everywhere and that soil in the ordinary sense of the word does not occur there. The close relationship of this flora to that of the West Indies is now established by the fact that considerably more than one half of the species found on the islands south of Miami are also native in Cuba and the Bahamas.

Since the publication of Dr. Small's last report on exploration in southern Florida, and a subsequently printed paper on the species added to the flora of that state, he has secured over fifty more species not before known to grow on the North American mainland. Eight or ten of these are complete novelties, inasmuch as they are not yet described. Noteworthy among the recent collections, which make an aggregate of 3,200 specimens, are seven species not previously included in the tree flora of the United States.

After an interesting discussion of Dr. Small's paper the club adjourned at five o'clock.

C. STUART GAGER,
Secretary

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 430th meeting was held April 20, 1907, with President Stejneger presiding.

The first paper, by Mr. George B. Morse, was entitled 'Preliminary Observations on the Quail Disease in the United States.'

The speaker quoted from a booklet entitled 'Quail Culture from A to Z,' published in 1905; "There is no contagious disease among quail that has yet made its appearance. * * * They have lice, but not disease." The facts recorded in these observations are a complete refutation of that statement. In April, 1906, there were received from a Washington dealer three dead bobwhites, the last of a large number that had been steadily falling victims to a highly contagious and rapidly fatal disease. In May, 1906, and January, 1907, letters were received from Boston, Mass., and Worcester, Mass., referring to what was undoubtedly the same disease. From February 11, 1907, to March 21, 1907, two dealers in Washington lost upwards of 250 bobwhites, and quite a number each of several other species of quail. Post-mortem examination revealed the same lesions in all. The sources from which these birds were received demonstrated as known centers of infection Alexander City and Dadeville, Tallapoosa County, and Birmingham, Jefferson County, Ala.; Wichita, Kans.; and Marlow, Chickasaw Nation, Ind. T. In addition to the above, other localities such as Washington, D. C., Boston and Worcester, Mass., Elizabeth, Pa., and Yarmouth, Nova Scotia, have become more or less infected by means of shipments of diseased birds received. The disease has been thus far demonstrated in the following species: bobwhite (*Colinus virginianus*), California quail (*Lophortyx californicus vallicola*), Gambel quail (*Lophortyx gambeli*), mountain quail (*Oreortyx pictus*), scaled quail, 'cotton-top' or blue quail (*Callipepla squamata*) and the sharp-tailed grouse (*Pediocætes phasianellus campestris*).

Period of incubation appears to be about ten days. Symptoms are: dullness, fluffed feathers, neglect of food. In acute cases (the most common) death occurs within two or three days. In chronic cases diarrhœa occurs and emaciation is extreme. At post-mortem examination the characteristic lesions are pulmonary congestion, superficial necroses of the liver and intestinal ulceration. Bacteriologic investigation of the cases studied in 1906 resulted in the isolation of a bacillus apparently identical with Klein's bacillus of grouse disease. The cases studied in 1907 yielded with striking unanimity a variant of *Bacillus coli* with which the author has produced death in mice, guinea-pigs and bobwhites with the characteristic lesions. The disease was therefore spoken of as an infectious disease of the grouse family produced by a member of the *B. coli* group, described in circular No. 109, of the Bureau of Animal Industry. No curative treatment was offered but procedures for prevention were outlined, methods applicable to the prevention of disease of intestinal origin among all wild birds brought under habits of life more restricted than those normally enjoyed.

Dr. T. S. Palmer referred to the importance of the establishment of this disease among American quail, as the grouse disease is established in Europe. When first heard of last autumn it was supposed the grouse disease had been imported. During the past ten years there has been a marked decrease in abundance of quail, particularly following severe winters and there is a large demand for birds to restock, reaching from 100,000 to 200,000 annually. Shipments from the supply centers, as Texas, Indian Territory and Alabama, are liable to be centers of quail disease infection, and in this case shipments may be discontinued by law.

The second paper was by Dr. F. V. Coville on 'Photographic Reproduction of Rare Botanical Books.' He referred to the desirability of having reproductions of rare and valuable works to which frequent reference is made, in order to preserve the originals. These were made by photographing each page and binding the resulting prints into a book. Specimens of such reproductions were exhibited. These had been made for actual use in botanical work, some of them so closely simulating the original as to scarcely show they were photographs. Pages yellowed by age, however, show black or dark in the reproduction.

The third paper, by Mr. R. E. C. Stearns on 'The Composition and Decomposition of Fresh Water Mussel Shells, with Notes and Queries,' was read by Dr. Palmer in the absence of the author. It will be published in full in the *Proceedings* of the society.

M. C. Marsh,
Recording Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The 634th meeting was held on April 27, 1907, President Hayford in the chair. The society heard an address by Dr. H. W. Wiley, upon the subject of 'How Much Do We Eat?' After briefly mentioning, without discussion, the three principal schools of philosophy relative to man's food consumption, the speaker proposed three points of discussion having a direct bearing upon the subject of how much do we eat, *viz*: the proper proportions of food necessary for growth; for equilibrium of weight, and for old age; the second only of these three was discussed at length. The speaker reviewed briefly the experiments carried out under his direction, where records have been kept for nearly five years of the quantities of food eaten by healthy young men. Some fifty or sixty young men have been under observation during this period, and all the food which they have eaten has been carefully weighed.

In the fore period, preliminary to the observations, the ration which the young men would normally choose was changed one way or the other in order to secure the equilibrium desired. The proportion of protein to the other elements of the ration was selected by the normal taste of the subject, save in some instances where there seemed to be a tendency to eat too much meat, this was slightly checked. A sample of all the results shows that the dry food eaten by a man each day is almost one per cent. of the weight of the body. In other words, a young man weighing 150 pounds will eat in twenty-four hours 1.5 pounds of dry food. The weight of moist food, including water, is almost exactly 4.25 per cent. of the weight of the body. The total amount, therefore, of food and drink in the state in which the food is consumed for a young man of 150 pounds in twenty-four hours is about 6.4 pounds. In other words, the amount of water taken in his food and drink during the day is nearly 5 pounds.

Important questions of social and scientific character arise in connection with the magnitude of the diet. Interesting observations have lately been made looking to the diminution of the quantity of food eaten per day. Mr. H. Fletcher has made interesting observations on this subject, and has sought to show that the quantity of food ordinarily eaten is too great. He calls attention to the fact that slow and patient mastication may suffice to make a less quantity of food satisfy hunger, and furnish the necessary heat and energy for the ordinary human activities. Mr. Fletcher himself submitted to experimental investigations in the calorimeter at Middletown, Conn. The data furnished by the calorimeter indicated that more heat was evolved than could possibly have been furnished by the quantity of food claimed to be eaten.

Of course, it is not possible that a man may live without damage on less food than would furnish the heat and energy for the ordinary activities of life. There must necessarily in this case be a waste and the waste can only be made from the tissues themselves.

The recent investigations of Professor Chittenden must be taken into consideration, where it was demonstrated that strength and body equilibrium could be secured by cutting down very materially the nitrogenous part of the ration. Some of these experiments were continued over a long period of time, and showed that strength even increased with the notable diminution of the nitrogenous elements consumed. This is all interesting, but probably not convincing. If we, for the sake of argument, assume that the theory of evolution is a correct one, then we must admit that man to a certain degree is a creature of his environment. Experience shows that when the human animal is allowed to choose his ration with reasonable facility to get what he wants he eats a certain weight of food in which there is a certain proportion of nitrogen, which it may be said for a man of 150 pounds is not far from 18 grams per day. What would be the effect upon the human animal of cutting this nitrogen out by one third or one half in the course of a few generations or of a few thousand or hundreds of thousands of years? It would, perhaps, change in a very marked degree the human animal. That change might be possibly for the better, but certainly it would not represent the animal himself as he is to-day.

I have just read in the newspapers, which are not always the most reliable purveyors of scientific information, that the recruiting officers in the German Empire have found very few young men in a certain locality suitable for military service, and the inference is that the high price of meat has probably excluded it from the ordinary diet of the peasant, so that the children of the peasants are not receiving the amount of meat food, and presumably of nitrogenous material, which they formerly were able to get. This report, of course, is not worthy of being considered from a scientific point of view, but it shows at least an indication of the trend of thought in this matter.

The best nourished nations, as a rule, are foremost in literature, science and arts, and, according to numbers, in physical power.

Those who treat of diet from an economic, as well as scientific point of view, should be very conservative in advocating any change in rations which would lead to a minimum diet naturally chosen or to a reduction of the proportion of nitrogen to the other constitutents therein.

R. L. FARIS,
Secretary

DISCUSSION AND CORRESPONDENCE

A PROTEST ON BEHALF OF THE SYSTEMATIC ZOOLOGIST AND THE BIBLIOGRAPHER

A PAPER recently come to hand on the Nearctic Hemerobiidæ, *Transactions of the American Entomological Society,* XXXII., pp. 21–52, furnishes an opportunity for a criticism that is not intended for the author in particular, but as a protest against a particular kind of carelessness that we meet with too frequently in present zoological literature. On page 40 of that paper is described what appears to be a new genus, and is so indicated by the abbreviation 'n. gen.' placed after the name. No other reference to the use of the name is indicated. Any bibliographer or future worker would be very justifiably led into the error of dating this genus, and of the several others in the paper which are all treated in the same way, from December, 1905, the date of the paper. But on turning to page 46, we are told in a brief appended note that Dr. Needham has in July, 1905, described this genus under another name. It is then explained that the author published the name of this genus, as well as of the others published in the paper under discussion, in connection with the name of a described species, as early as November, 1904, and that therefore Dr. Needham's name is a synonym. I find no fault with this conclusion, but why I ask, and I demand it in the name of the systematist and of the bibliographer, does he not indicate the date from which the genus originates in the early part of his paper? Why does he indicate as a new genus that which from the standpoint of nomenclature he has described a year earlier?

Take another instance. Dr. Ashmead in

his classification of the Ichneumonoidea published a few years ago has described genus after genus designating manuscript species as types and connecting no known species with them. Dr. Ashmead doubtless intended to describe these species. But he has never done so, and we learn with profound regret that his health is such that he never will be able to do so. Now what is going to be the status of these genera? There will be those who, interpreting strictly the laws of priority, will ignore them absolutely, on the ground that they are *nomena nuda*. There will be others who will attempt to assign them to this place or that, but no one will ever know what their author intended, unless some one, with this purpose in view, laboriously works over the collections on which Dr. Ashmead has based his names. Even then no agreement will be reached among future students as to what is to be done with these genera, which number no less than forty-eight, and like those of Forster they will remain for years a source of confusion, error and instability in our nomenclature.

Instances might be multiplied, but these will suffice, for I do not intend them as personal criticisms, rather merely as remonstrances against a too prevalent carelessness on a very important subject. In a day when the difficulties of the application of the laws of nomenclature, and the increasing confusion in zoological nomenclature are being continually brought home to us on every hand, are such practises on the part of those who are certainly by no means amateurs in systematic zoology to be condoned?

J. Chester Bradley
University of California,
April 24, 1907

SCIENCE AND POETRY—A PROTEST

The advisability of correlating literature and science in the schools was at one time a much-debated educational question. The writer has heard seriously advocated before a State Science Teachers' Association the advantage of always having the zoology class read 'The Chambered Nautilus' when studying the Mollusca, though assent was withheld by the same speaker from the proposition to have the members of every English literature class dissect a nautilus when studying Holmes's poem. That there is nothing poetical in the bare facts of nature, and that nothing is really interesting unless invested with poetry or fancy, are two ideas that can never, it seems, appear erroneous, except to one who has studied nature at first hand.

Sugar-coating the supposed pills of scientific fact in nature-study literature and teaching has been baneful enough, but when articles in reputable magazines, intended for mature minds, poeticize science to the verge of misrepresentation, it is difficult to know whether to blame the author the more, or regretfully to decide that, after all, the general public is still unable to appreciate natural facts as nature presents them.

A series of three articles in *Harper's Monthly Magazine* for December, 1906, and February and March, 1907, entitled 'The Intelligence of the Flowers,' by Maurice Maeterlinck, have been the inspiration of the protest.

To say that no flower is 'wholly devoid of wisdom'; that, in order to deprive a flower of reason and will, 'we must needs resort to very obscure hypotheses'; that it is in the vegetable world that 'impatience, the revolt against destiny, are the most vehement and stubborn'; and that the pollination of the eel-grass is 'a tragic episode,' may be most excellent poetry, and enhance the literary value of an article; may, indeed, for aught we know, be the necessary conclusions of a poet, but to read such statements in cold print congeals the blood of any botanist.

Still we might shiver in charity if interpretations only, and not facts, were open to question. We are told, for example, that the tip of the young stem of a seedling laurel tree, because the seed germinated on a perpendicular rock-wall, 'instead of rising towards the sky, bent down over the gulf,' notwithstanding its geotropism.

We learn that dodder 'voluntarily abandons its roots,' and that it will avoid other species and, 'go some distance, if necessary, in search of the stem of hemp, hop, lucerne or flax.'

In the second article we learn, for the first time, that the *flowers* of *Drosera* and *Nepenthes* are carnivorous, and that the problem of cross-fertilization is 'normally insoluble.' Here, also, obsolete terminology is perpetuated in the expression 'fertilization of the stigma,' and obsolete interpretation in referring to the stigma as the 'female organ,' and to the stamens as the 'male organs' of the flower.

The fact, stated in the first article, that the Virginia creeper or the convolvulus will begin to twine about the handle of a rake, temporarily laid against a wall, does not seem, in the author's mind at least, at variance with the clear 'perspicacity,' 'intelligence' and 'prudence' with which plants in general are attributed elsewhere in the articles. One wonders, though, why the convolvulus did not 'set its thought to working,' as did the *Silene Italica,* mentioned a few lines farther on. But doubtless we have failed to enter into the spirit of the author, for later he implies intelligence to the mountains, the seas and the stars.

'The flowers,' we are told, 'came upon our earth before the insects.'[1] This 'geologically incontestable fact' is, alone, 'enough to establish evolution'!

But the discoveries of recent science sadly pale in comparison with the root-intelligence described in a foot-note to the first article, and credited to Brandis. Thus:

> This root, in penetrating into the earth, had come upon an old boot sole: in order to cross this obstacle, which, apparently, it was the first of its kind to find upon its road, it subdivided itself into as many parts as there were holes left by the stitching needle; then, when the obstacle was overcome, it came together again and reunited all its divided radicles into a single and homogeneous tap-root.

Of course no one could state, *a priori,* that such a marvelous feat was impossible, but it is the kind of tale to which one more readily gives credence if substantiated by photographic evidence. Without such evidence the event, as narrated, is absolutely incredible to any botanist. But even if such an act were common for roots, by what stretch of the imagination could one infer that a root could have preconceived and reasoned out the plan so deftly executed?

[1] That insects appeared in Silurian times, and that there is no certain evidence of angiosperms earlier than the Cretaceous, are facts of paleontology too well known to be dwelt upon here.

There is much in these articles of interest, and of scientific accuracy, and the apparent appreciation, in the last one, of the value of the experimental study of variation is very gratifying.

"All that we observe within ourselves," says Maeterlinck, "is rightly open to suspicion; and we are too greatly interested in peopling our world with magnificent illusions and hopes." Perhaps this explains the impossible botany of the articles, but it can not excuse it.

C. STUART GAGER
NEW YORK BOTANICAL GARDEN,
April 30, 1907

CONCERNING LEFT-HANDED ABORIGINES

A RECENT article in SCIENCE requested people in charge of Indians to find the proportion of left-handed aborigines to the right-handed ones. Acting upon that request, the writer has been investigating the subject among the Hoh and Quileute Indians, and, out of a population of 231, five left-handed people were found: How-withlup (male), Walo-thlu (male), Hick-sh (male), Thle-ba-tolch (male), Hi-yic-to-utl (female).

ALBERT B. REAGAN
LA PUSH, WASH.

UPLIFT INCREASES RAINFALL, DENUDATION DIMINISHES IT

IT has long been known to students of geography that in most parts of the world more and more rain and snow is observed to fall as one examines greater and greater heights on the slopes of hills and mountains up to very considerable elevations. Hellmann's new rainfall map of Germany shows this to be true even of the very flat hills on the plains of northern Prussia. At any point on this plain the hills are a little wetter and the valleys drier than the ground about. Dr. Kassner has suggested in the February *Petermann* that in regions of subdued mountain form there must, therefore, have been greater rainfall in the past when erosion had not accomplished so much of its leveling effect, and remarks that a map of that old-time distribution of rainfall is capable of construction on the basis of the approximate land elevations of the land before denudation took place. In this sense the denudation of the land has been accompanied by diminution of precipitation. It should be remembered, however, that regional uplift has the opposite effect and has not infrequently been the occasion of increase of rainfall and denudation. The Black Hills of South Dakota, for instance, have more rainfall than the region about because of the domed uplift of the region above the plains. It is estimated that 3,000 feet have been removed from their summits by denudation since this uplift and this Kassner would suggest must have been accompanied by diminution of rainfall. But it is quite conceivable that the summits have never been more than 700 feet above the sea, for denudation has been lowering them at the same time that doming has thrust them up. In that case there has been no reduction in height or diminution of rainfall. When uplift ceases and denudation alone controls the elevation, rainfall must undergo the diminution spoken of, but the complete cycle of changes began with increase of rain as the doming first began. This supplied the abundant transporting agent with which erosion resisted further effective uplift and brought increase of precipitation to a halt much as the governor controls the throttle of an engine. From what we may call a mature stage of rainfall, reached likely enough in geographic maturity of the mountain forms, Kassner's diminution must come in.

Mark S. W. Jefferson
Ypsilanti, Mich.

SPECIAL ARTICLES

THE DEVELOPMENT OF UNFERTILIZED FROG EGGS INJECTED WITH BLOOD

During three successive springs (1905–7) the writer has experimented on unfertilized frog eggs by injecting them with blood or lymph of either male or female frogs. In all some fifteen hundred eggs have been so operated upon. Shortly before the time for laying, the eggs were taken from the uterus with every precaution to prevent contamination by sperm. Those nearest the cloacal opening were always set aside as a control and in not a single instance did any of them develop. The other eggs were pricked with a very fine-pointed capillary tube which had previously been charged with lymph and corpuscles by dipping it into the lymph or the blood of another frog.

In eggs so treated numerous instances of cell proliferation and embryonic development have been observed, provided the eggs were fully matured and ready for fertilization. Many eggs after six or eight days showed upon sectioning that they had approximated the full blastular and in some cases the gastrular stages, although the condition came about apparently by some sort of internal nuclear arrangement, as no superficial cleavage furrows were observable and no demarcation into cells was visible from the exterior until the third or fourth day, when close inspection showed in some cases numerous small vesicular or cellular outlines.

In some instances definite organs were developed, though frequently distorted and misplaced. Cross-sections of one embryo, for example, showed such pronounced defects as two neural tubes anteriorly. Of the whole number of eggs operated upon only two developed into free-swimming tadpoles and these were apparently normal as far as superficial examination disclosed. They have not yet been sectioned. After sixteen days one died and the other was killed to insure proper fixation for histological study.

Apparently the white rather than the red corpuscles are the stimulating agents which bring about development, because injections of lymph, which contains only white corpuscles, produce the same effect as injections of blood. Whether or not the fluid part of the lymph or blood produced any effect could not be definitely determined from the material at hand. The whole effect seems, however, to be the result of the proliferation of the leucocytes themselves, which, as they become more numerous, tend to migrate to the surface of the egg and finally form into one or more layers. Each nucleus apparently acquires a local area or zone of protoplasm which ultimately becomes marked off from adjacent areas as more or less of a definite cell. The pigment of the egg accumulates around the boundaries of the more superficial areas, which thus appear to be sharply delimited, as seen in sections under the microscope. Although the internal mass of yolk contains numerous nuclei, frequently undergoing amitotic division, the central mass of the eggs remains in a syncitial condition for considerable time.

I am inclined to believe that, in some cases at least, the female pronucleus of the egg takes no part in this cell proliferation, because I have been able to find in sections in several instances a comparatively large clear protoplasmic zone, variously placed in the egg, which, although invaded more or less on all sides by nuclei, itself remains undivided, and in it is visible what appears to be a degenerating nuclear-like structure, presumably the remainder of the female pronucleus.

Many eggs show no development, but this is not to be wondered at, since doubtless a number of them, although pricked, received no corpuscles from the orifice of the capillary tube. Other eggs, presumably not fully ready for fertilization, did not develop; although the corpuscles apparently proliferated extensively, they later ran together to form giant cells and frequently seemed to become phagocytic in nature. In still other cases, what seemed to be phagocytosis was visible on one side of the egg, while on the other side the nuclei appeared to be ranging up into a definite cellular layer. A detailed description of the experiments is in preparation.

MICHAEL F. GUYER

THE UNIVERSITY OF CINCINNATI,
May 13, 1907

TRANSMISSION INHERITANCE DISTINCT FROM EXPRESSION INHERITANCE[1]

CONJUGATIONS of sex-cells of higher plants and animals have two results, an intermediate and a final product. The intermediate product of conjugation is a new organism, the final product a new equipment of sex-cells. The new organism is built up by vegetative subdivisions of the conjugating pair of sex-cells. The conjugation is not completed until the new generation of sex-cells is to be formed. Fertilization is the beginning of the process of conjugation, which may not conclude for months or years after fertilization has taken place. The organism which is built up during conjugation may be called a conjugate organism, or *conjugate*. It belongs to the same generation as the sex-cells which initiate the conjugation. The next generation may be called *perjugate,* since it has passed through the conjugation of the preceding generation and represents its completed results.

When sex-cells of diverse parentage are associated in conjugation the organisms they build up (conjugates) may be like one parent, or like both parents, or intermediate between the two parents, or different from either parent. The same latitude of alternatives of expression is found in the perjugate generation. The crossing of two varieties of pink-eyed mice yields black-eyed conjugates. Two varieties of smooth-seeded cottons gave smooth-seeded conjugates, but woolly-seeded perjugates. Such instances prove that the expression-tendency of a gamete can be altered by association with another gamete of diverse parentage. Either the conjugate generation or the perjugate generation, or both, may show characters which neither of the parent gametes would have brought into expression if it had secured a partner of its own kind. There is no corresponding proof that transmission inheritance is altered by such associations. The reappearance of such characters as the black eyes and the woolly seeds, which have been abeyant through many generations, shows that failure of expression does not prove failure of transmission.

Transmission inheritance may be thought of as the dial of a compass which carries many character-directions, though the needle of expression points to only one. This expression-polarity is called dominance in conjugate organisms and potency in gametes. Nobody

[1] Read before the Botanical Society of Washington, April 13, 1907.

denies the transmission of unexpressed characters through conjugate organisms, but in dealing with gametes the distinction between transmission and expression has continued to be overlooked; otherwise the Mendelian hypothesis of pure germ-cells could not have attained its wide popularity.

Mendelism and other forms of polarized expression inheritance yield us no intimation whatever regarding the nature and mechanism of transmission inheritance. If transmission could be conceived as a matter of localized character-unit particles we should be justified in thinking of all germ-cells as containing full sets, and not variously mangled fractions of the ancestral equipments. Alternative inheritance of divergent characters means reciprocal expression-polarities. It has yet to be shown that there is any such phenomenon as alternative transmission inheritance, brought about by the segregation of the parental character-units in different germ-cells. Incompatibility sufficient to cause germinal segregation should preserve the original association of the characters, but no such tendency has appeared in Mendelian crosses. When there are several divergent characters they are always expressed in many different combinations, as though to show that the scale of transmission remains complete, no matter how narrowly the needle of expression may sometimes be directed.

O. F. Cook

A NEW METHOD BY WHICH SPONGES MAY BE ARTIFICIALLY REARED[1]

I have found in the course of an investigation carried on for the Bureau of Fisheries that silicious sponges when kept in confinement under proper conditions degenerate, giving rise to small masses of undifferentiated tissue which in their turn are able to grow and differentiate into perfect sponges. The investigation has been prosecuted during the past three summers at the Beaufort Laboratory. While the degeneration with the formation of the indifferent masses has been observed in several species, it is only in one species, a *Stylotella,* that the process as a whole has been worked out.

This sponge, which is exceedingly abundant in Beaufort Harbor, is a fleshy monactinellid commonly reaching a thickness and height of 10–12 cm. Conical processes with terminal oscula project upwards from the lower body. With this species, which is a light-loving form, I have obtained the best results when outside aquaria, either concrete aquaria or tubs, were used. The method of treatment is briefly this: Into a tub about 60 cm. by 30 cm. and covered with glass, a half dozen sponges, freed as far as possible from live oysters and crabs, are put. They are raised from the bottom on bricks. The tub is emptied, filled and flushed for some minutes three times in every twenty-four hours. Direct rays of the sun should be avoided. Tubs answer as well as concrete aquaria, and have the advantage of being movable.

In a day or two the oscula of the sponge disappear, and the surface begins to acquire a peculiar smooth, dense and uniform appearance. Microscopic study reveals the fact that not only the oscula, but the pores also, for the most part, close, and the canal system becomes interrupted and in some degree suppressed. The mesenchyme is more uniform, and is denser than in the normal sponge, owing in part at least to the disappearance of the extensive collenchymatous (very watery mesenchyme) tracts of the latter.

The whole sponge may pass into this state and remain without great change for weeks. During this period it shrinks greatly in size, in a given case to one quarter the original bulk. The arrangement of the skeletal spicules becomes much simplified. With the shrinkage in size the sponge becomes more solid, *i. e.,* more of the canal space is suppressed. Some flagellated chambers persist and there are a few small scattered apertures on the surface. The bulk of the chambers disappear as such, the collar-cells transforming into simple polyhedral masses which become scattered singly or in groups in the general mesenchyme. The mesenchyme is a syncytium composed of well-marked cells that are

[1] Published with the permission of Hon. Geo. M. Bowers, U. S. Commissioner of Fisheries.

freely interconnected. The sponge in this condition closely resembles *Spongilla* in its winter phase, as described by Weltner.[3] Presumably water continues to circulate through the body, but the current must be an exceedingly feeble and irregular one.

As a sponge in this condition continues to shrink, it may subdivide and thus a large sponge may eventually be represented by numerous masses, in a given case about 1 cm. in diameter. Now if the sponge in this condition or if one of the masses into which it has split up, be attached to wire gauze and suspended in a live box floating at the surface of the open water of the harbor, the sponge or piece will in a few days grow and redevelop the pores and oscula, flagellated chambers, tissue differentiation, and skeletal arrangement of the normal sponge. Whether in this regeneration the transformed and separated collar cells again unite to form the flagellated chambers, I can not say. I think it very doubtful.

In the two classes of cases just described the sponge as a whole degenerates and slowly shrinks. Cellular death takes place so gradually that at no time is there any obvious corpse tissue or skeletal débris. Much more common and of far greater interest are the following cases. In these a large part of the sponge body dies in the course of two or three weeks, leaving the skeletal network still in place and bearing the brown decaying remnants of the flesh, which, as maceration continues, are washed away. In places, however, the sponge body does not die. Here masses of living tissue are left, conspicuous amidst the dead remains by their bright color and smooth, clean surface. These living fragments may be classified into three groups. First, the upper end of an ascending lobe or a considerable part of the body of the lobe may be left alive in its entirety, thus forming a more or less cylindrical mass up to 5 mm. diameter, with a length sometimes two or three times the thickness. The histological condition of these masses is not very different from that of the sponges already described. Such a mass may be said to consist of anastomosing trabeculæ, separated by the remains of the canal system. The mesenchyme composing the trabeculæ consists of discrete cells interconnected by processes to form a syncytium. The flagellated chambers as such have nearly disappeared, although remnants may still be recognized. In them the collar cells have transformed into simple polyhedral bodies that are widely separated. The bulk of the chambers have broken up into their constituent cells, and these are now scattered as elementary parts of the general mesenchyme. When such masses are attached to wire gauze and hung in a floating live-box they transform into perfect sponges.

[3] 'Spongillidenstudien, II. Archiv für Naturgeschichte,' 1893.

A second class of surviving remnants includes masses scattered over the general surface of the sponge. These may be spheroidal and small, less than one millimeter in diameter. Usually they are flattened and of an irregular shape with lobes, suggesting a lobose rhizopod or myxomycete plasmodium. Such masses which may be connected by slender strands are commonly from two to five millimeters in the longest direction. The third class of remnants are found scattered through the body of the dead and macerated sponge, in which they sometimes occupy positions that are obviously favorable for respiration. These bodies are more or less spheroidal and small, their diameter varying commonly from one half to one and a half millimeters. In the most successful cases of treatment, the small masses, internal and superficial, are exceedingly abundant, and the dead and macerated sponge body with its contained nodules of conspicuous living tissue strongly suggests a *Spongilla* full of gemmules.

These living remnants of the sponge (bodies of the second and third classes) execute slow amœboid changes of shape and position, behaving thus like plasmodia, and they may be designated as plasmodial masses. Microscopic examination shows them to be of an exceedingly simple character, without canal spaces or flagellated chambers. The mass does not consist of discrete cells, but is an aggregation of syncytial protoplasm studded with nuclei. The protoplasm is stored with minute inclusions and is reticulate in arrangement. The nuclei are practically all alike, and there are no signs of persisting collar-cells. Such a mass represents a portion of the original sponge in which the degenerative changes have progressed farther than in the larger remnants. In the latter we find a syncytium made up of discrete cells among which some persisting collar-cells are distinguishable. But in the plasmodial mass the cells have united so intimately that cell outlines have been wiped out, and recognizable collar-cells (or their nuclei) have disappeared. The optical evidence points to the conclusion that the latter help to form the general syncytium, undergoing regressive changes in their differentiation which result in their becoming indifferent parts of this unspecialized tissue.

The plasmodial masses remain alive in the laboratory indefinitely, but do not transform. They attach to the bottom of the vessel, but so feebly as to be easily shaken loose. In order to see if they would transform when returned to natural conditions, I devised the simple plan of enclosing them in fine bolting-cloth bags which were hung in a live-box floating in the harbor. The bags, rectangular, were divided into compartments about an inch square with the two flat sides nearly touching. In each such space an isolated plasmodial mass was inserted, and the bag sewed up. It was found that in such bags the masses were held in place long enough for them firmly to attach to the bolting cloth. Once attached to the cloth they grow, sometimes quite through the wall of the bag to the outer water, and transform into perfect sponges with osculum, canals, pores and flagellated chambers in such abundance as to be crowded.

This ability to undergo—when the environment is unfavorable but not excessively so, regressive changes of differentiation resulting in the production of a simpler, more uniform tissue, is something that is plainly useful, *i. e.*, adaptive. In the simplified state the sponge protoplasm withstands conditions fatal to such parts of the body as do not succeed in passing into this state, and on the return of normal conditions again develops the characteristic structure and habits of the species. That this power is exercised in nature there can scarcely be a doubt, since the conditions that are present in an aquarium must now and then occur in tidepools.

It is probable that the power thus to degenerate with production of masses of regenerative tissue is general among sponges. I first discovered the phenomenon in *Microciona*, a very different form from *Stylotella* and one in which the skeleton includes much horny matter. And in two other Beaufort species I have succeeded in producing the plasmodial masses. There is every reason for believing that the commercial sponge shares in this ability. If this is so, we have here a means of propagation which with a further development of methods may at some time become economically practicable. In any case it is now possible to study the differentiation of a quite unspecialized tissue, one that is physiologically embryonic, into a perfect sponge at any time of the year irrespective of the breeding season. We may even exercise some direct control over the size of the plasmodial masses, as the following experiment shows.

Microciona was kept in aquaria until the degenerative process had begun. Pieces were then teased with needles in a watch glass of sea water in such a way as to liberate quantities of cells and small irregular cell-agglomerates. These were gently forced with pipette to the center of the watch glass. Fusion of cells and masses, with amœboid phenomena, began at once, and in half an hour quite large irregular masses existed. In the course of a few hours the masses grew enormously through continued fusion. From this time on they adhered firmly to the glass, retaining irregular plasmodium-like shapes, and the growth was inconspicuous. To bring them together once more and induce further fusion they were on the following day forcibly freed, with pipette and needle, and to clean them of cellular débris and bacteria were transferred to a tumbler (covered with bolting cloth) in which they were kept actively moving under a fine glass faucet for about thirty minutes. In the course of this violent agitation a good many masses were lost. Those remaining in the tumbler became in the next few hours notice-ably rounder and smoother at the surface. From this experiment eighteen more or less spheroidal masses were obtained, some of which measured one half millimeter in diameter. They were similar to the small plasmodial masses produced in this species (and in *Stylotella*) when the sponges are allowed to remain quietly in aquaria. As already stated, it is only in *Stylotella* that I have directly proved the regenerative power of these masses.

Maas has just announced[3] that calcareous sponges (*Sycons*) when exposed to sea water deprived of its calcium undergo marked degenerative changes, which may be of such a character that the living tissue quite separates from the skeleton and breaks up into compact cords of cells showing active amœboid phenomena. The cords further constrict into rounded masses the likeness of which to gemmules is pointed out. Maas states that he is not yet in a position to say whether these masses have the power to transform into sponges, but adds that some of his observations induce him to believe that this is possible.

It is evident that Maas, working on very different forms, has independently met with the same degenerative-regenerative phenomena as are described in this communication, the essential facts of which were presented (together with an exhibit of gemmule-like degeneration masses and young sponges into which such masses had transformed) at the recent December meeting of the American Society of Zoologists. I may add that more than two years ago at the end of the summer of 1904, in my official report (unpublished since the research was still in progress) to the Bureau of Fisheries on the investigation under my charge, I described the degenerative phenomena in *Microciona* and *Stylotella, i. e.,* the formation under certain conditions of confinement of minute masses presenting a likeness to gemmules, and emphasized the probability that these masses were able to regenerate the sponge. It was not, however, until the summer of 1906 that I was able to demonstrate the truth of this view.

[3] 'Ueber die Einwirkung karbonatfreier und kalkfreier Salzlösungen auf erwachsene Kalkschwämme und auf Entwicklungsstadien derselben. Archiv für Entwicklungsmechanik der Organismen,' Bd. XXII., Heft 4, December, 1906.

H. V. WILSON

UNIVERSITY OF NORTH CAROLINA,
CHAPEL HILL, N. C.,
February 16, 1907

SCIENTIFIC NOTES AND NEWS

THE honorary freedom of the City of London is to be conferred on Lord Lister.

THE gold medal of the Linnean Society, London, has been awarded to Dr. Melchior Treub, director of the Botanical Garden at Buitenzorg.

A COMMITTEE has been appointed to arrange for the presentation to the Medical Department of the University of Pennsylvania of a portrait of Dr. John Guitaras of Havana, formerly professor of pathology at the University of Pennsylvania. The portrait will be painted by Mr. Armando Menocal of Havana.

DR. W J MCGEE has been elected secretary of the Inland Waterways Commission, recently appointed by President Roosevelt.

PROFESSOR ELIJAH P. HARRIS, A.B. (Amherst, '55), Ph.D. (Göttingen, '59), since 1868 professor of chemistry at Amherst College, has retired from active service.

PROFESSOR ERNEST RUTHERFORD, whose call from McGill University to the University of Manchester was announced some time since, has now gone to Manchester.

DR. J. HALM, assistant at the Royal Observatory, Edinburgh, has been appointed first assistant at the Cape Observatory, in succession to Mr. S. S. Hough, F.R.S., who was recently promoted to succeed Sir David Gill as H.M. Astronomer at the Cape.

THE Chicago Chapter of the Sigma Xi Society has held three meetings during the year 1906–7. The following papers were read:

December 3, 1906—'Some Glimpses of Mexican Vegetation,' by Professor C. R. Barnes, of the University of Chicago.

February 21, 1907—'The Conduct of Research,' by Professor H. H. Donaldson, of Wistar Institute, Philadelphia.

May 11, 1907—'Some Problems in the Study of Nutrition,' by Professor R. H. Chittenden, director of Sheffield Scientific School, Yale University.

Thirty members have been elected to membership in the society during the year. At the meeting on May 11 the following officers were elected:

President—Professor J. P. Iddings.

Vice-president—Professor J. R. Angell.

Recording Secretary and Treasurer—Professor Carl Kinsley.

Corresponding Secretary—Professor F. R. Moulton.

National Councilor—Professor S. W. Williston.

Members of the Electoral Board—Professors S. W. Williston, E. H. Moore, R. A. Millikan, R. R. Bensley, J. Stieglitz.

THE Davenport Academy of Sciences has finished its annual popular scientific lecture course, the lectures this year being by Professor Reuben G. Thwaites, of the Wisconsin Historical Society; Arthur Farwell, of Newton Center, Mass.; Frank M. Chapman, of the American Museum of Natural History, New York; Professor Arthur Fairbanks, of the University of Michigan; Professor Samuel Calvin, of the University of Iowa; Professor Thomas H. Macbride, of the University of Iowa, and Professor Frederick Starr, of the University of Chicago.

PROFESSOR R. H. CHITTENDEN, director of the Sheffield Scientific School, Yale University, has accepted an invitation to deliver the Sigma Xi lectures before the Universities of Nebraska, Kansas, Iowa and Missouri next winter. These four universities have joined together to obtain a man who is an authority on some scientific subject to come west and give a joint lecture course, delivering one lecture before each university. Dr. Chittenden will give the course during the latter part of February, 1908.

THE Robert Boyle lecture of Oxford University for 1907 was delivered by Professor Karl Pearson, on 'The Scope and Importance to the State of National Eugenics,' on May 17.

DR. DAVID P. BARROWS has completed his courses of lectures, on the 'Peoples of the Philippines and the Ethnology of Malaysia,' at the University of California. These are probably the first regular courses of instruction given at any university in America on the ethnology of these regions. Dr. Barrows sails on July 5 to resume his work as director of education of the Philippine Islands.

THE committee of one hundred, appointed by the American Association for the Advancement of Science to further the promotion, of national interest in health, met in New York City, April 18, and organized by the adoption of rules, the election of officers and the appointment of an executive committee. Professor Irving Fisher, of New Haven, presided as the temporary chairman and was subsequently elected president. Ten vice-presidents were elected, as follows: President Charles W. Eliot, Harvard University; Felix Adler, New York; Dr. William H. Welch, Baltimore; Rev. Lyman Abbott, New York; President James B. Angell, University of Michigan; Miss Jane Addams, Chicago; Hon. Joseph H. Choate, New York; Rt.-Rev. John Ireland, St. Paul; Hon. Ben. B. Lindsey, Denver; Hon. John D. Long, Boston. Mr. Champe S. Andrews was elected secretary. Professor Irving Fisher, Dr. Thomas Darlington, Professor J. P. Norton, Dr. John S. Fulton, Dr. Richard C. Newton, Professor F. F. Wesbrook and Champe S. Andrews were elected an executive committee.

WE learn from the *British Medical Journal* that it is proposed to found a prize in memory of the late Dr. Paul Julius Möbius of Leipzig, well known for his contributions to the literature of neurology and psychiatry and by his medico-literary studies on Goethe and other celebrities. The prize is to be known by his name, and is to be awarded every year for the best essay dealing with some neurological or psychiatrical subject. A committee, which includes among its members Professor Edinger, of Frankfort-on-the-Main; Professor Moeli, of Berlin, and Dr. Lamhofer, of Leipzig, has been formed to collect subscriptions for the purpose.

PROFESSOR HENRY CREW, Fayerweather professor of physics in Northwestern University, has been elected president of the Northwestern Chapter of Sigma Xi. On May 24 Professor

William A. Locy delivered before this chapter a lecture on 'The Life and Work of Linnæus.'

THE Zoological Club of the University of Nebraska celebrated the centenary of the birth of Louis Agassiz on May 28. Addresses were given by Professor H. B. Ward on 'The Debt of the New World to Louis Agassiz,' and by Professor F. D. Losey on 'Lowell's Tribute to Agassiz.'

THE Denison Scientific Association on May 28 celebrated the one-hundredth anniversary of Agassiz's birth by a memorial exercise at which addresses were made by Dr. E. W. Hunt, president of Denison University, on 'The Personality of Agassiz'; by Professor M. E. Stickney, on 'Penikese and the Agassiz Museum'; by Professor Frank Carney, on 'Agassiz's Contribution to Geology,' and by Professor C. J. Herrick, on 'Louis Agassiz and the New Natural History.' These addresses were delivered in the new Barney Memorial Hall of Science of Denison University and were followed by a reception at which the building was opened to the public for inspection. This hall was originally built in 1894 and was destroyed by fire in 1905. It has been rebuilt fire-proof and greatly improved in other respects by the original donor, Mr. E. J. Barney, of Dayton, O.

A TABLET to the memory of Dr. Mary Putnam Jacobi was unveiled in the Woman's Medical College, New York, May 23. An address was made by Dr. Bertha Lewis on behalf of the alumnæ association, and Dr. William Welch accepted the tablet. The memorial tablet is of brass and is inscribed as follows. "In memoriam, Mary Putnam Jacobi, class of 1864, Woman's Medical College of Pennsylvania; president of the Alumnæ Association 1881–1891 and 1894–1895. Ecole de Médecine, Paris, class of 1871; professor of materia medica and therapeutics, Woman's Medical College of the New York Infirmary; professor of the diseases of children, New York Post-Graduate Medical College and Hospital; fellow of the New York Academy of Medicine. A distinguished contributor to medical literature, and one of the most eminent women of her time in the medical profession."

AT the recent session of the Pennsylvania legislature the senate voted $300,000 to enable the American Philosophical Society to erect a memorial to Franklin, but the house did not concur in the bill.

THE death is announced of Dr. Charles Féré, physician to the Bicêtre, Paris, well known for his researches in neurology and psychiatry.

SIR JOSEPH FAYRER, well known for his pathological work in India and as one of the most prominent British physicians, died at Falmouth on May 21, at the age of eighty-three years.

MR. SAMUEL LORD MORRISON, A.B. (Harvard, 1873), an engineer, known especially for his work on filtration plants, died in London on May 21, at the age of fifty-six years.

DR. VOLZ, of Berne, while making natural history collections in the Liberian Hinterland, was murdered by natives.

DR. WILHELM MÜLLER, curator of the mineralogical collections of the Technical Institute of Berlin, died on May 2.

THE death is announced at the age of fifty-two years of M. Charrin, professor of general and comparative pathology at the Collège de France.

A COMMITTEE of Filipinos appointed to investigate the inoculation of prisoners at Bilibid Prison with contaminated serum, has made a report. The committee finds that no one was responsible for the accident, and exonerates Dr. F. P. Strong, of the Bureau of Science, who was in charge of the bacteriological work, and was conducting the experiments.

A CONFERENCE of state geologists was held with the officers of the National Geological Survey at Washington, on May 20, to discuss cooperative work in various states by the federal and state authorities. State geologists were present from Alabama, Connecticut, Illinois, Indiana, Iowa, Kentucky, Maine, Maryland, Michigan, Missouri, New Jersey, New York, North Carolina, Ohio, Pennsylvania and South Carolina.

THE Paris Society of Chemistry celebrated the fiftieth anniversary of its foundation on

May 17; among the guests were Professor W. H. Perkin and Professor Henry E. Armstrong.

THE Swiss Scientific Society will hold its ninetieth annual meeting at Fribourg, beginning on January 28. . The Swiss National societies of geology, botany and chemistry will meet at the same time as the sections of the general society.

MR. W. T. HORNADAY has presented to the New York Zoological Society his collections of heads, horns and tusks, comprising 131 specimens, representing 108 species. These are to form the nucleus of a collection to be exhibited at the New York Zoological Park, and it is hoped notable additions may promptly be made. Mr. Charles T. Barney has recently given the world's record tusks of African elephant, one measuring 11 feet in length, the other 11 feet 5½ inches. The weight of the pair is 293 pounds. The record pair as regards weight is that shown by Tiffany & Co. some years ago, whose combined weight was 463 pounds.

THROUGH the generosity of Messrs. Alfred F., Charles C., and John S. Pillsbury, of Minneapolis, Dr. Thomas G. Lee has secured for the department of histology and embryology, University of Minnesota, the *Handapparat,* or working library, of the late Professor William His, of Leipzig. This collection comprises over 8,400 monographs and separates contributed by over 2,500 different authors.

THE *Journal* of the American Medical Association states that the library of the Royal College of Surgeons, which is the largest medical library in Great Britain and the largest in Europe, with the exception of the library in Paris, has outgrown the space allotted to it. It now contains 100,000 publications, made up of 60,000 volumes and 40,000 pamphlets. It increases at the rate of 1,000 volumes a year. A large room has been added over the library, which in former years consisted of a suite of apartments occupied by a clerk who lived in the college, a custom no longer followed. This room will be principally devoted to work on public health and official government reports. Thus space has been obtained for another 10,000 volumes.

MR. WALTER WELLMAN has now left for Norway en route for Spitzbergen on his second year's work in connection with his project for reaching the North Pole by means of his airship *America,* which is now on its way to Norway. In a statement made to Reuter's representative, he said all the members of the expedition would meet at Tromso, from which place they would sail on June 1 on board the expedition steamer Frithjof for Spitzbergen, where they will arrive on June 5 or 6. The rest of that month will be occupied in installing gas apparatus, enclosing the great balloon house, and assembling the car, motor, etc. At the end of June the balloon will be inflated. In the first week of July there will be trials of the airship until it is demonstrated that it is ready for the voyage. The start for the Pole will be made on the first favorable opportunity afterwards, probably between July 20 and August 10, but, if necessary, Mr. Wellman is prepared to start as late as August 20. The members of the expedition are: Mr. Walter Wellman; Major Hearsey, executive officer and scientific observer, who has been lent by the United States Government; Chief Engineer Vannerman, who is already in Norway; Dr. Fowler, surgeon; and M. Gaston Hervieu, the aeronautical engineer. About thirty men, sailors, mechanics, &c., will be embarked at Tromso, making a total expeditionary force of forty men, including Mr. Felix Riesenberg, the navigator of the expedition, who, together with two Norwegian companions, has spent all the winter at the base at Spitzbergen.

THE lecture arrangements at the Royal Institute of Public Health for the summer session are as follows: The Harben Lectures will be delivered by Professor Paul Ehrlich, director of the Royal Institute of Experimental Therapeutics at Frankfort, on June 5, 7 and 11, the subject being experimental researches on specific therapeutics. Lectures will be delivered each Thursday from May 23 to June 20, both days inclusive, on the Veterinary Aspects of the Tuberculosis Problem, by Professor J. Penberthy; on the Problem of a Pure Milk Supply, by Professor R. T. Hewlett; on Blood Immunity, by Professor G.

Sims Woodhead; on the Treatment of Infectious Diseases regarded from the point of view of hospital administration, by Dr. E. W. Goodall, Medical Superintendent of the Homerton Fever Hospital; and on the Development of Africa, as a problem of comparative pathology, by Dr. L. W. Sambon. The lectures are all free.

THE Botanical Department of the University of Pennsylvania will hold its closing meeting and reception in the Botanical Garden on June 8, from five to ten in the evening. At 7 P.M. Provost Charles Custis Harrison, LL.D., honorary president of the society, will make the introductory address, followed by a series of lectures and short talks and an inspection of flowers and specimens, many of them added to the department since the last annual meeting by gift or in consequence of the travels and researches of members of the faculty and students.

Nature gives the following account of the program of the meeting of the International Union for Cooperation in Solar Research, which was held recently in Meudon, near Paris: "The meeting will open on May 20, when formal business will be transacted in the morning. In the afternoon it is intended that all new proposals for joint work shall be submitted to the meeting, so that members will have an opportunity of privately discussing the desirability of adopting the proposals before a final decision is taken towards the end of the week. The mornings of May 21 and 22 will be spent in receiving the reports of the committees appointed at the Oxford meeting in 1905. It is understood that Professor Pérot is ready to submit his measurements of the wave-length of the red cadmium line, and that his results are in such good agreement with those previously obtained by Michelson that the meeting probably will be able to adopt finally a primary standard of wave-length. Other reports deal with the observations of sun-spot spectra and the organization of the systematic application of the spectroheliograph to solar work. A question of interest to which several members of the union have given considerable attention consists in fixing the best methods of measuring the areas of flocculi. This matter has been under consideration at some of the American observatories, as well as at the Solar Physics Observatory at South Kensington and at the University Observatory, Oxford. On Tuesday evening Dr. Janssen, the president of the congress, will give a banquet to the members at the Hôtel d'Orsay, in Paris, and on Wednesday afternoon Professor Julius will demonstrate in the physical laboratories of the Sorbonne some of his experiments on anomalous dispersion. Arrangements have also been made to visit the Observatory of Paris in the same afternoon. It is hoped that the scientific work of the meeting will be concluded on May 23, and an excursion to the Château de Chantilly has been arranged for Friday. A formal business meeting on May 25 will bring the meeting to a close."

UNIVERSITY AND EDUCATIONAL NEWS

THE Pennsylvania legislature voted at its recent session an appropriation of $500,000 for the University of Pennsylvania. The bill has not yet been signed by the governor.

MR. AND MRS. JOHN C. HEMMETER have given an endowment for the chair of physiology of the University of Maryland.

AN alumnus of Hobart College has given $20,000 for a new gymnasium.

ONE of the dormitories of Trinity College, Hartford, Conn., was injured by fire on May 22. Some damage was also done to the library, the entire loss being estimated at $15,000.

SARATOFF has been chosen as the seat of the new Russian University which is to replace that of Warsaw.

THE University of Montana announces the establishment of fellowships in the departments of chemistry, botany, physics and mechanical engineering, each with an income of five hundred dollars annually in addition to tuition and laboratory fees. Each fellow will be expected to devote approximately half his time to assistance in the department in which he is chosen. It is desirable that the fellow in botany should be interested in forestry, and the fellow in chemistry will be expected to take charge of the laboratory work in the first year's work in inorganic chemistry. There is an excellent opportunity for research work on smelter problems, and also along some lines of electro-chemistry. Graduates of any reputable college or university are eligible. Applicants should send their credentials to Dr. O. J. Craig, president, Missoula, Montana.

Mr. Daniel W. O'Hern, A.B. (Drake), A.M. (Virginia), now of the Johns Hopkins University, has been appointed associate in geology in Bryn Mawr College.

At Amherst College, Professor W. J. Newlin, associate professor of mathematics and psychology, has been appointed associate professor of philosophy. He will continue next year the work he has carried on since the death of Professor Garman.

The following changes have been made in the faculty and instructing staff of the Massachusetts Institute of Technology: Promotions from associate professor to professor: John O. Sumner, A.B., professor of history; Frederick H. Bailey, A.M., professor of mathematics; Henry Fay, Ph.D., professor of analytical chemistry. New appointment: Reginald A. Daly, Ph.D., professor of physical geology; Professor William O. Crosby, S.B., has been retired under the Carnegie Foundation. Promotions from assistant professor to associate professor: Henry G. Pearson, A.B., associate professor of English; Ralph R. Lawrence, S.B., associate professor of electrical engineering; George C. Shadd, S.B., E.E., associate professor of electrical engineering. New appointment: Edwin B. Wilson, Ph.D., associate professor of mathematics. Promotions from instructor to assistant professor: Leonard M. Passano, A.B., assistant professor of mathematics; George L. Hosmer, assistant professor of civil engineering; Charles B. Breed, S.B., assistant professor of civil engineering; Maurice De K. Thompson, Ph.D., assistant professor of electro-chemistry; Henry L. Seaver, A.B., assistant professor of English. New appointments: Gilbert N. Lewis, Ph.D., assistant professor of physico-chemical research; Earle B. Phelps, S.B., assistant professor of research in chemical biology. The following instructors have received leave of absence: Clifford M. Swan, S.B., instructor in physics; Clarence L. E. Moore, Ph.D., instructor in mathematics; Francis Harold Dike, A.B., instructor in modern languages. Return from leave of absence: Daniel F. Comstock, Ph.D., instructor in theoretical physics. Resignations: Raymond Haskell, S.B., S.M., instructor in theoretical chemistry; Champion H. Mathewson, Ph.D., instructor in analytical chemistry. Promotions from assistant to instructor: Clinton H. Collester, A.M., instructor in English; Harold G. Crane, S.B., instructor in electrical engineering; George A. Rodenbaeck, S.B., instructor in electrical engineering. New appointments: Nels J. Lennes, M.Sc., instructor in mathematics; Richard C. Tolman, S.B., instructor in theoretical chemistry; Robert S. Williams, instructor in analytical chemistry; Henry B. Phillips, Ph.D., instructor in mathematics. Appointments as assistants: Charles R. Bragdon, A.B., S.B., assistant in theoretical chemistry; Paul S. Fiske, A.B., assistant in inorganic chemistry; George F. White, S.B., assistant in organic chemistry; Frank B. Shields, assistant in technical analysis; Herman W. Mahr, research assistant in organic chemistry. Resignations: John C. Hudgins, A.B., assistant in inorganic chemistry; Ralph S. Gifford, S.B., assistant in theoretical chemistry; Frank J. Quinlan, assistant in inorganic chemistry; Albert H. Smith, assistant in mechanical engineering; Albert L. Smith, S.B., assistant in analytical chemistry; Anna M. Cederholm, S.B., assistant in technical chemical research; Walter G. de Steiguer, S.B., assistant in geology; Arthur Neale, S.B., A.R.C.Sc., assistant in technical analysis; Fred C. Mabee, A.M., research assistant in physical chemistry; Ledyard Sargent, A.M., research assistant in physical chemistry; E. B. Spear, B.A., research assistant in physical chemistry. Lecturers — New appointments: James F. Kemp, A.B., E.M., Sc.D., on economic geology; M. C. Whitaker, S.M., on factory organization and management.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, JUNE 14, 1907

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

SOME PRINCIPLES OF MUSEUM ADMINISTRATION

IN No. 641 of SCIENCE (April 12, 1907) Dr. George A. Dorsey discusses in some detail the installation of the ethnological collections in the American Museum of Natural History, basing his criticism essentially on the point of view that the arrangement is an unsatisfactory attempt at popularizing the results of ethnological research. In his discussion he assumes that the essential object of a large museum must be research, not instruction, without, however, discussing the validity of this fundamental assumption.

I may be allowed in the following remarks to discuss what seems to me the vital question of the uses of museums as research institutions and as educational institutions. Since my own practical experience has largely been gained in ethnographical museums, I may be allowed to take my examples particularly from these, indicating at the same time in what respects ethnological museums seem to differ from natural-history museums.

Museums may serve three objects. They may be institutions designed to furnish healthy entertainment, they may be intended for instruction and they may be intended for the promotion of research.

The value of the museum as a resort for popular entertainment must not be underrated, particularly in a large city, where every opportunity that is given to the people to employ their leisure time in healthy and stimulating surroundings should be developed, where every attraction that counteracts the influence of the saloon and of the race-track is of great social importance. If a museum is to serve this end, it must, first of all, be entertaining, and try to instill by the kind of entertainment offered some useful stimulant. The people who seek rest and recreation resent an attempt at systematic instruction while they are looking for some emotional excitement. They want to admire, to be impressed by something great and wonderful; and if the underlying idea of the exhibit can be brought out with sufficient clearness, some great truths may be impressed upon them without requiring at the moment any particular effort. The visitor of this class does not go to the museum to study the exhibits case by case and to follow a plan carefully laid out by the curator, but he strolls through the halls examining something that attracts his attention here and there without much plan or purpose.

It is a fond delusion of many museum officers that the attitude of the majority of the public is a more serious one; but a calm examination of the visitors passing through museum halls shows very clearly that the majority do not want anything beyond entertainment. This can easily be proved by following them through the halls and listening to their remarks, by the general tendency of visitors to go through all the halls of the museums from end to end in order 'to have seen' the museum. It may be seen in the Sunday afternoon crowds in New York City when parents pass the hours after dinner with their children in pleasant surroundings, trying to take in the curious sights.

If this is true, then the very serious question arises, what can be done for this very large class of visitors? Obviously, a systematic exhibit will not appeal to them, and the best we can hope for is to bring home to them by single exhibits important points of view. Most of our museums are not built on a plan which promises success in this direction. To impress a point of view requires at least the possibility of concentration; while our large halls, built with a view to architectural impressiveness, do everything that is possible to distract the visitor, who, when just beginning to take in one exhibit, already looks forward to the next one, thus being prevented from ever concentrating his attention on any particular subject. Effectiveness must be based on the effort to concentrate attention, and on the unity of the idea expressed in each exhibit. Those who have seen the room in the Dresden Museum containing the Sistine Madonna will know what I mean. In this room is nothing to distract the attention of the visitor from the single exhibit, and consequently the room is a sanctuary.

It seems essential that before deciding upon the selection of subjects to be presented to the public, the museum director should be clear as to the objects to be obtained by popular exhibits. Popularization of science has become of late years a kind of Shibboleth, and we are only too apt to believe that an effort to present in a simple way results of scientific inquiry is in itself a praiseworthy endeavor.

I fear that in this belief some of the fundamental objects of the popularization of science are overlooked. In the mass of lectures intended to popularize knowledge, in popular books, and not less in popular museums, intelligibility is too often obtained by slurring over unknown and obscure points which tend to make the public believe that without any effort, by listening for a brief hour or less to the exposition of a problem, they have mastered it. This I consider one of the serious dangers of popular presentation of science. It is a stimulus to the overestimation of one's own powers, which is so characteristic of many phases of our public life. It tends to stimulate the idea that the necessity for training for thorough work is an antiquated prejudice, and that good common sense with a little smattering of knowledge fits a man for any place in life, in business as well as in science and in public affairs.

What I understand by popularization of science is an endeavor to counteract these very influences, and to bring out the sublimity of truth and the earnest efforts that are needed to acquire it. Therefore every kind of inaccuracy should be most carefully avoided, and attempts to make all problems appear childishly simple by the elimination of everything that is obscure should not be tolerated.

This does not mean that the most complex problems should be selected for popular presentation, but the serious effort required to reach results should be emphasized. To apply this to the striking popular exhibits to which I referred before, enough should be given surrounding these exhibits to convey the impression that the visitor, by looking at the single thing, has not grasped all that is conveyed by the collections, and that there is more to study.

Another point of view should be borne in mind. When the technical perfection of the striking exhibits is very great, the danger is ever-present that the admiring public will not see the idea that is to be conveyed by the exhibit, but will forget even to look for it in its admiration of the technical skill exhibited in the installation. For instance, in an exhibit of gulls hovering over the waves of the sea, it is only too likely that the visitors will ask, 'How are they suspended?' and that upon coming back from the museum, they will tell their friends of the skill exhibited in the invisible suspension of the birds, but presumably they will not know what birds they were. Thus every incidental point that is added to the essentials of the exhibit will distract attention from the fundamental idea. I fear that in some cases an interest in the artificial likeness to nature may be engendered like that felt by the courtiers of the Emperor of China in Andersen's fairy tale, 'The Nightingale,' when they all exclaim on discovering that the nightingale is not a mechanical toy: 'How uninteresting! It is a real bird!'

In order to attract the attention of the visitors who stroll through the halls, the museum needs a somewhat indifferent background of material, from which is set off here and there a striking exhibit intended to arrest attention; and the art of the museum administrator consists in the proper selection of such exhibits as will drive home a definite idea. A museum consisting only of an array of striking exhibits defeats to a certain extent its own ends, because where a great many objects of equal interest are assembled, the attention given to each is only slight. Furthermore, the indifferent background which consists of exhibits related to the one illustrating a particular idea elucidates the vastness of the problem dealt with, and is a check against the superficial assumption that the one exhibit exhausts the subject.

There are only two methods possible to reach the visitors who come to the museum to be entertained. The one is to have only a very few exhibits of rare beauty and excellence, which by their own merit will prove attractive. An attempt to carry this idea into execution has been made in parts of the Museum of the Brooklyn Institute. However, this is avowedly neither the object nor the method of a large museum which endeavors to gather under its roof a great variety of objects, and to impose not only by a small selection of exhibits, but also by the comprehensiveness of subjects presented. Wherever this is true, it must be recognized that it is impossible to hold the attention of the people by the whole mass of exhibits, but that for every visitor the bulk of the material must merely give the background from which some subject that happens to strike his fancy will stand out in bold relief.

I think the experience of all large museums shows that this point of view, so far as the general public is concerned, is the correct one. When, for instance, the installation of a new immense mounted skeleton of some extinct animal is announced, people will flock in crowds to the museum to see the specimen, and the receptiveness of their minds is increased by the whole mass of material from which the new impressive specimen is set off, and by the striking difference of the atmosphere in the museum as compared with the ordinary everyday routine. The same is true when a large, beautiful group of birds is exhibited. The impression, however, is quite different if the museum should contain a great many mounted skeletons of immense size, or a great many groups of birds of similar character. The visitors will pass from one to another, but the amount of impressiveness of each will be correspondingly decreased.

Considering this point of view, I think no word has ever been said that is less true than Dr. Brown Goode's oft-repeated statement that a museum is a well-arranged collection of labels illustrated by specimens. On the contrary, the attraction for the public is the striking specimen; and whatever additional information either the label or the surrounding specimens may be able to convey to the mind of the visitor is the only result that can be hoped for.

I believe experienced museum administrators will agree with me in thinking that among the museums having the best attendance, so far as it is not artificially increased by including the attendance of lectures among the museum visitors, this class of visitors amounts easily to ninety per cent. or more of the total number. This is true particularly of the United States National Museum, which is simply taken in as one of the sights of the national capital, and it is also true of the Saturday and Sunday attendance of a museum like the American Museum of Natural History in this city.

I am inclined to think that the museum can do a great deal towards public education by a judicious treatment of this aspect of its work; but it requires the highest talent to select and set off a striking object which brings home an important idea against the indifferent background so as to obtain the best results.

From the remarks that have been made, it will be seen that in a large museum opportunity is given to select objects in such a way that a great variety of important points of view are brought out by special exhibits.

An attempt of this kind has been made, in the large entrance-hall, a number of very excellent exhibits are arranged, partly of a systematic character, partly intended to bring out certain special biological points. I believe the feeling that is conveyed here upon the visitor is a very favorable one, in so far as the assemblage of this material in the entrance has for its background the expectancy created by the mass of material to be found when the visitor moves from this hall into the galleries of the building. On the other hand, it may be said that still better results might be had if these halls themselves were to contain here and there equally striking exhibits.

I believe the appreciation of the needs of the visitor who wants to be entertained has led most museums to lay much stress upon the preparation of groups in which certain objects are brought together, and which are generally intended to illustrate some important point. In the practise of group-making that has developed during the last fifteen years the need of the class of visitors for whom they are intended is often lost sight of. A group does not convey any more information than a picture in an ordinary picture-book might be made to convey. It differs from the picture-book in being more impressive by its size and surroundings. Therefore a series of groups, all of which illustrate different aspects of the same idea, are undesirable, because the impressiveness of each is decreased by the excessive application of the same device. I believe the effect of this undue multiplication of groups of the same type may be noticed in the collections of the United States National Museum. It is true that the multiplication of groups in the anthropological department of that museum is not due to a systematic endeavor on the part of the administration to present every Indian type in the form of a group. It is due rather to the onerous duty imposed upon the museum to send some new striking exhibit to every one of the endless series of national and international expositions, which, of course, are seen almost exclusively by sight-seers, who can not be reached by anything but such large exhibits as groups. Any one who will observe the visitors of the United States National Museum strolling through the Catlin Hall, which contains the Indian groups, will readily see how the first group seems very interesting, and how quickly the others appear of less and less interest and importance. For this reason it may safely be said that the method of bringing together large exhibits should be employed only sparingly, and that the effect of each of these exhibits will be the greater the better it is set off against an indifferent background.

I have mentioned here large exhibits as those which will attract the general public. This is not quite correct, in so far as there will always be an appreciable number of visitors of a higher education, who may be attracted by the beauty and compact idea brought out by small special exhibits.

Museums may also be employed for the purpose of imparting systematic information. The number of people who visit the museum in search of such information is, comparatively speaking, small, but not by any means negligible; and the duty of the museum to supply such information to those who are in search of it must not be questioned. The question arises, however, in how far a very large museum is capable of supplying the needs of students of this type. Assuming a building like the American Museum of Natural History, which has at present eighteen halls and six galleries, with a floor space of from eight to ten thousand feet for the halls, and of from four to five thousand feet for the galleries, and imagining the various halls so arranged as to give a systematic presentation of the various sciences, we find that the whole becomes such a maze of separate and intercrossing systems, that the average visitor, even if desirous of obtaining systematic information, would be frustrated by the mass of material presented.

Here, obviously, the fundamental principle of elementary education has to be applied; namely, that effectiveness does not lie in diversity, but in the thoroughness of the material presented. *Multum, non multa.* So far as I am aware, the attempt at systematizing the collections of a very large museum according to a rigid scheme has never been made, obviously on account of the insuperable difficulties that present themselves.

One of these difficulties consists in the lack of systematic collections illustrating all the different branches of science. This lack is very striking in all our American museums, where the mass of material consists almost everywhere of collections from North America, and where material from other continents is very inadequately represented. There is no inherent difficulty in obtaining small systematic collections covering any particular branch of science, but in a large museum there is always a preponderance of material relating to particular problems or to particular regions. This lack of material will always be an obstacle to a complete systematization of large collections, even if such systematization were desirable. A number of small museums have tried to develop representative and systematic collections, with excellent success, and have become in this way important adjuncts for the teaching facilities of the cities in which they are located; but here efficiency is inseparable from small size.

The difficulties that lie in the way of arranging a large museum according to a systematic plan of instruction are manifold. First of all, it must be recognized that in a large city people with entirely different interests will consult the museum, and will therefore desire to find the material in entirely different systematic arrangement. To take the example of zoology. One teacher may desire to utilize the museum for his classes in which he gives a review of systematic zoology. Another one may desire to impress upon the student the development of the nervous system or of certain organs of the body. Still another may be interested in the essential phenomena relating to the question of evolution of species. And still another may want to illustrate by means of collections the traits of local faunas. The greater the number of people who desire to consult the museum in this manner, the more numerous will also be the points of view from which systematization will appear desirable. I think even to those not familiar with museum administration it will be at once apparent that the attempt to organize the entire collections of a large museum from this point of view can have only one result. If every justifiable point of view is included, the complexity of the system will become so great that the usefulness of the whole series will become very doubtful. If, on the other hand, only a few points of view are selected, then all sciences as presented in that particular museum will appear in the strait-jacket into which they have been put by the narrowness of the selected view-points, while the material should rather be so arranged that it can be grasped from a multitude of points of view.

The experience of school museums and of university museums points clearly the way in which this difficulty may best be solved. A large museum might have a wing or a small group of halls set aside for the purpose of systematic instruction, where classes could be taken from one case to another, and where the essential points of view which are used in the ordinary teaching of science are utilized as the principle of installation; but the usefulness of these halls should not be overestimated, because the museum, with its mass of exhibits, is not a favorable place to obtain concentration of attention of students. That much can be attained in this manner by a small museum, and with very slender means, is shown, for instance, by the museum in Salem, Mass., which, with an annual appropriation of $8,000 (including all salaries, maintenance and purchases), has, under the able direction of Professor Edward S. Morse, done much for public education.

In cities of the size of New York or Chicago or Philadelphia, the best use of such a centralized collection can not be made. On account of the enormous distances in the city, it will very seldom be possible to assemble at any definite time in the museum a group of students who might profit by a collection of this kind. Furthermore, the collection, once installed in a large museum, and intended to serve teaching interests scattered all over the city, must necessarily be more or less stationary—and the more so, the more money is expended on excellence of installation—and can not be adapted to the needs of different schools. For this reason the system which is used in many schools, of having separate school museums which are intended to serve this purpose, is infinitely preferable, and renders entirely unnecessary the attempt to make a large institution serve primarily the demands of school classes.

To take again the example of the American Museum of Natural History, I believe it is claimed by the administration of that museum that the systematic arrangement of collections assists the public schools, and that the large appropriation which the museum receives from the city is largely justifiable for this reason. The appropriation amounts, I believe, to nearly two hundred thousand dollars annually, while the buildings without grounds represent an approximate value of three million dollars. If we imagine that only one third of this annual appropriation were used for the maintenance of school museums, and that instead of the single large complex of buildings, twenty small museum buildings were established in various parts of the city, these ends would be infinitely better subserved, and the central museum—that is, the American Museum of Natural History—would be relieved of a duty which it tries to perform, but which, owing to the very size of the institution and of the city, it can not fulfil. Such small museums would have the same relation to the main museum, that the branches of the public library have to the central library, which, through this agency, has increased its educational usefulness many times, and has diverted a certain class of demands from the central library into other channels better able to meet them.

It would be an excellent plan if that museum and others similarly supported were required to furnish school museums with the necessary material and information, leaving to the teachers of the schools the free use of the specimens, for no printed label can take the place of the freedom of selection of specimens, picked out by the teacher as occasion may arise in the course of his instruction.

For a great many years attempts have been made in France and Switzerland, and these have recently been repeated in America, to arrange small collections for public-school use, and to send these about from school to school. This attempt deserves every encouragement, although here also in our large New York schools there will be ample opportunity for the use of specimens to justify the establishment of small permanent school collections, which will be found much more economical than the constant transportation of museum material from place to place, and which may be purchased at fairly reasonable rates from dealers in teaching-material.

Even if such school museums were established, it would still be justifiable, and perhaps desirable, for the museum to maintain a few halls intended for systematic instruction; but if museums are to serve only educational purposes, then large museums are not only unnecessary, but even undesirable.

The same objections that may be raised against the wholesale elimination of large collections from the exhibits, and the retention of striking exhibits only, should also be raised against the schematization of museum material. Nothing perhaps helps more to convey the idea of completeness and of the uselessness of further effort than the presentation of a whole museum as a complete finished exhibit, in which everything has its place in a definite system. Such a museum will fail to bring home the complexity of nature and an appreciation of the efforts required for a mastery of its secrets.

The impossibility of basing museum installation on a classification of objects from a single material point of view can be made clear best by the example of anthropological exhibits. At the same time this consideration will show in what the difference between anthropological collections and natural-history collections consists. An assemblage of material such as is found in anthropological collections consists entirely of things made by the various peoples of the world—their tools, household utensils, their ceremonial objects, etc. All of these are used in the daily life of the people, and almost all of them receive their significance only through the thoughts that cluster around them. For example, a pipe of the North American Indians is not only a curious implement out of which the Indian smokes, but it has a great number of uses and meanings, which can be understood only when viewed from the standpoint of the social and religious life of the people. It even happens frequently in anthropological collections that a vast field of thought may be expressed by a single object or by no object whatever, because that particular aspect of life may consist of ideas only; for instance, if one tribe uses a great many objects in its religious worship, while among another, practically no material objects of worship are used, the religious life of these tribes, which may be equally vigorous, appears quite out of its true proportions in the museum collections. Another reason, namely the natural destruction of material, makes it quite impossible to make archeological collections systematic. Thus it happens that any array of objects is always only an exceedingly fragmentary presentation of the true life of a people. For this reason any attempt to present ethnological data by a systematic classification of specimens will not only be artificial, but will be entirely misleading. The psychological as well as the historical relations of cultures, which are the only objects of anthropological inquiry, can not be expressed by any arrangement based on so small a portion of the manifestation of ethnic life as is presented by specimens. Any one who has grasped this truth will recognize at once that an anthropological exhibit can not be cast into the single schematic mold which is to be repeated automatically the world over for every single people. With the wealth of interesting and important problems of anthropology, it is, however, perfectly easy to bring out in a popular manner one salient point here, another salient point there, according to the characteristics of the life of the people dealt with.

The difference between anthropological exhibits and those relating to natural sciences is only one of degree, because in no case do specimens alone convey the full idea that a collection is intended to express. This is particularly true in any exhibit intended to express function rather than form; as, for instance, in exhibits illustrating dynamic geology or facts relating to the physiology of plants and animals. The difference between anthropological and natural-history collections, however, consists in the trifling importance of the specimens as compared with their functional importance in anthropology, and to the fact that all the specimens are primarily incidental expressions of complex mental processes that are themselves the subject of anthropological inquiry. These latter are almost entirely missing in that field of biology which is ordinarily presented in museums.

For this reason anthropological collections should be treated like collections of artistic industry and art collections rather than like collections illustrating natural sciences.

It is therefore clear, that, so far as the public is concerned, the essential point of view of the anthropological collection and that of the natural-history collection are entirely distinct; and, if the attempt is to be made to bring out coherently the ideas underlying the anthropological exhibit, there ought to be no necessity for the visitor to come into contact with the natural-history exhibits while passing through the anthropological halls. On the whole, this end is difficult to attain in a large complex museum building; and the question may therefore be very well raised, whether it would not be better to separate entirely anthropological collections from those relating to natural history.

Still another consideration may be mentioned here, which has an important bearing upon the systematic arrangement of anthropological collections. It has been pointed out before that anthropology is essentially an historical science, and consequently not readily amenable to systematization; but, further than this, there is so much disagreement among the best anthropologists of our times in regard to the significance of anthropological data in a systematic presentation of the subject, that it seems hardly justifiable for any museum to assume to dictate by its arrangement what the approved system of anthropological science shall be.

Before further discussing the question of museum policy in regard to its relation to the public and to schools, it may be well to discuss the value of the museum as an institution intended to serve the progress of science.

The objection which is raised against the concentration of the work of the large museum in these lines rather than in educational lines is the old objection against serving the few rather than the masses. Serious educators have long since recognized that the education of the masses which we all desire is impossible without the most thorough and painstaking education of the teacher, and that the applicability of a sound educational system can not be confined to elementary schools, but that without secondary schools, colleges, universities and training schools for teachers, the whole system of public education falls to the ground. Therefore, we do not at all agree with the popular illusion that opportunities given to the few who advance science are opposed to the advancement of the masses, but we rather recognize in them an indispensable means of advancing public education.

I do not hesitate to say that the essential justification for the maintenance of large museums lies wholly in their importance as necessary means for the advancement of science. This is particularly clear in the case of the United States National Museum, which is the depository of all the government surveys, and whose duty it is to preserve the material on which the work of the surveys is based. The education of the masses can be infinitely better subserved by small museums.

What, then, is the function of the large museum? It is the only means of bringing together and of preserving intact large series of material which for all time to come must form the basis of scientific inductions. Every year shows more clearly that the loss of old collections, due to the lack of large museums until the middle of the last century, is one of the serious obstacles to the advancement of science. Museums are the storehouses in which not only must the material be preserved by means of which deductions of scientists can be checked, but they are also the place where scientific materials from distant countries, vanishing species, paleontological remains, and the objects used by vanishing tribes, are kept and preserved for all future time, and may thus be made the basis of studies which, without them, would be impossible. We are spending vast sums year after year to bring together evidences of life forms of distant countries and of past ages, to accumulate the monuments of the past and objects used by remote tribes. We collect these because they are the foundation of scientific study. Should we then be unwilling to provide adequate means for keeping intact the results of our expensive inquiries? It is the essential function of the museum as a scientific institution to preserve for all future time, in the best possible way, the valuable material that has been collected, and not to allow it to be scattered and to deteriorate.

Considering this point of view, there can be no greater misconception of the duties of a museum administrator than the belief that proper care of accumulated material is less important than beautiful exhibits. The lack of proper care of inflammable and perishable material, the constant shifting about of material not used for exhibits, the lack of conservatism in exchanging and giving away collections for elementary educational purposes, belong to the most inexcusable features of museum administration. Unfortunately the method of preservation of collections in our museums is in many cases not what it ought to be, partly from necessity, partly from choice. The crowded condition of the building, like that of the United States National Museum, or the attempt to relegate vast amounts of material to storage-rooms, as in the American Museum of Natural History, and the use of wooden receptacles for the storage of valuable material, endanger the safety of the collections and make their use temporarily or permanently difficult. Serious scientists know perfectly well that in the study of biological and anthropological phenomena observations on a single specimen are generally misleading, and that one of the great advantages gained in modern times, and based to a great extent upon the improvement of museum methods, consists in the possibility of examining long series rather than individuals. The reason for this is that the series alone can give us what is characteristic, while, when only an individual is available, characteristic traits may be overlooked, or we may be liable to consider an accidental trait as characteristic for a whole group. For this reason science is better served by the preservation of large series relating to the same question in one place rather than by scattering such series over a great many different places. This is true of all sciences, and this is the justification for the accumulation of extended material bearing upon the same point. Inroads that are made upon large collections in order to obtain scattering material otherwise not represented in the museum should be resisted by every conscientious scientist.

In order to make large series useful, the bulk of the material in a museum should be kept in such a manner that it is not only accessible at a moment's notice, but that it can also be examined from any point of view. While in zoological collections consisting of skeletons and skins, this end may be attained fairly adequately by storage in metal boxes systematically arranged and easily opened, other material can not be handled in the same manner. This is particularly true of anthropological material, which, on account of the difference in size, form and material of the objects, and on account of the multiplicity of the points of view from which the material can be viewed, can only be stored satisfactorily in such a way that each specimen can be seen.

I do not consider it necessary to discuss in greater detail the functions of the large museum as an agency in promoting science, because there can be little difference of opinion in regard to this question. Wherever investigations are undertaken that are based largely upon specimens needing preservation, the work is necessarily undertaken by a museum or by institutions closely affiliated with museums. It may be pointed out, however, that the strong tendency to accumulate specimens has often been a disadvantage in the development of anthropology, because, as was pointed out before, there are many aspects of this science in which the material objects are insignificant as compared with the actual scientific questions involved.

The experience of institutions like the Field Museum of Natural History and the United States National Museum shows clearly that the necessity of accumulating collections practically excludes important aspects of anthropological work from the field of museum activity. In former times the American Museum of Natural History followed a more liberal policy in this respect, while at present the broader point of view seems to be gradually becoming recognized in the Field Museum; but the rapid changes of policy through which these institutions have passed show that anthropology requires a broader point of view for its field-work than that offered by the strict requirements of the acquisition of museum specimens. The only institution in which the necessary freedom is offered is the Bureau of American Ethnology, which is not hampered by any requirement of accumulating specimens through its investigations.

This same point of view brings it about that museums of natural history are liable to lay much greater stress upon systematic zoology and botany than upon detailed anatomical study, the results of which can not be exhibited equally well, and that the study of functional traits is hardly ever attempted, because it offers still greater difficulties to the exhibitor.

So far as the scientific administration of museums is concerned, the principal problem is that of the extension of museum activities so as to overcome the limitations set by the tendency to acquire a considerable number of specimens.

I believe that among American museum administrators Professor F. W. Putnam deserves the highest credit for having been the first to recognize the limitations of the activity of the museum if restricted entirely by the desire for the acquisition of specimens, and for having courageously set to the museum scientific problems selected in accordance rather with their scientific importance than with the probability of yielding many specimens.

Bearing these points in view, the question arises, in how far the interests of the public and the interests of science can be harmonized. It is my opinion that the attempt at a thorough systematization of a large museum must be given up, because it is based upon a misconception of the function of the large museum. Systematic museums must be small museums.

It is very probable that in a large museum in which the systematization of the exhibit for the benefit of educational purposes is made the principal point of view the function of the individual curator will become more and more that of an officer who carries out the orders received from the general museum administration, so that there would hardly be room for investigators of the highest order in such an institution. That the systematization and popularization of the collections of a large museum does not agree with the best interests of science, has evidently been felt by the administration of the United States National Museum, in which, in the Biological Department, the work on the exhibit halls has been divorced completely from the scientific work on the collections.

The question then arises, What shall we do with our collections to make them useful to the public and at the same time useful for the advancement of science? Two methods are possible for reaching this end. Either we may have a complete separation of the collections intended for the public and of those intended for the scientist, or we may decide to make the entire collection equally accessible to the public and to the scientist.

Reasons may be brought forward in favor of either method, and it is largely a question of economy what method shall be pursued. The method adopted will also determine the form of the museum building. Unfortunately this point of view is seldom considered in planning museum edifices. Taking the example of the American Museum of Natural History, we find the whole museum, with the exception of the cellar and the top floor, which is a half-attic, laid out in large magnificent halls, which, of course, means that the whole museum is to be accessible to the public. Consequently there is no choice but to subserve in the exhibits both the aims of the scientist and those of the general public. The proportional amount of space available for storage in a building of this kind is so small that full use of the stored material for scientific purposes is entirely out of the question. The opposite point of view has been followed in the Zoological Museum in Berlin, one of the very few buildings in which the deliberate attempt has been made to separate exhibit collections from study collections. Here, however, the space allotted to the study collections is more than twice as large as the space allotted to exhibit collections.

If a museum is planned like the American Museum of Natural History, the only thing to do is to acknowledge freely that the public is to be admitted to all the collections in the museum; to arrange the collections from scientific points of view, and to set off from these collections in conspicuous places those exhibits which are intended for the public. The central aisles of the large halls, for instance, lend themselves admirably for exhibits of this type, while the side alcoves may be used to furnish the indifferent background from which the popular exhibits should be set off.

I am not by any means convinced that this is the best solution of a difficult problem. The attempt to make accessible in this way the entire collections is unnecessarily expensive; and the work that must go on in the collections, if the museum is to be a live institution at all, will tend to distract from the dignity of the halls, which I consider, so far as the public is concerned, as one of the essential features of the museum. It seems to me that while the public is admitted to a museum hall, everything in the hall should be calculated to increase the impression of dignity and of aloofness from every-day life. No dusting, no mopping, no trundling-about of boxes, should be permitted in a hall visited by the public, because it disturbs that state of mind that seems best adapted to bring home the ideas for which the museum stands.

It has been proposed to overcome the economic difficulty involved in the necessity of having large collections accessible, and the expensiveness of exhibit halls intended for the public, by placing the study collections outside of the large cities, in suburbs, where land is inexpensive, and where unpretentious buildings can be erected. This proposition has been made in England, and has been carried out by the Ethnographical Museum in Berlin. Although the separation of the exhibit collections and the storage collections involves considerable administrative difficulties, and is open to scientific objections, it is not impossible that we shall necessarily be led to the adoption of this principle of administration. While, however, the collections are concentrated in one large building, we must accept the principle that the collections must receive proper care, and must be available for scientific study. In our museum buildings with which we have to get along at the present time, this end might very well be attained by placing either in one wing or on one floor the exhibits intended for the general public, and also those intended for students in high schools, special training schools, colleges, and even for many students of universities. In collections of this kind the more advanced collections intended for students would give what I called before the indifferent background which is so necessary for the general public. A large number of halls, however, will have to be installed in a more condensed manner, perhaps by adding galleries to halls of unnecessary height, in which material could be made accessible to students. There is no reason why the public should not be admitted to halls of this kind, although presumably very few of the visitors would carry away any other impression than that of the magnitude of the field of work covered by the museum. A thorough reorganization of museum administration will not be possible until the plan of operation of the museum is decided upon before the museum building is erected, and until the small systematic educational museum, which serves as an adjunct to elementary instruction, is separated entirely from the large museum. Like the university, the large museum must stand first and last, in its relation to the public as well as in its relation to the scientist, for the highest ideals of science.

FRANZ BOAS

SCIENTIFIC BOOKS

THE COLLECTED WORKS OF GEORGE WILLIAM HILL

THE Carnegie Institution of Washington has already undertaken many forms of scientific activity during the short period of its existence. These may be divided into two classes. First, the cases where it assists science indirectly by a grant to an individual for the prosecution of some piece of work which might or might not be done without this aid; and second, the cases where some particular branch of knowledge is to be advanced or assisted by expenditure on lines which will not benefit any individual in particular, either in money or in reputation. There is considerable doubt whether an ultimate gain is to accrue to the scientific world from the former method: the danger of pauperizing research is a matter which can not be regarded lightly, for the most notable contributions have more frequently been made by those who have done their work in spite of difficulties and who, under an easier régime, would not have felt the need for exertion. Little criticism can be made on the second class of cases, where organization and a large equipment is frequently required. The routine work involved in making or collecting or publishing huge masses of data is often neither possible for an individual nor stimulating to any one who is obliged to undertake it for some definite object which he may have in view.

To the second class belongs some of the work that may be done by a publishing house whose sole concern is not the maximum financial gain to be extracted from its operations. Of this there already exist excellent English examples in the Pitt Press at Cambridge and the Clarendon Press at Oxford. It is true that these businesses are run on a commercial basis in so far as they publish books which appeal to a large circle, but they also issue works on which a considerable financial loss is expected, so that the net annual profit is not large. The trustees of the Carnegie Institution early recognized the fact that similar opportunities were needed in the United States for publications which the scientific societies are too poor to undertake, and which, for a business firm, would simply mean a gift to education. The subject of this article is one of the earliest projects of the institution; its successful completion gives reason to hope that it may be the forerunner of others on the same lines.

The memoirs of Dr. G. W. Hill occupy over seventeen hundred pages arranged in four quarto volumes. Of these just one third (Vol. III.) are taken up by his well-known theory of Jupiter and Saturn. In his preface to this work Dr. Hill says: "It was desired to abandon the use of the antiquated tables of Bouvard, and it appeared uncertain when Leverrier would publish his. The plan, therefore, was to form theories of Jupiter and Saturn which would be practically serviceable for a space of three hundred years on each side of a central epoch taken near the center of gravity of all the times of observation; theories whose errors in this interval would simply result, not from neglected terms in the developments, but from the unavoidable imperfections in the values of the arbitrary constants and masses adopted from the indications of observation." How well he succeeded is now beginning to be seen. The observations which were used in forming the tables ended with the year 1888. In memoir 76, a comparison between the results of theory and observation is given from 1889 to 1900 which shows that the mean error for each year in right ascension and declination scarcely exceeds one second of arc. And further, unlike the comparisons from the majority of astronomical tables, the errors show no tendency to increase steadily as time goes on.

But the subject which is more closely associated with Dr. Hill's name is the theory of the moon's motion. It is difficult to overestimate the services which he rendered by the publication in 1878 of the one memoir, 'Researches in the Lunar Theory.' Before this time there had been a growing feeling amongst mathematicians that the motions of the moon and planets, as subjects for investigation on theoretical lines, had been worked out and that there was little to attract a student unless he wished to take up the practical side by more accurate computations of existing developments. This false view of the situation was corrected in a single step, although it was reserved for Poincaré to show the full importance of the advance which had been made by his development of Hill's idea of the periodic solution. But the advance from the point of view of computation was not far behind, since this paper also laid a basis for the accurate calculation of the moon's motion without the excessive labor which the earlier theories would have demanded. The newly awakened interest in celestial mechanics is made sufficiently evident by the fact that over twenty treatises and text-books have appeared during the last thirty years and these quite apart from scores of original memoirs.

Connected with this paper was the memoir on the motion of the perigee of the moon, in which the idea of a determinant with an infinite number of elements to solve an infinite system of linear equations was introduced and used as a powerful instrument for accurate computation. In the introduction written by Poincaré and printed in the first of the four volumes the latter says: "Avait-on le droit d'égaler à zéro le déterminant de ces équations? M. Hill l'a osé et c'était là une grande hardiesse; on n'avait jamais jusque-là considéré des équations linéaires en nombre infini; on n'avait jamais étudié les déterminants d'ordre infini; on ne savait même pas les définir et on n'était pas certain qu'il fût possible de donner à cette notion un sens précis." * * * "Mais il ne suffit pas d'être hardi, il faut que la hardiesse soit justifiée par le succès. M. Hill évita heureusement tous les pièges dont il était environné, et qu'on ne dise pas qu'en opérant de la sorte il s'exposait aux erreurs les plus grossières; non, si la méthode n'avait pas été légitime, il en aurait été tout de suite averti, car il serait arrivé à un resultat numérique absolument différent de ce que donnent les observations." But is there not something more than the mere numerical agreement? Does not intuition, conscious or unconscious judgment, penetration into the heart of a matter—whatever we like to call it—play a large part in the selection of means to an end? It is not necessary—is it even advisable?—to stop and consider the theoretical possibilities of a new step if one feels certain it is to lead to the desired end, especially in the application of mathematics to physical problems. If astronomers had known that the series they were to use were nearly all divergent without at the same time knowing that they could still be used, would they have been inclined to undertake the enormous calculations which have resulted in our present tables for the positions of the moon and planets?

> The centipede was happy till
> One day the toad in fun
> Said, "Pray, which leg comes after which?"
> This raised his thoughts to such a pitch,
> He lay distracted in a ditch,
> Not knowing how to run.

At the same time, one does not in the least wish to depreciate the value of the labors of those, and above all of Poincaré himself, who have rendered such magnificent services to the cause of pure science by placing the methods of the applied mathematicians on a secure foundation.

In reading through the memoirs there are certain features of Dr. Hill's work which impress themselves on the mind. His power of dealing with long and complicated expressions with apparent ease is often the secret of his success. Unlike the methods in the two papers just mentioned which possess an excellent symmetry of mathematical form, expressions best adapted for computation are usually least symmetrical. And the reason for this is not difficult to understand. For the symmetry frequently implies some kind of relation between the symbols constituting the expression, which relation can often be used for abbreviating the work. In many cases one would shrink from attempting to reduce Dr. Hill's formulas to numbers, but he rarely fails to give one or more numerical examples to show how his methods can be applied. For instance, in memoir 79, which is an attempt to introduce the use of purely periodic terms to express the coordinates of the planets in terms of the time, instead of the usual method which involves secular terms, he estimates that some 2,800 special values of a certain expression will have to be computed. He immediately sets out the computation and the results for 175 of these to 13 places of decimals. And again, in the last paper on 'Dynamic Geodesy' in which he examines methods for computing the effects of the continents and seas in order to obtain a more accurate expression for the value of gravity at any place, there are five suppositions as to the distribution of the earth's mass; in each case the value of g and the deviation of the plumb line is found at several positions on the earth's surface.

The freshness and originality of Hill's work make it difficult to attach him to any particular school of mathematicians; if any such attempt is made, he belongs perhaps more closely to that of the mathematical astronomers of the latter part of the eighteenth century, and of their immediate successors. This is not unnatural, for it was to them that he owed his first inspiration. But his methods are essentially his own, even when he is expounding or using the work of his predecessors. We perhaps need more such men, lest the font of originality be choked up by the attempt to assimilate the mass of work which is being turned out every year in increasing quantities. That Dr. Hill has by no means ceased to contribute his share is shown by the last five papers contained in over a hundred pages which had not previously been published. The range of subjects is sufficiently varied. Two are continuations of memoir 79, to which reference has already been made; one is on the development of the disturbing function; one on the construction of maps, in which he sets forth a method for the better representation of large areas of the earth's surface or of the sky on paper; and the final one, also mentioned above, on geodesy.

It is to be regretted that the paper used for the reproduction is thick and unsized. In consequence of this the volumes are heavy and those who wish to make notes will find it necessary to use a sharp pencil rather than a pen if the writing is to be legible. In other respects, the printing is good and clear, and wide margins are supplied.

In closing this brief notice, I can not do better than again to refer to the introduction. In the last paragraph, M. Poincaré says: "Ainsi aucune des parties de la Mécanique Céleste ne lui a été étrangère, mais son œuvre propre, celle qui fera son nom immortel, c'est sa théorie de la Lune; c'est là qu'il a été non seulement un artiste habile, un chercheur curieux, mais un inventeur original et profond. Je ne veux pas dire que ces méthodes qu'il a créées, ne sont applicables qu'à la Lune; je suis bien persuadé du contraire, je crois que ceux qui s'occupent des petites planètes seront étonnés des facilités qu'ils rencontreront le jour où en ayant pénétré l'esprit ils les appliqueront à ce nouvel objet. Mais jusqu'ici c'est pour la Lune qu'elles ont fait leurs preuves; quand elles s'étendront à un domaine plus vaste, on ne devra pas oublier que c'est à M. Hill que nous devons un instrument si précieux."

E. W. B.

Pocket-book of Aeronautics. By Hermann W. L. Moedebeck in collaboration with O. Chanute and others. Authorized English edition translated by W. Mansergh Varley. London, Whittaker & Co. 1907. Small 8vo. Pp. 496.

Moedebeck's 'Taschenbuck für Flugtechniker und Luftschiffer,' which first appeared in 1895 as a little volume of 198 pages, besides the ruled pages for entering observations, was intended to be carried in the pocket of the experimenter or aeronaut, and although a useful compendium it was hardly known outside Germany. A new and greatly enlarged edition (which renders the name 'pocket-book' inappropriate) has just been issued, and through the generous help of Patrick Alexander, an English gentleman interested in aeronautics, who cooperated with Mr. Chanute, the eminent Chicago engineer, the revised treatise has been made accessible to English readers.

The following summary of its contents will show the scope of this useful and timely handbook. Chapter I. deals with the gases used in filling balloons, and the next chapter, by Professor Kremser, a Berlin meteorologist, treats of the physics of the atmosphere. The observations in the free air quoted were, however, obtained in Europe and no reference is made to the large amount of data collected with kites in the United States by our Weather Bureau and at the Blue Hill Observatory, nor to the more recent observations with balloons at great heights, which were instituted by this observatory. The same writer, in Chapter III., gives practical directions for making and reducing balloon observations, but the translator has confused some of the meteorological symbols. In the next chapter Major Moedebeck discusses the technique of ballooning with which Chapter VI. on ballooning might properly have been combined. Kites and parachutes are treated in Chapter V. by the Hamburg meteorologist, Professor Köppen, who was one of the first persons in Europe to experiment with kites for meteorological purposes after their usefulness had been shown at Blue Hill in 1894. It may be said that neither figure 51*b* nor 53 represents a typical Hargrave kite, H. H. Clayton having invented this form with four continuous corner-sticks. Alexander Graham Bell's tetrahedral kite is omitted from the types described, notwithstanding the fact that the fame of the inventor has attracted wide attention to it. No mention is made of the practise of using larger wire for the lower portions of the line which enables great heights to be attained by attaching successive kites. The bibliography should include the important memoir by S. P. Fergusson, describing the perfected equipment at Blue Hill, which was published in *Annals of Harvard College Observatory,* Vol. 43, Part 3. In Chapter VII. balloon photography is discussed by Professor Miethe, an eminent authority, as is, in the following chapter by Professor Kutta, the allied subject of photographic surveying. Next comes a detailed account, by Major Moedebeck, of the history and present status of military ballooning in the different countries of the world. The editor's technical knowledge of the subject gives authority to his estimate of the value of the future air-ship in warfare.

The remainder of the book is mainly devoted to dynamical aeronautics. Professor Müllenhoff analyzes briefly the principles of animal flight in Chapter X., and in the first part of the next one, Major Moedebeck gives the history of man's attempts at flight. In the same chapter a paper by the late Otto Lilienthal on artificial flight is followed by Mr. Chanute's account of the modern experiments where one looks in vain for any mention of the remarkable machines of the late Professor Langley. In Chapter XII. Major Moedebeck describes the air-ship or motor-balloon, in the list of whose performances, by some error, the drifting race of spherical balloons in 1906 for the Gordon-Bennett cup has been included, with the name of the winner strangely distorted. The next three chapters, on flying-machines, their motors and screws, are by the well-known Austrian expert, Major Hoernes. Chapter XVI., the last one, contains a convenient list of the aeronautical societies of the world and an appendix has a useful collection of tables and formulæ. The index is inadequate to so much material, but, in spite of this and some minor defects, the work can be highly recommended to the increasing number of persons interested in the investigation or navigation of the air, as the best existing treatise on this rapidly-developing subject.

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SOCIETIES AND ACADEMIES

THE AMERICAN CHEMICAL SOCIETY. NORTHEASTERN SECTION

THE seventy-seventh regular meeting of the section was held at the State Mutual Restaurant, Worcester, Mass., on May 18, at seven o'clock P.M. The paper of the evening was upon 'Ceramics,' by Dr. Frederic Bonnet, Jr. The speaker first referred to the importance of the clay-making industry, it being the third in magnitude and only surpassed by those of coal and iron. The value of clay products in 1905 reached the immense sum of $145,697,188. Of this, brick represent nearly one half. Clay consists of naturally occurring earthy materials having more or less plasticity when wet, and which, when heated to redness or higher, becomes hard and rocklike. Clays are of secondary origin, and are the product of the decomposition of feldspathic or serpentine rocks. Brongniart, and also Dr. Cushman, of the U. S. Department of Agriculture, testing laboratory for road materials, have made researches which indicate that the decomposition of the feldspar is a kind of electrolysis, in which the alkali constituent passes into solution, leaving the alumina and silica. The noted deposits of Cornwall, England, Zettlitz in Bohemia and certain deposits in Germany, however, appear to have resulted from the action of acid vapors on feldspar. Deposits formed by weathering are usually shallow and the original feldspar is found beneath. True kaolin is formed from feldspar and is essentially a basic hydrated aluminum silicate. If the clay has been transported by water and again deposited, it usually contains some impurities; if little iron is present and the clay is tough and plastic, it is called ball clay. The cause of plasticity is not fully understood and no entirely satisfactory theory has been advanced. One of the most recent, the colloid theory, fails to meet the case and does not explain the cohesiveness of a ball clay. The history of pottery is, to some extent, the history of man; from the crude pots of primitive races to the decorative ware and porcelain of advanced civilization. Clay is often used just as it is found for brick, tile and common pottery, but for all better ware it needs selection and preparation. In the finest ware and for some special purposes, it is subjected to very fine grinding and mixing, or long tempering and ageing. The effect of silica on the fusion point of clay is very important; pure kaolin fuses at temperatures about 1,800° C., or higher, but free silica lowers this, and hence should not be present in too great an amount in fire-clays. But metallic oxides are the most noticeable fluxes in clays; the fusion point decreases as the percentage of bases rises. But the bases exert this depressing effect on the fusion point in proportion to their chemical equivalence; thus 40 MgO has the same lowering effect as 56 CaO, or 62 Na_2O. This is called the Law of Richter, but it does not apply to glazes where the amount of fluxes is large. Fusibility of clay is determined by test pieces (Seger cones), or by pyrometry. The Seger cones are made of pure clay, mixed with fluxes in such graded proportions that the fusion temperature of the consecutive numbers are about 20° C. apart. The No. I. cone fuses at the same temperature as an alloy of one part platinum and nine parts gold, *i. e.*, at 1,150° C. Since this temperature is rather high, Cramer and Hecht prepared cones containing B_2O_3 and PbO, fusing at definite temperatures down to 590° C. These cones give the true measure of the heat effect, but not necessarily the exact temperature, and hence are more useful to the potter than is the pyrometer, since they show the effect which will be produced on the ware. But cones do not show the temperature below 590° C., *e. g.*, at 200° to 400° C., when the water is given off from the clay, nor can they indicate anything as to the rate of cooling of a kiln, which is often important in reference to producing, or preventing crystallization of the glaze. The main difference between the glaze and the body of the ware is one of fusion temperature; the former fuses completely and is essentially a glass. A good glaze must have proper expansibility, to neither chip off nor crack (craze) upon the surface of the ware; and not be attacked by water or ordinary acids, especially for culinary ware, and must be hard to resist wear. The ordinary salt glaze on stoneware and the hard glaze on true porcelain meet all these conditions, but all other glazes fail in some degree. Four types of glaze are in common use: alkaline or salt glaze, feldspathic, lead, and stanniferous (enamels). The general formula for glaze is $xRO, yR_2O_3 \cdot zSiO_2$, where RO = sum of metallic fluxes (CaO, MgO, K_2O, etc.); R_2O_2 is usually Al_2O_3 or the sum of Al_2O_3, Fe_2O_3, and Cr_2O_3; some of the SiO_2 may be replaced by TiO_2, SnO_2, etc. In the raw glaze, insoluble substances are finely ground and suspended in water, into which the ware is dipped. A fritted glaze has its materials partially fused and combined before grinding for the dipping. Thus a fritted glaze is made from soluble substances, or those of high specific gravity which would tend to segregate when the ware is dipped. The fusion of silicates results in the formation of igneous solutions holding the ingredients dissolved, and the temperature of fusion is lowest when several silicates are thus mixed. The more complex glazes and slags are the most fusible.

During the afternoon, before the meeting, parties were formed to visit the following manufacturing plants in Worcester: American Steel and Wire Company; Graton and Knight, Tannery and Leather-belting Company; the Worcester Sewage Plant; and the Polytechnic Institute Laboratories and Electrical Engineering Building. Later, the members of the Section were entertained at afternoon tea by Professor and Mrs. Leonard P. Kinnicutt at their home on Elm Street.

Specimens of various kinds of pottery were shown, among which were some from the Art Students' Club of Worcester, and examples of crystalline glazes from the New York State School of Ceramics, at Alfred.

FRANK H. THORP,
Secretary

THE TORREY BOTANICAL CLUB

THE meeting of April 24, 1907, was called to order at the museum building of the New York Botanical Garden, at 3:40 P.M., with Dr. M. A. Howe in the chair. Twenty persons were present.

The following scientific program was presented:

Ecological Distribution of the Beach and Dune Flora about Chicago, Ill.: Miss MARY PERLE ANDERSON.

Miss Anderson gave a brief account of the geological history of the ancient Lake Chicago and its succession of beaches, the Glenwood, the Calumet and the Toleston. These ancient beaches were formed by changes in the lake-level, and at the present time are indicated by ridges of wooded land more or less parallel to the present coastal beach of Lake

Michigan. The ridges are separated by the low level prairie land which makes up the Chicago Plain.

The formation of the dunes along the present shore of the head of Lake Michigan was considered, and also the changes in the flora that may be noted as one passes from the naked shifting dunes and extremely xerophytic conditions of those recently fixed, to the dunes farthest inland where mesophytic conditions prevail. Certain grasses, species of *Calamagrostis, Andropogon, Ammophila, Elymus,* do much to bind the dunes. The first trees to appear are the cottonwood and certain willows which are also of value in fixing the dunes. The scrub-oak and black-oak soon appear and are followed by the bur-oak, the white-oak, and the red-oak. *Pinus Banksiana* is followed by the white pine; the pig-nut hickory is succeeded by the shag-bark; other trees, such as the basswood, ash, cherry and black walnut, come in, and on the most mesophytic slopes of the oldest dunes and beaches one finds the sugar maple and, more rarely, the beech, hemlock, and southern tulip-tree. Corresponding changes in the shrubby and herbaceous vegetation occur, and at Stevensville and Porter, one may pass, in a short time, from extreme desert conditions through successive stages of the open forest of low trees and shrubs to the oak-hickory type and finally to the beech-maple-hemlock combination, which indicates the culmination of the forest in this region.

The usual ecological factors, heat, light, water, soil, wind, and direction of slope all have their influence in the floral distribution. Conditions in the dunes are extreme. Thus, for example, the trailing-arbutus and the bearberry, both northern types, may appear on the north-facing slope of a dune, while just over the crest, on the south-facing slope, the cactus may flourish.

Emphasis was laid on the fact that species vary with environment, often losing more or less of their xerophytic adaptations under mesophytic conditions; that a plant-society is only a stage in the development of a region; that the apparent tendency is for all to approach the mesophytic condition.

The paper was discussed by Dr. Grout and Dr. Rydberg.

Some Relations between Habitat and Structure in Mosses: Dr. A. J. GROUT.

Xerophytic mosses apparently tend to develop short, thick-walled cells, often with papillæ over the lumen. Nearly all mosses with papillæ over the lumen of the cell are xerophytic, or belong in groups that are largely xerophytic. Presumably the papillæ tend to retard transpiration.

Pleurocarpous mosses growing on trees tend to develop short thick-walled cells, especially at the basal angles, and a similarity of leaf structure in tree-growing mosses due to this fact has produced much of the confusion and uncertainty in classifying such mosses, *e. g., Alsia, Dendroalsia, Bestia, Groutia* and their relatives.

Tree-growing mosses also tend to develop erect capsules, and the correlated imperfect peristomes. To some extent this seems to apply to other xerophytic mosses.

Aquatic or subaquatic pleurocarpous mosses have an apparent tendency to develop enlarged and inflated alar cells.

Cleistocarpous and gymnostomous mosses appear, for the most part, to be mosses of various relationships adapted to damp soil, not closely covered with other vegetation, and best suited to support a rather short-lived annual moss.

The speaker recognized numerous exceptions to the above relationships, if stated as general principles, but, stated as tendencies, he believes they are worthy of serious consideration by the systematist, the morphologist, and the ecologist.

A brief discussion followed.

C. STUART GAGER,
Secretary

DISCUSSION AND CORRESPONDENCE

ELIMINATION OR FIRST SPECIES

HAVING followed the discussion of the proposed new rules of zoological nomenclature in the pages of SCIENCE, I feel that I voice the opinion of many zoologists when I say 'a plague o' both your houses.' For thirty years

I have been looking for fixity in zoological names, but that desirable condition seems further off than ever. It is all very well to indulge in these antiquarian researches, these games of taxonomic logomachy, if they be recognized as such, but the players fail to realize one thing: Names of animals and plants are but means for easy reference; nomenclature is not the end and object of all biological science.

The sanest word in all this discussion has, in my opinion, been said by Dr. Williston. This digging up of forgotten screeds means but the relegating of the great masters of the past to a secondary position; this framing of ex post facto laws offers a precedent for the future subject of that intolerable disease once known as the 'mihi itch' to set aside as lightly the laborious schemes of the sciolists of to-day.

Biologists may apparently be divided into two groups: One contains those who find great enjoyment in renaming things already well named and who regard names as the object of all science. The other group have something to tell us about animals and plants and they regard names merely as means of identification of the forms referred to. Certainly they have some rights which should be considered. Must they run through the gamut of *Triton, Triturus, Molge,* etc., every time the systematist changes his mind? Must I know the mental make-up—radical or conservative—of the biologist to know what he means when he refers to *Uca* or to *Acer saccharinum?* An article deals with *Esox;* does it treat of a pike or a needle fish?

The safest way for the morphologist or the ecologist is to stick to the well-accepted, time-honored names and to utterly ignore the vagaries of the nominalist. The question once was 'Who reads an American book?' If the present tendency continues it will soon be 'Who can read an American biological work?' It would be most desirable that at the coming Zoological Congress a morphologist or two should be added to the committee on nomenclature to act as a balance wheel.

J. S. Kingsley

A CORRECTION

To the Editor of Science: A statement on page 452 of Science of March 22 requires a rectification in the interest of the unprejudiced reader.

The sentence in question reads as follows:

> These results show conclusively that magnesium sulphate in proper dilution is beneficial to the growth of seedlings, and that any inhibitory effects are due to the presence of excessive amounts, thus controverting Loew's theory that magnesium salts when alone in solution are always injurious to plant growth.

Permit me the following remarks regarding this remarkable sentence:

1. It is not a *theory* that magnesium salts act poisonously on plants; it is a *fact.*

2. Not only Loew, but also others have observed the same fact. Loew has merely furnished an explanation well in accord with certain observations.

3. The doses at which magnesium salts, applied alone, are poisonous for plants can *impossibly* be called *excessive,* since even at 0.02 per cent. a poisonous action of magnesium salts on algæ can be observed, while calcium nitrate is not in the least injurious for algæ at even 1 per cent.

4. It is a well-known fact that many compounds that act poisonously at a certain concentration can act in very high dilution as stimulants of growth.

5. It is erroneous to attribute this stimulating action to any nutritive quality of the poison.

The unprejudiced reader who desires some information as to the nutritive rôle of magnesium salts in plants and to the conditions under which this function can be performed, is kindly requested to consult Bulletin No. 45 of the Bureau of Plant Industry, 'The Physiological Rôle of Mineral Nutrients in Plants,' Washington, 1903.

O. Loew

Imperial University of Tokyo, Japan,
April, 1907

SPECIAL ARTICLES

THE BEHAVIOR OF THE SEEDLINGS OF CERTAIN VIOLET HYBRIDS

During the summer of 1906 I raised plants from the seeds of twenty-five different hybrids of *Viola,* and also from the seeds of about twenty pure species. The behavior of these two classes of seedlings was surprisingly unlike; the offspring of the pure species resembled each other closely, but the offspring of the hybrids were often much unlike each other and unlike their immediate parents, reverting in some qualities to one parent of the hybrid and in other qualities to the other parent, and this in a great variety of ways.

The species involved in these experimental cultures all belong to the group commonly known as 'blue stemless violets,' of which *V. palmata, V. cucullata* and *V. sagittata* are familiar examples. Of this group there are about twenty species in the northeastern United States. I may not repeat here the proof, published elsewhere,[1] that these closely allied species, when growing together, freely interbreed. I will merely say that the facts to be presented in the present paper furnish most positive confirmation of the opinion that these anomalous plants are hybrids.

One of the corollaries of Mendel's law is that each pair of contrasting characters in a hybrid works out its effects, for the most part, independently of all other pairs. As in Newton's 'Law of the Coexistence of Motion,' the final result is but the summing up of the various component movements taken separately. It will be simpler for us, therefore, in describing the behavior of violet hybrids, to consider each pair of characters by itself, taking up, in order, the divergence that occurs in respect to leaf-outline, in respect to pubescence, in respect to size, and lastly in respect to color of capsules and of seeds.

1. A striking illustration of diversity in leaf-form was seen in the offspring of *Viola cucullata* × *septemloba.* This hybrid was published by Mr. Bicknell in September, 1904, as a species, *V. notabilis.* It has been found in five different stations, always growing with the reputed parents. In June, 1904, I received from New Jersey one of these plants that I have grown now for three seasons. From its cleistogamous capsules, which of necessity give pure cultures, I collected seeds in 1905 that furnished the following summer ten vigorous plants. These bore in August and September an abundance of cleistogamous flowers that matured seeds; several plants bore also in the autumn petaliferous flowers.

The leaves of the parent species are very dissimilar, that of *V. cucullata* being broadly heart-shaped and pointed, that of *V. septemloba* (*V. Brittoniana*) primarily 3-parted, with the segments 2–4-lobed. The hybrids of these two species in all the five known stations exhibit a fair compromise in leaf-outline between the two quite unlike leaves of the parents, and closely resemble each other. They show about the same number of lobes as in *V. septemloba,* but the lobes are shorter and broader, the sinuses only half as deep. But in the offspring of this hybrid we have in addition to plants with this compromise leaf-form, plants with leaves but slightly lobed and showing the cordate base and acute apex of *V. cucullata,* and still other plants in which the leaf-outline is almost a complete reversion to *V. cucullata.* In the living plants that display, each, twelve or more leaves of these several patterns, the impression of dissimilarity is most striking.

Another marked case of diversity of leaf-outline in the progeny of the same hybrid was seen in *V. fimbriatula* × *septemloba.* This is Mr. Pollard's *V. Mulfordæ,* found first in 1902 at Hempstead Plains, Long Island. It has turned up during the past season in two other stations, but always in close proximity to the two parents. As before, the leaf of the hybrid is markedly intermediate between the very unlike leaves of the parent species; but not so with the leaves of several plants that I raised from this hybrid the past summer. The seedlings all came from the self-fertilized capsules of plants sent by Miss Mulford from the type station, but appeared most dissimilar in foliage, though growing side by side under the same cultural conditions. Some plants bore leaves like those of the parent hybrid; others bore leaves resembling those of *V. septemloba* in width, but those of *V. fimbriatula* in having only basal lobes; and still other plants were in leaf-pattern complete reversions to *V. fimbriatula.*

[1] See *Rhodora,* VI., 213–223; VIII., 6–10, 49–60.

2. I would speak, secondly, of diversity in hybrid seedlings as respects pubescence. When one parent is pubescent and the other glabrous, the hybrids of the first generation, such as we usually find in the field, are as a rule intermediate in this character, being *somewhat* pubescent. In certain large colonies, where the plants appear to have been long established, and to have produced offspring, we find notable reversions. I have visited several stations where *V. fimbriatula* and *V. sagittata* grew in abundance, and where many plants were to be seen having the leaf-outline of *V. fimbriatula* with the glabrous character of *V. sagittata;* and conversely, many having the leaf-outline of *V. sagittata* with the hairiness of *V. fimbriatula.* Among cultivated seedlings of hybrids this reversion was seen in *V. fimbriatula* × *septemloba,* referred to above. The plants from Long Island and from Connecticut are all more or less pubescent; but among the five seedlings that I raised from the Long Island plant, two that in leaf-outline revert to *V. fimbriatula* are quite as glabrous as *V. septemloba,* retaining only the fine ciliation that appears on the margin of the leaves in both these species.

3. As respects diversity in size, I have a notable instance in the seedlings of *V. papilionacea* × *villosa,* two species that are respectively the largest and the smallest plants of the group. The mother plant was collected near Philadelphia by Mr. Witmer Stone. The five seedlings that I raised from it grew side by side under the same external conditions; but three of the plants had leaves twice as broad as the leaves of the other two.

4. One more particular in which I have found hybrid seedlings of *Viola* to differ from one another is in the color of the seeds and of the cleistogamous capsules. In about one half the species of this group these capsules are commonly blotched or dotted with purple. The hybrid of any of these with a green-fruited species bears ordinarily a capsule of an intermediate color. But in the offspring of the hybrid the capsule is frequently seen to have the pure green of the one grandparent, or the normal purple of the other.

But this color reversion is more strikingly exhibited in the behavior of the seeds of some of these hybrid offspring. Our species of 'blue stemless violets' vary much in the color of the seeds; and these colors are quite constant in the same species, as seen in specimens growing a thousand miles apart. For example, *V. cucullata* and *V. papilionacea* have dark brown, almost black, seeds; *V. fimbriatula* has nut-brown seeds; *V. affinis, V. villosa*[2] and *V. septemloba* seeds of a light straw-color. Now, when two species with different colored seeds are crossed, the color of the seeds of the first-cross is usually a mean between the colors of the parents. In a hybrid from Ivy Hill Cemetery, Philadelphia, black-seeded *V. papilionacea* is crossed with pale-seeded *V. villosa,* and produces brown seeds. But in seven plants raised from these brown seeds, four had the dark-colored seeds of *V. papilionacea* and three the light-colored seeds of *V. villosa.* In five seedlings of one plant of *V. affinis* × *cucullata,* two bore seeds quite as pale as those of *V. affinis;* the remaining three, however, bore not the black seeds of the other grandparent, but the brown seeds of the mother-hybrid. This divergence in seed-color also appears in the seedlings of the two hybrids used to illustrate diversity in leaf-pattern. In ten seedlings of *V. cucullata* × *septemloba,* four ripened dark-colored seeds; six, light-colored seeds. In five seedlings of *V. fimbriatula* × *septemloba,* three bore the brown seeds of *fimbriatula* and two the straw-colored seeds of *V. septemloba.*

I am not yet able to state definitely what proportion of violet hybrids produce heterogeneous offspring. In some cases the number of plants raised was too small to admit of a satisfactory conclusion regarding this tendency. But in several instances there were strong indications that the hybrid was stable, and produced offspring quite like itself. The most noteworthy instance was that of *V. affinis* × *septentrionalis,* of which I raised twenty-four seedlings in 1904, and the past season from the seeds of these, many plants of a third generation. The meager pubes-

[2] The species here referred to is the *V. villosa* of recent authors, which is probably not *V. villosa* Walter.

cence, the brown seeds, the impaired fertility of the original hybrid plant, have remained unchanged.

Such behavior is by no means rare among blend-hybrids in other genera than *Viola*. Willow hybrids, for example, are said to produce stable offspring always like the parent hybrid. Dr. MacDougal states[3] that "more than a thousand such fixed hybrids or hybrid species are known." In fact, de Vries is of the opinion that this procedure takes place in all hybrids between pure species—that is, species that differ from each other by no 'varietal' character. In such hybrids the differences of the parents are so thoroughly blended, that they do not disunite in the germ-cells, as in hybrids governed by the law of Mendel, and therefore the offspring simply repeat the form of the parent hybrid.

One further observation is of interest—a tendency in certain individual seedlings to recover from the marked impairment of fertility that characterizes nearly all violet hybrids. It is well known that partial or complete sterility is usually found in a hybrid, when the parent forms differ from each other in several or many characters; but that when the differences are few, especially when only one or two (as often between a species and its variety), there is seldom any loss of fertility. We are further taught by Mendel's law, that when the parents of the first cross differ in more than one character, the majority of the offspring will be hybrid in fewer characters than the parent; in fact, if the offspring be sufficiently numerous, there will be found a certain percentage[4] of forms that have no hybrid character, but various combinations of the pure characters of the two parents; and such forms, though often new, will prove stable when reproduced by self-fertilized seed. With this diminution, or entire loss of hybridity, we should naturally expect a partial or total recovery from the impairment of fertility produced in the first-cross. At any rate, it is an observed fact that many violet seedlings, whose hybrid parents produced seed from only about one tenth of their ovules, are themselves normally fertile.

We have, then, in our blue stemless violets a rather large group of closely allied species that freely interbreed, producing 'blend-hybrids,' that is, hybrids in which the differences of the parents appear in a compromise form. While it may occur in *Viola*, there has been observed no instance of what Mendel calls 'dominance'—the appearance in the hybrid in full force of a character of one of the parents to the suppression of the contrasting character of the other parent. Even differences in respect to pubescence, or in respect to color of capsules or of seeds, are in violet hybrids represented in an intermediate condition, though these differences in other genera usually give rise to dominance.

Nevertheless, in the behavior of their offspring many violet hybrids obey the Mendelian law of segregation. The compromise effected in the sporophyte stage between the conflicting characters of the parentage, is annulled when the plant passes into the gametophyte stage; the germ-cells are for the most part pure, and the offspring heterogeneous, consisting of reversions to the original species, of new and stable forms, and of various hybrid forms. This is by no means a novel phenomenon in the history of hybridism. Professor Castle says,[5] "dominance is purely a secondary matter; it may, or may not, occur along with segregation." Professor Bateson says,[6] "the applicability of the Mendelian hypothesis has, intrinsically, nothing to do with the question of the inheritance being blended or alternative." Numerous instances might be cited; but it may be questioned, if in any other group of plants the phenome-

[3] 'Heredity, and the Origin of Species,' p. 8.

[4] The average ratio of such forms to the whole number of offspring is expressed by the fraction $\frac{1}{2^n}$ where n is the number of differences between the parents of the first cross; this fraction also expresses the proportion of the forms that exactly resemble the parent hybrid; the remainder, or the offspring with reduced hybrid characters, will be, respectively, 0, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{7}{8}$, $\frac{15}{16}$, etc., when the differences of the grandparents are, respectively, 1, 2, 3, 4, 5, etc.

[5] Mark Anniversary Volume, p. 383.

[6] 'Mendel's Principles of Heredity,' Smithsonian Report, 1902, p. 574.

non is more extensively and clearly exhibited than in *Viola*. Ezra Brainerd

Middlebury, Vermont

FORMULAS FOR THE COMPARISON OF ASTRONOMICAL PHOTOGRAPHS

The present paper contains formulas suitable for the direct comparison of rectangular coordinates measured on different astronomical negatives. The problem here involved supplements what may be called the fundamental transformations in the reduction of celestial photographs; *viz.*, the calculation of right-ascensions and declinations from rectangular coordinates, and rectangular coordinates from right-ascensions and declinations. The writer has published formulas for all these transformations in 'Tables for the Reduction of Astronomical Photographs,' Contrib. Obs. Col. Univ., No. 23. In these formulas the problem is solved by expansion into series, taking advantage of the fact that the photographs under consideration cover but a very small part of the sky, so that measured coordinates may be regarded as small quantities.

It is sometimes desirable to compare rectangular coordinates of the same stars measured on two different overlapping photographs without computing right ascensions and declinations. For instance, Donner used this method for strengthening his determination of plate-constants in his reduction of the astrophotographic catalogue plates ('Sur le Rattachement des clichés astrophotographiques,' *Acta Soc. Sci. Fenn.*, Tom XXI., No. 8). Another important application will doubtless occur in the calculation of the solar parallax from Eros observations by the diurnal method.

For these reasons, the writer has thought it desirable to expand directly the x and y of a star on one plate in terms of its x and y on a second plate. The resulting series, though clumsy in appearance, are rapidly convergent, and in most practical cases, convenient in use. As here given, all terms to the fifth order, inclusive, have been retained; but a table is attached to the formulas showing the declination at which any term may be omitted in actual applications of the method. When this declination is greater than 75°, the table contains the number 75+. Inasmuch as we require a precision of 0″.01 up to 75° declination, the table has been arranged so as to exclude only terms less than 0″.005.

To obtain the desired expansions, we let:

x_1, y_1, be the coordinates of a star on a correctly oriented plate whose center corresponds to the right-ascension α_1 and declination δ_1 on the sky.

x_2, y_2, be the coordinates of the same star on a second correctly oriented plate whose center corresponds to the right-ascension α_2 and declination δ_2 on the sky.

M_1, M_2, . . . N_1, N_2, . . . be certain auxiliary quantities, constant for all stars on a given pair of plates.

If we now put:

$$d\alpha = \alpha_1 - \alpha_2, \quad d\delta = \delta_1 - \delta_2, \quad \delta = \tfrac{1}{2}(\delta_1 + \delta_2),$$

we can express x_2, y_2, in terms of x_1, y_1, as follows:

$$(1)\left\{\begin{aligned} x_2 = {} & x_1 + M_1 + M_2x_1 + M_3y_1 + M_4x_1^2 + M_5x_1y_1 \\ & + M_6y_1^2 + M_7x_1^3 + M_8x_1^2y_1 + M_9x_1y_1^2, \\ y_2 = {} & y_1 + N_1 + N_2x_1 + N_3y_1 + N_4x_1^2 + N_5x_1y_1 \\ & + N_6y_1^2 + N_7x_1^2y_1 + N_8x_1y_1^2 + N_9y_1^3. \end{aligned}\right.$$

Expressions for the M's and N's, with the table mentioned above, are given at the end of the present paper. The writer is under special obligations to Mr. G. W. Hartwell, assistant in mathematics, Columbia University, for help in this part of the work. Demonstrations are omitted here, because the formulas can be verified satisfactorily by means of a numerical example, such as the following particularly unfavorable one. Let us assume two plates and an imaginary star such that:

$$\begin{aligned} &\alpha_1 = 0° \ 0' \ 0''.00, && \alpha_2 = 2° \ 0' \ 0''.00, \\ &\delta_1 = 74° \ 0' \ 0''.00, && \delta_2 = 75° \ 0' \ 0''.00, \\ &x_1 = +3600'', && \delta = 74° \ 30' \ 0''.00. \\ &y_1 = +3600'', && \end{aligned}$$

The right ascension and declination of the imaginary star, which we will call A and D, can then be computed readily from x_1, y_1, α_1, δ_1, by means of our former series published in Contrib. Obs. Col. Univ., No. 23.

VALUES OF M'S AND N'S, WITH LIMITING DECLINATIONS.

	Decl. at which Term can amount to 0″.005					
	$x_1=30'$, $y_1=30'$			$x_1=1^\circ$, $y_1=1^\circ$		
	$da \cos\delta=10'$ $d\delta=10'$	30′ 30′	1° 1°	10′ 10′	30′ 30′	1° 1°
$M_1=+da\cos\delta$	0°.0	0°.0	0°.0	0°.0	0°.0	0°.0
$-1/2\, da\cos\delta\, d\delta\, \delta \tan \sin 1''$	0.3	0.0	0.0	0.3	0.0	0.0
$+3/8\, da\cos\delta\, d\delta^2 \sin^2 1''$	75+	0.0	0.0	75+	0.0	0.0
$-1/6\, da^3\cos^3\delta\,(\tan^2\delta-2)\sin^2 1''$	70.4	0.0	0.0	70.4	0.0	0.0
$-11/48\, da\cos\delta\, d\delta^3\tan\delta\sin^3 1''$	75+	75+	48.7	75+	75+	48.7
$+1/12\, da^3\cos^3\delta\, d\delta\tan\delta\,(\tan^2\delta-2)\sin^3 1''$	75+	75+	62.3	75+	75+	62.3
$+19/128\, da\cos\delta\, d\delta^4\sin^4 1''$	75+	75+	75+	75+	75+	75+
$-1/16\, da^3\cos^3\delta\, d\delta^2\,(3\tan^2\delta-4)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+1/120\, da^5\cos^5\delta\,(16-13\tan^2\delta+\tan^4\delta)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_2=+1/2\, d\delta^2\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-1/2\, da^2\cos^2\delta\,(\tan^2\delta-2)\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-1/8\, da^2\cos^2\delta\, d\delta^2\,(5\tan^2\delta-7)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+1/24\, da^4\cos^4\delta\,(\tan^4\delta-13\tan^2\delta+16)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+5/24\, d\delta^4\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_3=-da\cos\delta\tan\delta\sin 1''$	0.1	0.0	0.0	0.0	0.0	0.0
$+1/2\, da\cos\delta\, d\delta\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-7/8\, da\cos\delta\, d\delta^2\tan\delta\sin^3 1''$	75+	75+	30.8	75+	67.3	16.6
$+1/6\, da^3\cos^3\delta\tan\delta\,(\tan^2\delta-5)\sin^3 1''$	75+	74.0	34.7	75+	71.7	17.7
$-1/12\, da^3\cos^3\delta\, d\delta\,(\tan^2\delta-5)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+23/48\, da\cos\delta\, d\delta^3\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_4=da\cos\delta\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$+1/2\, da\cos\delta\, d\delta\tan\delta\sin^3 1''$	75+	75+	64.4	75+	64.4	27.6
$+7/8\, da\cos\delta\, d\delta^2\sin^4 1''$	75+	75+	75+	75+	75+	75+
$-2/3\, da^3\cos^3\delta\,(\tan^2\delta-2)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_5=d\delta\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-3/2\, da^2\cos^2\delta\tan\delta\sin^3 1''$	75+	70.3	34.9	75+	34.9	9.9
$+5/6\, d\delta^3\sin^4 1''$	75+	75+	75+	75+	75+	75+
$-1/4\, da^2\cos^2\delta\, d\delta\,(5\tan^2\delta-7)\sin^4 1''$	75+	75+	75+	75+	75+	74.7
$M_6=-da\cos\delta\, d\delta\tan\delta\sin^3 1''$	75+	75+	46.3	75+	46.3	14.6
$+1/2\, da\cos\delta\, d\delta^2\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+1/2\, da^3\cos^3\delta\tan^2\delta\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_7=1/6\, da^2\cos^2\delta\,(\sec^4\delta+6)\sin^4 1''$	75+	75+	75+	75+	75+	70.7
$M_8=2\, da\cos\delta\, d\delta\sin^4 1''$	75+	75+	75+	75+	75+	75+
$M_9=d\delta^2\sin^4 1''$	75+	75+	75+	75+	75	75+
$N_1=d\delta$	0.0	0.0	0.0	0.0	0.0	0.0
$+1/2\, da^2\cos^2\delta\tan\delta\sin 1''$	0.3	0.0	0.0	0.3	0.0	0.0
$-1/4\, da^2\cos^2\delta\, d\delta\,(\tan^2\delta-1)\sin^2 1''$	65.8	0.0	0.0	65.8	0.0	0.0
$+1/3\, d\delta^3\sin^2 1''$	75+	0.0	0.0	75+	0.0	0.0
$+1/4\, da^2\cos^2\delta\, d\delta^2\tan\delta\sin^3 1''$	75+	75+	46.3	75+	75+	46.3
$-1/24\, da^4\cos^4\delta\tan\delta\,(\tan^2\delta-5)\sin^3 1''$	75+	75+	69.7	75+	75+	69.7
$+1/48\, da^4\cos^4\delta\, d\delta\,(\tan^4\delta-6\tan^2\delta+5)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+2/15\, d\delta^5\sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_2=da\cos\delta\tan\delta\sin 1''$	0.1	0.0	0.0	0.0	0.0	0.0
$+1/2\, da\cos\delta\, d\delta\sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$+7/8\, da\cos\delta\, d\delta^2\tan\delta\sin^3 1''$	75+	75+	30.8	75+	67.3	16.6
$-1/6\, da^3\cos^3\delta\tan\delta\,(\tan^2\delta-5)\sin^3 1''$	75+	74.0	34.7	75+	71.8	17.9
$-1/12\, da^3\cos^3\delta\, d\delta\,(\tan^2\delta-5)\sin^4 1''$	75+	75+	75+	75+	75+	75+
$+23/48\, da\cos\delta\, d\delta^3\sin^4 1''$	75+	75+	75+	75+	75+	75+

VALUES OF M'S AND N'S, WITH LIMITING DECLINATIONS.

	Decl. at which Term can amount to 0″.005					
	$x_1 = 30'$, $y_1 = 30'$			$x_1 = 1^\circ$, $y_1 = 1^\circ$		
	$da \cos\delta = 10'$, $d\delta = 10'$	30′, 30′	1°, 1°	10′, 10′	30′, 30′	1°, 1°
$N_3 = -1/2\, da^2 \cos^2\delta\, (\tan^2\delta - 1) \sin^2 1''$	0°.0	0°.0	0°.0	0°.0	0°.0	0°.0
$+ d\delta^2 \sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-3/4\, da^2 \cos^2\delta\, d\delta^2\, (\tan^2\delta - 1) \sin^4 1''$	75+	75+	75+	75+	75+	75+
$+1/24\, da^4 \cos^4\delta\, (\tan^4\delta - 12 \tan^2\delta + 5) \sin^4 1''$	75+	75+	75+	75+	75+	75+
$+2/3\, d\delta^4 \sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_4 = da^2 \cos^2\delta \tan\delta \sin^3 1''$	75+	75+	46.3	75+	46.3	14.6
$+1/2\, da^2 \cos^2\delta\, d\delta \sec^2\delta \sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_5 = da \cos\delta \sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$+3/2\, da \cos\delta\, d\delta \tan\delta \sin^3 1''$	75+	70.3	34.9	75+	34.9	9.9
$+15/8\, da \cos\delta\, d\delta^2 \sin^4 1''$	75+	75+	75+	75+	75+	75+
$-1/6\, da^3 \cos^3\delta\, (7 \tan^2\delta - 5) \sin^4 1''$	75+	75+	75+	75+	75+	74.8
$N_6 = d\delta \sin^2 1''$	0.0	0.0	0.0	0.0	0.0	0.0
$-1/2\, da^2 \cos^2\delta \tan\delta \sin^3 1''$	75+	75+	64.4	75+	64.4	27.6
$-3/4\, da^2 \cos^2\delta\, d\delta\, (\tan^2\delta - 1) \sin^4 1''$	75+	75+	75+	75+	75+	75+
$+4/3\, d\delta^3 \sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_7 = 1/8\, d\delta^2\, (3 \tan^4\delta - 2 \tan^2\delta - 1) \sin^4 1''$	75+	75+	75+	75+	74.4	68.8
$+ da^2 \cos^2\delta \sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_8 = 2da \cos\delta\, d\delta \sin^4 1''$	75+	75+	75+	75+	75+	75+
$N_9 = d\delta^2 \sin^4 1''$	75+	75+	75+	75+	75+	75+

We find them to be:

Right ascension, $A, = 3^\circ\ 51'\ 26''.08$,
Declination, $D, = 74^\circ\ 58'\ 2''.52$.

From A, D, a_2, δ_2, we now compute x_2, y_2, also by our former series. These come out:

$$x_2 = +1733''.92, \qquad y_2 = -90''.28.$$

If we now apply equations (1) of the present paper to the data a_1, δ_1, a_2, δ_2, x_1, y_1, we should arrive at the same values of x_2, y_2. Actual calculation of the expressions appended below gives:

	″		″
x_1	$= + 3600.000$	y_1	$= + 3600.000$
M_1	$= - 1984.573$	N_1	$= - 3567.062$
$M_2 x_1$	$= - 1.176$	$N_2 x_1$	$= - 120.818$
$M_3 y_1$	$= + 121.404$	$N_3 y_1$	$= - 0.783$
$M_4 x_1^2$	$= - 0.568$	$N_4 x_1^2$	$= + 0.020$
$M_5 x_1 y_1$	$= - 1.126$	$N_5 x_1 y_1$	$= - 0.531$
$M_6 y_1^2$	$= - 0.037$	$N_6 y_1^2$	$= - 1.107$
$M_7 x_1^3$	$= + 0.003$	$N_7 x_1^2 y_1$	$= + 0.020$
$M_8 x_1^2 y_1$	$= 0.000$	$N_8 x_1 y_1^2$	$= 0.000$
$M_9 x_1 y_1^2$	$= 0.000$	$N_9 y_1^3$	$= 0.000$
x_2	$= + 1733.927$	y_2	$= - 90.261$

These numbers are in satisfactory accord with the values obtained in the previous calculation with the old series.

HAROLD JACOBY
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CURRENT NOTES ON LAND FORMS

UPWARPED MOUNTAINS IN ALASKA

THE descriptions of certain ranges given by A. H. Brooks in 'The Geography and Geology of Alaska' (prof. paper no. 45, U. S. Geol. Survey, 1906) furnish additional examples of upwarped plateaus, carved into mountainous form by normal and glacial erosion, as already indicated in Gilbert's volume on 'Glaciers' in the reports of the Harriman Alaskan Expedition. The coast range, or southeastern part of the Pacific mountain system in Alaska, is said to be an irregular aggregate of mountain masses with little symmetry of arrangement except a rough alignment along a northwest-southeast axis. The whole aspect of the range is rugged and precipitous, from the needle peaks and knife-edge crests down to the sharply incised channels. This young topography appears to have been carved out of an ancient highland, as is indicated by the striking uniformity of the summit altitudes, which range from about 7,500 feet in British Columbia to about 5,000 feet in Alaska. Here and there pyramidal peaks rise above the general sky line, as if representing the fast-disappearing remnants of eminences not reduced to the general regional level in an earlier cycle of erosion. The Endicott range, the most important member of the Rocky mountain province in Alaska, appears to have had a similar history. When viewed from altitudes of 6,000 feet, the summits 'show a remarkably even sky-line and strongly suggest that they have been carved from a former plateau.' Here, while the transverse valleys are sharply cut, the longitudinal valleys are broad with gentle slopes. The Central plateau, between the Endicott and the Pacific ranges, is described as a gently rolling upland in which the rivers have trenched broad valleys; occasional mountains or mountain groups break the continuity of the plateau. All these provinces appear to have been eroded to moderate relief during a lower stand of the land; the contrasts that they now present seem to be due in part to difference in the amount of uplift, and in part to difference in the depth and stage of revived erosion. Although the ranges here considered do not possess even-topped summits, such as occur in certain other mountain ranges lately referred to in these notes, they appear with many others to confirm the law to which Powell gave so much emphasis: that plateaus of uplift are fashioned into mountains by rivers and glaciers.

I. B.

THE WOLDS AND VALES OF BELTED COASTAL PLAINS

THE development of longitudinal belts of higher and lower ground in a coastal plain (or other similar structure) that initially possessed a single continuous transverse slope toward the sea, is a question to which systematic attention has been given but recently in books on physical geography. A terminology appropriate for the description of longitudinal relief of this kind has lately been suggested by A. C. Veatch in connection with the examples that occur in New Jersey and Long Island ('Underground Water Resources of Long Island, N. Y.,' prof. paper 44, U. S. Geol. Survey, 1906, 28–32). He introduces the English terms, *wold* and *vale;* wold, for the upland which is sustained on a belt of more resistant strata; and vale, for the longitudinal depression that is excavated, chiefly by subsequent streams, on a belt of weaker strata. Thus he calls the inner lowland in New Jersey the Hightstown vale; and the enclosing upland the Perrineville wold; the former having its northeastward extension submerged in Long Island Sound; and the latter forming the body of Long Island itself, now ornamented with glacial additions. In this connection, an ingenious explanation is offered for the deflection of the Delaware, Susquehanna and Potomac rivers for short distances southwestward along the vale, before they transect the wold: Direct consequent courses are assumed to have prevailed in the first cycle of coastal-plain erosion (in which the small relief of old age was presumably attained); then during a time of depression, the transverse passages through the wold were obstructed by Lafayette deposits; and on reelevation—probably with a slight tilt to the southwest—the three rivers deserted their former transverse notches and sought new ones. The problem is necessarily an obscure one, because of the large amount of erosion since the deflected courses were taken. Darton (and later, Newsom) had previously explained these cases of river deflection as caused by coastal sand reefs during a time of submergence; but Veatch points out that this would not account for the occurrence of deflection only in those rivers where a vale had been eroded on weak Cretaceous beds.

In my own practise, the forms here designated by wold and vale have been called cuesta and (inner) lowland. Objection has been frequently urged against the Spanish word, cuesta, because it does not mean only a lop-sided ridge, but a hill or slope of any kind. To this my answer has been that, as soon as any other fitting term comes to be generally adopted, the Spanish name may be given up; but that in the meantime cuesta is much better than no name at all. It will now be interesting to note what acceptance is gained, especially in England, by wold and vale in the restricted sense proposed by Veatch; and to know how many American physiographers will say 'Chunnenugga wold' and 'Winnebago vale' for the cuesta of southern Alabama and the inner lowland of eastern Wisconsin. 'Wold,' like 'forest,' originally meaning wild-land, but not necessarily wood-land, is taken from eastern England, where it names the lop-sided ridge of chalk which ends at Flamboro head; but the chalk cuesta elsewhere in England has other names, such as Chiltern 'Hills,' near Oxford, and the North and South 'downs,' on either side of the eroded lowland of the Weald (another form of wold): and 'vale' is used in England not only for lowlands of the kind here considered, but also for the Vale of Eden, eroded on a faulted mass next west of the Pennine escarpment; and for the Vale of Pewsey, an anticlinal valley. It is perhaps as doubtful whether English physiographers will be content to use these semi-poetic terms in the limited systematic sense proposed by Veatch, as whether Spanish physiographers (if such there be) will be satisfied with the foreign use of cuesta for a low lop-sided ridge.

W. M. D.

SOUTHERN ARKANSAS AND NORTHERN LOUISIANA

THE inner part of the coastal plain in the Gulf States is well known to be a hilly district, but it is not often that one meets specific and systematic accounts of its topographic features. A few pages of welcome information on this matter are found in a recent report by A. C. Veatch ('Geology and Underground Water Resources of Northern Louisiana and Southern Arkansas,' prof. paper 46, U. S. Geol. Survey, 1906, 14–16), where the hill lands between the broad flood plains of the Mississippi, Ouachita, Red and Sabine rivers are described as traversed (ENE–WSW) by several 'ranges of hills, which are more or less persistent for many miles and which follow the general strike of the formations producing them.' The ranges and the strike valleys between them are called wolds and vales, according to the terminology referred to in the preceding note. The Kisatchie wold, formed on the Catahoula (Grand Gulf) formation in northwestern Louisiana, is perhaps the most important. According to McGee it is continued southeastward through the state of Mississippi as the 'Grand Gulf hill land'; and it is due to this wold-making formation that the Mississippi flood plain is narrowed and its enclosing bluffs are increased in height near Natchez ('Lafayette Formation,' 12th Ann. Rep. U. S. Geol. Survey, 1891, 366–370). Sulphur wold, formed on the sandy beds of the lower Eocene, extends from southwestern Arkansas into Texas: between its inner face and the next (Saratoga) wold, farther inland, is a vale along which a main railway line runs from Little Rock. Sulphur wold would appear to be the trans-Mississippi representative of what is known in Alabama as the Chunnenugga ridge (best described by E. A. Smith, Geol. Surv., Alabama, Rep. for 1881–2, p. 273), and in Mississippi as the 'Lignitic hill lands' (see McGee, as above). The wolds, cuestas or hill lands of Arkansas and Louisiana are not continuous upland belts, but are maturely dissected into gentle hills and open valleys by consequent and insequent streams (subsequent streams are poorly developed), with a relief of 100 or 200 feet.

The peculiar shallow lakes which once occupied the valleys lateral to that of the Red river of Louisiana are described with care (pp. 59–64). The explanation which attributes them to obstruction by normal though rapid aggradation of the main-river flood-plain is shown to be in error. They are due to the obstruction of the main river by its 'raft,' or jam of fallen trees. The raft grew by gradual addition to its upper end, while its downstream end slowly decayed and drifted away. As one tributary stream after another was thus obstructed, a shallow lake rose in the lateral valleys. The raft has been artificially removed (1873) by cutting away the tree trunks; and since then the river has lowered its bed and the lakes have shrunk or disappeared. They are given too great number and size on most maps. The deflection of the Red river by the raft to the northeast side of its flood plain, and the resulting development of rapids, not yet graded, where it turns by a new course into the Mississippi flood-plain are items worth mention.

The lakes on the tributaries of the Danube near its mouth have, like those lateral to the Red river of Louisiana, been explained as due to aggradation of the main-river flood-plain. In view of the above restatement of the problem of the Red river lakes, that of the Danube lakes also may require a new interpretation.

W. M. D.

THE BICENTENARY OF LINNÆUS

THE trustees of the British Museum have deputed one of their officers, Dr. F. A. Bather, assistant keeper of the geological department, to represent the museum at the celebrations in Sweden of the bicentenary of the birth of Linnæus. Dr. Bather has been instructed to present two addresses to the University of Upsala and the Swedish Academy of Sciences, the former of which reads as follows:

The British Museum (Natural History), London.
To the Royal University of Upsala.

It is with feelings of peculiar indebtedness that the Board of Trustees of the British Museum desires on this occasion to greet and congratulate the University of Linnæus.

In January, 1758, was published the tenth edition of the 'Systema Naturæ,' the edition from which the zoologists of the world now date the technical nomenclature of animals. In January, 1759, the British Museum was first opened to the public, and its Natural History Departments began the systematic study of the living and extinct animals and plants, taking for their guidance the works of Linnæus, and for their teacher his favorite pupil, Daniel Charles Solander.

By the acquisition of the Banksian Herbarium and Library, already brought to such perfection of arrangement by Solander and Jonas Dryander, the British Museum became the repository of many plants described by Linnæus, notably the originals of the celebrated 'Hortus Cliffortianus,' as well as of valuable manuscripts and books connected with the great Swede.

Desiring, therefore, to share in your celebration of one to whom the British Museum owes so much, the Trustees beg to join with this letter 'A Catalogue of the Works of Linnæus Preserved in the Libraries of the British Museum,' which they have had specially printed in honor of this occasion, and they have appointed as their delegate to present the same one of their officers, Dr. Francis Arthur Bather, M.A.Oxon, Assistant Keeper of the Geological Department.

May the world-wide fame of Linnæus and the fortune of the ancient University of Upsala ever endure and increase to the advancement of learning and the benefit of mankind!

EDWIN RAY LANKESTER,
Director

BRITISH MUSEUM (NATURAL HISTORY),
May 11

SCIENTIFIC NOTES AND NEWS

M. DE LAPPARENT, professor of mineralogy and geology at Paris, has been elected permanent secretary of the Paris Academy of Sciences in succession to the late M. Berthelot.

THE senate of the University of Toronto has conferred the degree of LL.D., on Dr. S. Weir Mitchell, of Philadelphia.

AT the recent commencement of the Jefferson Medical College, of Philadelphia, the honorary degree of doctor of laws was conferred upon George Sumner Huntington, M.D., ScD., professor of anatomy, College of Physicians and Surgeons, Columbia University. Professor Huntington delivered an address on 'Modern advances in the teaching of anatomy and other medical sciences.'

PROFESSOR ROLLA C. CARPENTER, who holds the chair of experimental engineering at Cornell University, has been given the degree of doctor of laws by the University of Michigan.

NEW YORK UNIVERSITY has conferred the doctorate of laws on Dr. Joseph D. Bryant, of New York City, retiring president of the American Medical Association; on Charles W. Hunt, New York City, secretary of the American Society of Civil Engineers, and on Professor George F. Swain, professor of civil engineering of the Massachusetts Institute of Technology.

SIR WILLIAM RAMSAY, K.C.B., has received the Order of Commendatore della Corona d'Italia from the King of Italy.

The friends of Dr. K. Mitsukuri, the distinguished professor of zoology and dean of the School of Science of the Imperial University in Tokyo, have been pained to learn that he was attacked on April 17 with apoplexy. After lying unconscious for a week or more he was on May 13 slightly better, but still seriously ill.

Dr. Thomas S. Fiske, professor of mathematics at Columbia University, has been given sabbatical leave of absence for next year.

Mr. Andrew Watt has been elected meteorological secretary of the Scottish Meteorological Society in succession to the late Dr. Buchan.

Professor Maurice Henriot has been appointed director of the Experimental Laboratory of the Paris Mint.

The Transvaal government has appointed a commission, consisting of Dr. Kynaston (Geological Survey Department), Mr. T. N. Leslie (Vereeniging), Mr. J. P. Johnson (Johannesburg), and Professor R. B. Young (Transvaal University College), 'to report to the government on the Bushmen paintings and stone etchings existing in the Transvaal, and as to what steps should be taken to preserve them from decay and mutilation.'

A grant of £150 has been made from the Balfour fund, Cambridge University to W. E. Agar, of King's College, in furtherance of his proposed expedition to the Paraguayan Chaco.

Dr. Bergen Davis, instructor in physics at Columbia University, has been appointed to the Ernest Kempton Adams fellowship for next year.

The fellowship sustained by the alumnæ association of the Woman's College of Baltimore, to be awarded annually to a member of the association, has this year been awarded to Miss Mary J. Hogue, of the class of 1905. Miss Hogue has already held the foundation scholarship in biology at Bryn Mawr College and the graduate scholarship in the same institution. She will pursue the study of biology in German universities. The Woman's College of Baltimore maintains three tables at the United States Marine Biological Laboratory at Woods Holl, Massachusetts. The following students have been awarded scholarships entitling them to the use of tables during the coming session: Anita Shemwell Dowell, Grace Imogene Guy, M. Louise Frazee.

The Phi Beta Kappa address at Vassar was given on June 10 by Dr. Elmer Ellsworth Brown, U. S. Commissioner of Education.

Dr. W. W. Folwell, professor of political economy at the University of Minnesota and formerly president of the institution, will make the address at Hobart College, on June 18, on the occasion of the laying of the cornerstone of the William Smith Hall of Science.

The two-hundredth anniversary of the birth of Linnæus was celebrated on May 23 at the Western College for Women, Oxford, Ohio. An address was made by Dr. John M. Coulter, head professor of botany in the University of Chicago.

We learn from English journals that among those upon whom an honorary degree was conferred at the recent celebrations at the University of Upsala in commemoration of the bicentenary of Linnæus were Mr. William Carruthers, F.R.S., former president of the Linnean Society of London, and Mr. Francis Darwin, F.R.S., who represented Cambridge University.

Speaking at the forty-seventh annual dinner of King's College, London, on May 27, Dr. Headlam, the principal, referred to the loss which the college has sustained by the death of Dr. MacFadyean, and suggested that there should be a public recognition of one who died as a martyr in the cause of science, and for the sake of amelioration of disease and the benefit of the human race.

Sir Dietrich Brandis, F.R.S., the son of Dr. C. H. Brandis, professor of philosophy at Bonn University, and for many years inspector general of the forests of India, died on May 29, at the age of eighty-three years.

There will be on June 19 a civil service examination for the position of scientific assistant with a salary of $750 in the Bureau of Fisheries.

THE eastern branch of the American Society of Zoologists will hold its next annual meeting during convocation week at the Sheffield Scientific School, New Haven.

FOREIGN journals report that the movement for the institution of an Italian Association for the Advancement of Science, proposed at Milan last year, has now taken form and development under capable organizers, including Professor Romiti, of Pisa. The first meeting will be held at Parma in September next, when it is hoped that the sister associations of Europe and America will send delegates. Italy has many associations for the advancement of special sciences, but, as Professor Romiti has put it, she has yet to form an association which shall 'represent the synthesis' of them all. Attempts were made in 1839 and 1875 to start such an association on the British and German models, but they have had no successor. It is hoped and believed that the attempt which has now been renewed will result in the establishment of a permanent institution.

DR. LAWRENCE F. FLICK, director of the Phipps Institute, Philadelphia, and chairman of the committee on the International Congress of Tuberculosis, which is to be held in Washington in the fall of 1908, announces that he has received $35,000 in subscriptions to a fund of $100,000 which he is raising to meet the necessary expenses. The subscribers include Messrs. Martin Maloney and William H. Henscri, of Philadelphia; Messrs. Henry Phipps, George Blumenthal and Henry C. Frick, of New York, and Mr. Henry L. Higginson, of Boston.

MR. WILLIAM URBAN, of Brooklyn, N. Y., has recently presented his collection of minerals to Colgate University as a memorial to his friend, the late Rev. Edward Lathrop, D.D., who had been for some years before his decease president of the corporation of the university. The value of the collection is about $2,500, and it will be installed in the new Science Hall, which bears the name of Dr. Lathrop.

THE library of Mr. Stuart M. Samuel, M.P., which Messrs. Sotheby will sell on July 1, contains the author's autograph manuscript of Gilbert White's 'Natural History and Antiquities of Selborne,' in the form of letters to Thomas Pennant and Daines Barrington, and arranged in a folio volume. The MS. remained in the possession of the author's descendants until 1895, when it was sold at Sotheby's and acquired by the present owner.

THE Norddeutscher Lloyd and the Hamburg-Amerika Lines have agreed to allow to members proceeding to the Fourteenth International Congress of Hygiene and Demography, to be held at Berlin, September 23 to 29, a reduction on the price of tickets.

AN exceptional opportunity for the study of evaporation in relation to climate is afforded by the Salton Sea. As our readers are aware, the sea was created by the accidental turning of the Colorado River into the Salton Basin, a dry valley in southeastern California lying below the level of the ocean; and now that the river has been restored to its original channel the sea has begun to dry away. The time required for its complete dissipation is estimated at from ten to fifteen years, and during that period the accession of water from all sources will be nominal. It will thus constitute an evaporation pan on a grand scale, and the measurement of its progressive lowering will give valuable information to engineers charged with the planning of reservoirs. The matter is also of importance to meteorologists, and arrangements have been made for a joint investigation by the U. S. Weather Bureau, the U. S. Reclamation Service and the U. S. Geological Survey. To determine the relation of the evaporation to temperature, atmospheric humidity and wind, a group of meteorological stations are to be maintained in the Salton Basin; and the endeavor will be made to develop a general formula for the estimation of evaporation in any locality where the ordinary climatic factors are known. A reconnaissance of the region has just been made by a board composed of F. H. Bigelow, C. E. Grunsky and G. K. Gilbert, representing severally the bureaus mentioned above.

Beginning on June 2, the Boston Society of Natural History proposes to open its museum free to the public on Sunday afternoons from 1 to 5, during June, July, August and September of this year. The exhibition rooms are regularly open free on Wednesday and Saturday from 10 till 5, but it is believed that the Sunday afternoon opening will accommodate many who are unable to visit the museum on week days. For the last five years, under the direction of its curator, Mr. Charles W. Johnson, the society has expended its efforts in endeavoring to make the New England collections the chief display of the museum so that at the present time a very good representation of the New England fauna is on exhibition. It is the society's intention to build up the New England collection of animals and plants so that it shall be as nearly complete as possible. In addition to the specimens on exhibition, there is also a rapidly growing study collection of birds, shells, insects and plants which may be consulted upon application to the curator on week days. Since this is the only natural history museum in the city and the only one whose particular scope is limited to the preservation and study of the New England fauna and flora, it is hoped that this action on the part of the society in opening its museum to visitors on Sundays will arouse additional interest in the study of New England natural history and that the museum may be a center to which all who are interested in this study shall feel themselves welcome.

The New York Medical Record states that the Carnegie Institution of Washington, which has been bearing most of the cost of publication of the present series of the *Index Medicus,* announces that as the journal has not met with the support from the profession that was hoped for, unless it appears that the *Index Medicus* is of greater service to the medical profession and can help to support itself to a greater extent than in the past, it may become advisable to discontinue its publication. The *Index Medicus* was established in 1879, under the editorship of Dr. John S. Billings and Dr. Robert Fletcher, and was discontinued in 1899. The present series began with the number for January, 1903.

UNIVERSITY AND EDUCATIONAL NEWS

Announcement is made that the sum of $430,000 has been contributed to Columbia University towards erecting Kent Hall, a building for the school of law and the faculty of political science.

The Jefferson Medical College, Philadelphia, graduated, at its eighty-second annual commencement held on June 3, 126 students, of which 105 had received hospital appointments in the recent competitive examinations.

The University of Giessen will celebrate its three-hundredth anniversary from July 31 to August 4.

Professor A. J. Hopkins, A.B., Amherst, '85, Ph.D., Hopkins, '93, associate professor of chemistry at Amherst College, has been appointed head of the department in view of the retirement of Professor Elijah P. Harris.

At Dartmouth College promotions from instructor to assistant professor have been made as follows: Charles A. Proctor, mathematics; Julius A. Brown, physics; Dr. Charles E. Bolser, chemistry.

At Clark College, Dr. J. B. Porter has been promoted to an assistant professorship in psychology, and Dr. F. B. Williams, of Union College, has been called to an assistant professorship of mathematics.

Mr. H. F. Rolker, of the Johns Hopkins University, has been appointed private research assistant to Dr. J. Bishop Tingle, recently appointed to the chair of chemistry at the McMaster University, Toronto.

Dr. Alexander Hill has announced his intention of resigning from the mastership of Downing College, Cambridge.

Mr. H. Bateman, fellow of Trinity College, Cambridge, has been elected to the readership in mathematical physics, endowed by Professor Arthur Schuster to encourage research in mathematical physics.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

LINNÆUS AS A ZOOLOGIST[1]

CAROLUS LINNÆUS, later known as Carl von Linné, was born at Roeshult, in the province of Smaland, Sweden, May 13, old style, 1707, and died at Hammerby, near Upsala, on January 10, 1778. His grandfather was a farmer; his father, a clergyman. Young Linnæus, the future naturalist, was intended by his parents for the ministry, and his early education was conducted with this end in view. At the age of ten, he was sent to the Latin school at Vexio, but after seven years at this school he was found to be so deficient in his scholastic studies that his parents thought of apprenticing him to a shoemaker.

While at Vexio, much of his time was devoted to the study of plants and insects, an inclination apparently favored by his master, who was himself greatly interested in botany. Fortunately young Linnæus was rescued from his threatened degradation by Dr. John Rothman, a physician of Vexio, who recognized his superior abilities and appreciated his interest in natural history. He took him into his own home, where for a year Linnæus continued his botanical studies, aided by the advice and library of his patron. At the age of twenty he entered the University of Lund, where he soon found himself without means of support, through the death of his patron and friend, the kind-hearted physician of Vexio. Fortunately he soon won the friendship of Dr. Kilian Stobæus, the pro-

[1] Read at the exercises of the New York Academy of Sciences in commemoration of the two hundredth anniversary of the birth of Linnæus.

fessor of botany and medicine, who made him a member of his family. Here he had access to books and to a small museum of natural history, and found much leisure for exploring the neighboring country and for collecting objects of natural history. At the end of a year he went to Upsala, where, under Rudbeck and Roberg, he advanced rapidly in medicine and botany. Here he won the friendship of the renowned Olaf Celsius, whom he later characterized as the best botanist in Sweden, and of Artedi, a fellow student, who afterwards became the founder of ichthyology. During his whole course at Upsala, it is said that he did not hear a single public lecture on either anatomy, botany or chemistry, but he and Artedi, in good-tempered rivalry, were devoting their energies to natural history, Linnæus to plants, birds and insects, and Artedi to amphibia and fishes. Linnæus here also began the preparation of his epoch-making works on botany and of the first edition of his 'Systema Naturæ,' published a few years later in Holland.

In 1732, at the age of twenty-five years, he was commissioned by the Upsala Academy of Sciences to make a tour of exploration in Lapland in the interest of natural history. He left Upsala on the twelfth of May, and after an absence of five months returned to Upsala on the tenth of October. This remarkable journey of 4,600 miles was made partly on horseback, partly by boat and partly on foot; it extended northwestward across the Norwegian Alps to the coast of Norway beyond the Arctic Circle; the return journey was made by way of eastern Finland. It was an undertaking of great hardship and much danger, being performed alone, aided only by local guides employed to conduct the way from one point to another. On his return a report of his journey was presented to the academy, but it remained in manuscript until translated and published in English by Dr. James Edward Smith, the first president of the Linnæan Society of London, in 1811.[2] The botanical results, however, were published separately by Linnæus himself, in 1737.

The following year was spent at Upsala, where he attempted to eke out his scanty means of support by giving lectures on botany, mineralogy and chemistry. This proved contrary to one of the statutes of the university, to the effect that no one should give public lectures who had not obtained his doctor's degree, which statute was invoked against him by Rosen, the successor to Rudbeck in the professorship of medicine and anatomy, who was jealous of Linnæus's abilities and attainments. Deprived of this financial resource, he took some of his pupils on excursions into the neighboring mountains, where he met the governor of the province of Dalecarlia, who sent him to explore and report on certain copper mines in which he was interested. While on this journey he gave lectures at Fahlun on mineralogy and assaying. Here he made the acquaintance of Dr. Moræus, a learned and wealthy physician of the district, and his two daughters, to one of whom he became betrothed; the father, however, insisted on deferring the marriage till Linnæus had completed his professional studies and obtained his medical degree. For this purpose, in the spring of 1735, he jour-

[2] The herbaria, library (about 2,500 volumes), manuscripts and correspondence of Linnæus were offered by his widow and daughters, 'by the advice of friends,' to Sir Joseph Banks, 'for the sum of a thousand guineas'; Sir Joseph, not feeling inclined to the purchase, recommended it to the consideration of his friend Doctor (later Sir) J. E. Smith, by whom these treasures were secured and transferred to England (Turton, 'Life and Writings of Linnæus,' 1806, p. [39]), and later passed into the possession of the Linnæan Society of London, founded in 1788 through the efforts of Dr. Smith, who was its first president. (See Jardine's 'Naturalist's Library,' Vol. I., 1833, p. 58.)

neyed to Lubek and Hamburg, and later to Holland, where, in June, he received from the University of Harderwyk the degree of doctor of medicine. At Leyden he became acquainted with the leading men of science of that city, which soon led to his engagement by Dr. George Cliffort, a wealthy burgomaster of Amsterdam, to take charge, at a liberal salary, of his extensive museum and botanic garden. Later he was sent by him to England to secure rare plants for his garden, with a letter of introduction from the great Boerhave to Sir Hans Sloane. He thus came in contact with the botanists of London, where, however, his reception was not always cordial.

On his return to Holland he was offered the position of government physician to the Dutch colony in Surinam, which he prudently declined, and became an assistant to his friend Van Royen at the botanic garden in Leyden. After a brief visit to Paris, he returned to Stockholm in September, 1738, where he determined to settle as a physician. Notwithstanding his fame abroad and his skill as a botanist, the pecuniary returns from his profession were at first small, but they gradually increased, and, obtaining some government patronage, his marriage to Miss Moræus was celebrated on June 26, 1739.

He remained only three years in Stockholm, during which period he helped to found the Royal Academy of Sciences of that city and served as its first president. In 1741, under an order from the government, he made a journey through Öland and Gothland. In the same year, he was called to the chair of botany at the University of Upsala, a position to which he had long aspired and which he filled for thirty years, when impaired health compelled him to resign his official duties and to discontinue his literary labors. The University of Upsala, through the fame of Linnæus, became widely renowned as a seat of learning, and attracted students from various parts of Europe. During these years of almost uninterrupted activity, most of Linnæus's numerous botanical and other works were published, the material for which reached him in ever-increasing abundance, not only from distant parts of Europe, but from Siberia, China, India, Egypt, South Africa and North and South America.

Academic honors were showered upon him by all the learned societies of Europe; a gold medal was struck in his honor by the nobles of Sweden, and in 1757 he was created by King Frederic a Knight of the Polar Star and admitted to hereditary nobility. Foreign courts made overtures for his presence, and his own country neglected no opportunity to do him honor. His death in 1778, after six years of invalidism resulting from an attack of apoplexy, was recognized as a national calamity; the University of Upsala went into mourning, and the king ordered a medal to be struck in his memory.

Although cramped by poverty during the earlier part of his career, prosperity did not long withhold her smile; not only were the nobles of his country his patrons, but he was an especial favorite of both King Frederic and his queen. Through various emoluments showered upon him he was able, later in life, to purchase a large estate and to construct for himself a museum, wherein he gathered the largest collection of botanical treasures that at that time had anywhere been brought together. He was happy in his domestic relations, and lived to see his son succeed to his chair at the University of Upsala.

Although Linnæus's publications relate mainly to botany and medicine, they cover the whole realm of natural history. His earliest contribution to science is generally considered to be his 'Florula Lapponica,' the first part of which appeared in the

Transactions of the Swedish Academy in 1732.[3] This was followed by the first edition of his 'Systema Naturæ,' published in Leyden in 1735. The 'Fundamenta Botanica' followed in 1736, and was later enlarged and republished as the 'Philosophia Botanica,' in 1751. During the next ten years various other botanical publications appeared in rapid succession. His 'Fauna Suecica,' published in 1746, was his first special work relating to zoology. It is also notable as being the first work especially devoted to the entire fauna of any country. It was republished, with many additions, in 1761. Other botanical and several medical works followed during the next seven years, including his monumental 'Species Plantarum,' published in 1753. In the same year also appeared the 'Museum Tessianum,' consisting chiefly of descriptions of minerals and fossils, the latter mainly shells and corals, and in 1754 the 'Museum Adolphi Friderici,' relating exclusively to exotic animals. This was a folio with 33 plates, the most extensive and most elaborately illustrated of all of Linnæus's works. Two important medical works appeared in 1760, and his third zoological work, the 'Museum Ludoviciæ Ulricæ,' in 1764, a thick octavo, to which was annexed the second part of the 'Museum Adolphi Friderici.'

During these thirty years of marvelous scientific activity, Linnæus also contributed many papers to the transactions of the Upsala and Stockholm Academies and to the 'Amœnitates Academici.' The latter, in ten octavo volumes, consists of dissertations or academical theses, mostly by his students, selected, edited and published by him, and thus may be regarded as of equal authority with his own writings. Seven of these volumes were published during his lifetime and contain a number of his own minor papers.

[3] His 'Hortus Uplandicus' is said to have appeared one year earlier. See 'List of the Works of Linnæus,' in Jardine's Naturalist's Library, Vol. I., 1833, p. xvii, foot-note.

This brief outline of Linnæus's life, his opportunities, and the published results of his scientific labors, affords the basis for the consideration of Linnæus as a zoologist. As has been shown, he was primarily a botanist; he was also a mineralogist, an entomologist and a conchologist, but only incidentally a vertebrate zoologist. In this field his interest was less strong, his opportunities for research the most restricted. His zoological writings, exclusive of a few minor papers, are comprised in the 'Fauna Suecica,' the 'Museum Adolphi Friderici,' the 'Museum Ludoviciæ Ulricæ' and the several editions of his 'Systema Naturæ.' The first edition, appearing in 1735, was a folio of only 12 pages, consisting merely of a conspectus of his 'Systema,' in tabular form. The second edition, published in 1740, was an octavo of 40 pages, in which were added, for the animal kingdom, the characters of the groups. The sixth, published in 1748, was greatly enlarged, the zoological part alone consisting of 76 pages, illustrated with six plates, or one for each of his six classes of animals. The tenth, published in 1758, was in two octavo volumes, of which the zoology formed the first volume, consisting of 824 pages. The twelfth, and the last edition revised by the author, was issued in three volumes, the first of which, containing the zoology, and comprising 1,427 pages, appeared in 1766. Thus in thirty-three years this work grew from a brochure of 12 pages to a work of 2,400 pages.

The first edition of the 'Systema' was published when the author was only twenty-eight years old, during his sojourn in Holland; he had never previously been beyond the confines of southern Sweden, except on his journey to Lapland and Finland in 1732, and he had had access to no large collection of animals. Thus his resources for such an important undertaking were extremely limited, being restricted to his own considerable first-hand knowledge of the fauna of Sweden, to the few specimens of exotic animals he had been able to see in Lund, Upsala and Stockholm, and to the scanty literature of the subject there available. When the second edition appeared, in 1740, he had spent less than three years and a half in foreign countries, mainly in Holland with single brief visits to London and Paris, but his interests on these occasions were botanical and not zoological.

The sixth edition (the third revised by the author), published in 1748, was in effect a synopsis of the fauna of Sweden, filled in, as regards the fauna of the rest of the world, by compilations from his predecessors. Strange as it may seem, outside of the tropical genera *Simia, Bradypus, Dasypus, Myrmecophaga* and *Manis,* this edition enumerates only thirteen species of mammals not found in Sweden. Only 140 are recorded for his whole class 'Animalium quadrupedium,' one third of which are Scandinavian.

This analysis could be extended to other classes with practically similar results. The class Insecta, for example, includes only 13 species that are not also recorded in the 'Fauna Suecica,' showing how limited was his knowledge of the world's fauna at 1748.

The tenth edition (the fourth revised by the author), published in 1758, is *the* epoch-making work in the history of zoology, as in this the binomial system of nomenclature for the whole animal kingdom is introduced for the first time. The work is also greatly enlarged, and the classification greatly improved, especially that of the mammals, which class is now for the first time aptly designated Mammalia. The ordinal term Primates is substituted for Anthropomorpha of the sixth and previous editions, the sloths (genus *Bradypus*) are removed from it, the genus *Lemur* is added, as a new genus, and the bats are transferred to it from the Feræ. A new order, Bruta, is made up of his former third order Agriæ, now suppressed, and of such other extremely heterogeneous elements as the elephant, the manatee, sloths, anteaters and the scaly anteaters. The order Feræ consists of six properly associated genera; the armadillos, insectivores and bats, formerly included in it, being removed elsewhere. His fourth order, Bestiæ, is a new group, composed of the pigs, armadillos, opossums and insectivores. The fifth order, Glires, is a natural group, except for the inclusion of the genus *Rhinoceros,* now most strangely placed with the squirrels and mice. His sixth order, Pecora, is retained as in the previous editions, and is also a natural group. The seventh, Belluæ, is a new ordinal group, consisting of the genera *Equus* and *Hippopotamus,* transferred from the here disrupted order Jumenta of previous editions. The Cete, now removed by him from the fishes, form his eighth and last order. This reconstruction of the ordinal groups is a great improvement; five new genera are added, two old ones eliminated, and the number of species is increased from 140 to 185. In some of the other classes there are similar radical changes, but there is not time to refer to them.

The twelfth, and the last edition revised by the author, published in 1766, shows many improvements over the tenth. It is greatly increased in bulk through the addition of many new genera and a large number of new species. The classification is also judiciously modified at many points. Taking again the class of mammals for illustration, the number of orders is reduced from eight to seven, through the suppression of the grossly unnatural order Bestiæ and the transference of its genera to other associations, with, however, the retrograde change of placing the insectivores and the genus *Didelphis* among the Feræ. The Glires are modified by the removal of the genus *Rhinoceros* to the order Belluæ, and the addition to it of *Noctilio,* a genus of bats. The order Bruta is the same incongruous association of elephants, manatees, sloths and anteaters as in the tenth edition.

The orders of mammals, as now left, correspond in several instances very nearly with those of our modern systems, notably the Primates, Glires, Pecora and Cete. The Feræ of the tenth edition corresponds to the modern Carnivora, but in the twelfth he made the mistake of putting back into it the marsupials and the insectivores. His order Belluæ being essentially the modern suborder Perissodactyla, his order Bruta is the only grossly incongruous association of types.

The only previous classification of mammals with which Linnæus's need to be compared is Ray's, published in 1693, whose system, taken as a whole, is far more artificial than Linnæus's. Naturally there are some striking coincidences of grouping, and in the characters employed by the two authors. As to the latter, Ray so well covered the field that there was little left for Linnæus to add, since during the interval between Ray and Linnæus not much was learned about the anatomy and relations of the ordinal groups of mammals. Doubtless Linnæus was influenced, in his removal of the cetaceans from the fish to the mammal class, by the systems of his contemporaries, Klein (1751) and Brisson (1756), in which respect only are their systems better or less artificial than his. Inasmuch, however, as Brisson divided mammals into eighteen orders instead of seven, he escaped some of the grotesque combinations made by Linnæus; on the other hand, he gave undue emphasis to relatively unimportant differences.

Linnæus's classification of birds is closely modelled upon that of Ray, and his departures from it are seldom improvements. His lack of knowledge of ornithology is strikingly apparent through his repeated association of very unlike species in the same genus, as where a penguin is combined with a tropic-bird to form his genus *Phaëthon,* and another species of penguin with an albatross to form his genus *Diomedea.* In the tenth edition, he recorded only about 550 species of birds; in the twelfth, this number was raised to nearly a thousand, mainly on the basis of Brisson's great work, which appeared in 1760. The greater part were based on the writings of previous authors; probably less than one fourth of them being known to him from specimens.

His class Amphibia contained four orders, of which the fourth consisted of cartilaginous and other wholly unrelated fishes, and shows how slight was his acquaintance with the lower classes of vertebrates. His first order, Reptilia, includes such diverse animals as turtles, lizards, salamanders, frogs and toads. The snakes formed his second order Serpentes.

His arrangement of the fishes was originally based on that of Artedi, whose 'Ichthyologia' Linnæus published while sojourning in Holland in 1738, after Artedi's untimely death by accidental drowning.

His class Insecta is nearly equivalent to the modern subphylum Arthropoda, as it includes the Arachnida and the Crustacea.

His class Vermes was the waste-basket of his system, including all the forms of animal life that were neither vertebrates nor insects, which he distributed into five orders, some of them as heterogeneous in character as the class itself. The second order, Mollusca, comprised all sorts of soft-bodied animals, mostly marine, as slugs, sea-anemones, ascidians, holothurians, cuttlefishes, starfishes, sea-urchins and jelly-fishes. The animals now commonly known as Mollusca formed his third order Testacea.

It is not, however, just to judge Linnæus's work by the standards of to-day. The above comparison of the zoological part of the 'Systema Naturæ' with our present knowledge of animals is not to be taken as a disparagement; we merely note the progress of zoology during the last century and a half of the world's history. Linnæus was a born systematist; his energy and industry were enormous; his isolation promoted independence and originality. He devised new classifications, and thoroughly systematized not only the knowledge of his predecessors, but the vast increment he himself added. He inspired his students with his own enthusiasm, taught them his own advanced methods, and influenced a goodly number of them to undertake natural history explorations in distant and zoologically unknown parts of the world.

In special lines of research he was far behind several of his contemporaries, notably Brisson in respect to both mammals and birds. But he nearly doubled the number of known forms of reptiles, amphibians and fishes, and increased many fold the number of species of cœlenterates, on the basis of wholly new material gathered through his own efforts.

Disgusted with the needlessly detailed accounts and repetitions that characterized the writings of most of his predecessors, he unfortunately adopted the extreme of condensation, thereby adding greatly to the difficulties of his successors in determining to just what forms the thousands of new names he introduced really belonged. Many of his species, based on the accounts given by previous authors, were also composite, often containing very diverse elements. But this detracts little from his credit. As one of his appreciative biographers has tersely put it: "He found biology a chaos; he left it a cosmos."

Linnæus's beneficent influence upon biology was hardly less as a nomenclator than as a taxonomist. He not only invented a descriptive terminology for animals and plants, but devised a system of nomenclature at once simple and efficient, and which for a hundred and fifty years has been accepted without essential modification.

Linnæus divided the three kingdoms of nature into classes, the classes into orders, the orders into genera, the genera into species, under which latter he sometimes recognized varieties. Of these groups, as he understood them, he gave clear definitions, but they were in most cases much more comprehensive than the limits now assigned to groups of corresponding rank. His genera correspond in some cases to groups now termed orders, and frequently to the modern idea of family; in some cases they contained species now placed in separate orders. Prior to Linnæus, these groups had less definite significance, and were often designated by a phrase instead of a single word. Species were indicated only by a cumbersome diagnosis, intended to express their chief distinctive characters. For this Linnæus substituted a single word, an innovation the merits of which were at once almost universally recognized. But Linnæus reached this solution of a grave inconvenience somewhat slowly, and not till 1753 did he fully adopt the *nomen triviale,* when he introduced it into botany in his 'Species Plantarum,' which is taken by botanists as the point of departure for the binomial system. In the following year, 1754, he introduced it into zoology, using it throughout his 'Museum Adolphi Friderici' for all the animals catalogued or described in this superb work, namely, 39 species of mammals, 23 of birds, 90 of reptiles and amphibians, 91 of fishes and 64 of invertebrates, or for an aggregate of 307 species of animals. Four years later, in the tenth or 1758 edition of his 'Systema Naturæ,' he adopted it for the whole animal kingdom, which date is now generally taken as the beginning of the binomial system for zoology. The importance and utility of this simple innovation in a matter of nomenclature are beyond estimate, and if Linnæus had done nothing else for the advancement of biology he would be entitled to a conspicuous niche in the temple of fame and to the gratitude of all subsequent workers in this field. He for the first time gave technical standing to the systematic names, both generic and specific, of all the plants and animals known at the dates when he introduced the *nomen triviale* into the nomenclature of botany and zoology.

It is of interest in this connection to note the number of species of animals known to Linnæus at the date of publication of the last edition of the 'Systema Naturæ,' the number known to him personally, and the number recorded respectively from North America and from South America.

Of mammals, the whole number of species recorded is 190, of which three fourths are based on the descriptions of previous authors. Only 48 were American—12 from North America and 36 from South America. The five North American mammals known to Linnæus from specimens were the raccoon, star-nosed mole, common mole, flying squirrel and chipmunk. The number of species at present known from North America is 600, excluding subspecies. The number for the world, including the extinct as well as the living, is about 10,000, as against less than 200 recorded by Linnæus.

Of birds about 925 are recorded of the 15,000 known to-day. The 200 known from America are divided about equally between North America and South America, only 50 of which were described from specimens.

The amphibia and reptiles number collectively about 250, of which about one third are American, 40 per cent. of the latter being North American and 60 per cent. South American. The North American include three salamanders, the box-turtle, the six-lined lizard, the blue-tailed lizard and 14 snakes. The greater part of the 20 North American species of reptiles and amphibians known to him personally were based on specimens transmitted by his former student, Dr. C. D. Garden, from the Carolinas, and on a few sent from Pennsylvania by Peter Kalm, also one of his students. Thus the greater part of the snakes of the eastern United States became known to Linnæus prior to 1766.

About 500 species of fishes are recorded, of which 100 are American, divided about equally between North and South America. Forty of the nearly 60 North American species described are based on specimens sent from the Carolinas by Dr. Garden, the others mainly on specimens in the museum of King Frederic.

There is not time to notice in detail the various classes of Cœlenterates. A few words about insects will serve as a general illustration for this phylum. Linnæus recorded about 2,400 species, the greater part of which he was the first to describe. About 300,000 are now recognized. Of the insects known to him, 65 per cent. are recorded in the second (1761) edition of his 'Fauna Sueccia,' and many of the remainder are European, so that his knowledge of exotic species was exceedingly restricted. Of Coleoptera he recorded about 800 species; the number now known is estimated at 12,000. Of Lepidoptera he recorded about 800; 7,000 are now known from North America alone. Of Diptera he recorded 278 species, of which 200 were from Sweden; 12,000 are now known from North America.

Linnæus's system of classification was based on a few external characters, and was recognized by himself as artificial and provisional. It was intended only as a stepping-stone to better things, when the structure and affinities of animals should become better known. The statistics already given indicate how limited was his knowledge of the world's fauna; his classification of animals shows how little he knew of their structure, and how often he was misled by superficial resemblances. Yet his 'Systema Naturæ' was the working basis of all naturalists for the next half century;[4] twelve editions were published during his lifetime, and it was later translated into several of the continental languages. To such an extent was it regarded as final by many subsequent naturalists that when his groups began to be changed and new genera interpolated it was deemed by some of them little less than sacrilege. When convenience demanded subdivision of the larger genera, owing to the great number of new species that had become known since 1766, it was quite common to consider the new groups as 'sections,' and to give them merely vernacular names, or, if their authors were bold enough to designate them by Latin names, they were commonly called subgenera.

It was not till near the close of the eighteenth century that there arose a new class of naturalists, the anatomical school, led by the elder Geoffroy and G. Cuvier, who studied the internal structure of animals as well as their external parts. It was, however, many years before the new systems began to displace or greatly to modify the long-accepted and strongly intrenched Linnæan methods of grouping animals.

The great advance in biologic knowledge since the time of Linnæus can not be easily measured; it can be suggested by noting the fact that comparative anatomy, embryology, histology, paleontology, evolutionism and many kindred lines of research have nearly all had their origin or principal development within the last century, all converging for the solution of the genetic relationships of animals and the origin of life. Linnæus, in an oration delivered in 1743,[5] held that each species of animal originated from a single pair, citing as incontrovertible proof the Mosaic account of the creation. It is indeed a long look back to the middle of the eighteenth century when his labors marked a new era in the history of biology. In commemorating to-day the two-hundredth anniversary of his birth, we honor ourselves by showing our esteem for the greatest naturalist of the eighteenth century.

J. A. ALLEN

[4] Turton, in his 'Life and Writings of Linné,' says: "To this system may be justly applied the nervous observations of Dr. Johnson, in his delineation of the character of Shakespeare. 'The stream of time, which is continually washing away the dissoluble fabrics of other systems, passes without injury by the adamant of Linné.'" —William Turton, 'A General System of Nature' * * * by Sir Charles Linné, Vol. VII., 1806, p. [42].

[5] In his oration 'De telluris habitabilis incremento,' delivered and first published in 1743 and republished in 1744, and again in the second volume of the 'Amœnitates Academicæ' in 1751, he gives his reasons for believing: "That at the beginning to the world, there was created one single sexual pair of every species of living thing.

"To the proofs of this proposition," he continues, "I request those who are my auditors to lend a favourable ear and willing attention.

"Our holy Faith instructs us to believe that the Divinity created a single pair of the human kind, one individual Male, the other Female: The sacred writing of Moses acquaint us that they were placed in the Garden of Eden, and that Adam there gave names to every species of animal, God causing them to appear before him.

"By a sexual pair I mean one male, and one female in every species where the individuals differ in sex: * * *—J. F. Brand's translation, in 'Select Dissertations from the Amœnitates Academicæ,' 1781, pp. 75, 76.

SCIENTIFIC BOOKS

Hydrates in Aqueous Solution. Evidence for the existence of hydrates in solution, their approximate composition, and certain spectroscopic investigations bearing upon the hydrate problem. By Harry C. Jones, with the assistance of F. H. Getman, H. P. Bassett, L. McMaster and H. S. Uhler. Pp. viii + 264. Publication No. 60 of the Carnegie Institution, Washington, D. C., 1907.

The first part of this monograph, covering 157 pages, presents the detailed results of earlier work and the measurements of Getman and Bassett of freezing-points, electrical conductivity, specific gravity and refractive power of solutions. All the observations were made on aqueous solutions, except a series of determinations made by McMaster, presented on pages 149 to 152, which relate to solutions of lithium chloride, bromide and nitrate in methyl and ethyl alcohol and to calcium nitrate in the latter solvent. In the case of the aqueous solutions, 98 compounds, including acids, salts and bases were studied; and in the course of this work between 1,400 and 1,500 solutions were examined. The purpose of making the specific gravity determinations was merely to permit a computation of the lowering of the freezing-point per 1,000 grams of solvent, the solutions having been prepared by dissolving a certain weight of substance in water and then diluting to a definite volume.

The refractive indexes when charted as ordinates against the concentration of the solutions as abscissas yield curves which are practically straight lines in all cases. They consequently bear no simple relation to either the conductivity or freezing-point curves, and are, indeed, not used later in the argument concerning hydrates in solution.

The electrical conductivities of the solutions were measured for the purpose of ascertaining the so-called degree of electrolytic dissociation of the dissolved substances. When the molecular conductivities are charted as ordinates and the corresponding volumes as abscissas, smooth curves are obtained in all cases. These curves have in general a similar trend, being concave toward the axis of abscissas for the various electrolytes investigated.

The ebullioscopic method was employed in but few of the aqueous solutions; the alcoholic solutions were, of course, investigated by this method. On the other hand, the cryoscopic method was very extensively employed in studying the aqueous solutions, and in order to make comparison possible, the electrical conductivity measurements were made at 0° C. When the molecular lowerings of the freezing-point are charted as ordinates and the molecular concentrations as abscissas, curves are obtained which are in general convex toward the axis of abscissas. In some cases the points of inflexion are quite well defined, in other cases the curves are fairly flat, and in still others they are nearly straight lines. In the few cases where boiling-point curves have been similarly charted, these show the same general behavior, except that the points of inflexion occur at a greater concentration than in the freezing-point curves.

The chief argument presented in the monograph for the existence of hydrates in solution is the occurrence of minima in these freezing-point and boiling-point curves. From the fact that the minima occur at higher concentrations in the boiling-point curves, it is argued that the hydrates are less stable at higher temperatures and hence require greater concentration for their formation than at lower temperatures. To be sure a point is also made of a supposed relation between lowering of the freezing-point and water of crystallization of the dissolved substance, it being claimed that high crystal water content and a large depression of the freezing-point go together. As a matter of fact, however, the behavior of solutions of many salts rich in crystal water speaks strongly against this supposed relation. So for instance, the sulphates in general give relatively small lowerings of the freezing-point, a fact which, moreover, is not new. This behavior of the sulphates it is stated is 'abnormal' and due to polymerization of the salts. However, no independent evidence is adduced to support the assumption that such polymerization occurs, and so the relation which it is claimed exists between lowering of the freezing-point and crystal water content of the solute can not be considered as established. The well-known fact that salts generally separate from hot solutions with less water of crystallization than from cold solutions is also regarded as evidence of the existence of hydrates in solution.

The fact that salts frequently crystallize with water has long been considered as evidence in favor of the assumption that hydrates exist in solution. The reasoning has simply been that whatever separates from a solution at a given temperature has also existed in that solution. So in this respect the paper really presents nothing new. Furthermore, it has long been known that the various physical properties of solutions can not be foretold on the assumption that solutions are merely mechanical mixtures of solvent and solute. The deviations which the properties of solutions have shown from those computed on the assumption that they are merely mechanical mixtures have always been regarded as evidence in favor of the idea that in the process of solution some form of combination between solvent and solute takes place; and so the argument in favor of the existence of hydrates in solution based upon the occurrence of minima in the freezing-point curves also presents nothing new in principle. However, there has been in the past great difficulty in determining definitely just what the composition of the supposed hydrates in solution really is, and as Professor Jones presumes to compute, at least approximately, the composition of these hydrates, the vital interest of the whole monograph really centers upon the results of these computations.

Now an inspection of the method adopted by the author for determining the approximate composition of the hydrates in solutions shows that it consists of assuming the approximate validity of the simple gas laws for all solutions, concentrated as well as dilute; and that in the case of electrolytes the solutes are electrolytically dissociated, the so-called degree of electrolytic dissociation being calculable according to the well-known formula of Arrhenius even in concentrated solutions, at least approximately. On the basis of these assumptions it is computed what the molecular lowering of the freezing-point ought to be; and from this value and that actually found in practise, it is calculated how much water is combined with the solute. In other words, the method simply consists of assuming just so much of solvent as combined with the dissolved substance in any given case as is necessary to make the simple gas laws hold even for concentrated solutions. That the gas laws do not hold for concentrated solutions, even approximately, is a well-recognized fact; and consequently the numerous computations of the approximate composition of the supposed hydrates in the solutions studied and the curves representing the variations of the composition of these hydrates with change of concentration need not be further considered here, for they are quite valueless. The republication of the results of the calculations of the composition of these supposed hydrates in this monograph is all the more inexcusable because Van Laar[1] had already called attention to the gross error of Jones and Getman when a part of the work appeared in the *Zeitschrift für physikalische Chemie*, 49, 385, 1904.

While then the monograph has failed in what must be regarded as its main objective, namely, the computation of the approximate composition of hydrates in solution, the large bulk of experimental evidence presented, though also in part not new, undoubtedly goes to show that solutions are not merely mechanical mixtures, but that combination of a specific nature does take place between solvent and solute. The experimental data recorded will no doubt prove useful in the future.

Part II. of the paper, pages 161 to 259, presents the results of H. S. Uhler of the study of the absorption spectra of aqueous solutions of cobalt chloride, copper chloride and copper bromide and of these solutions mixed in different proportions with calcium chloride, calcium bromide or aluminum bromide. The absorption spectra of cobalt chloride, copper

[1] *Chemisch Weekblad*, 2, 6–8, 1905. Abstract in *Chem. Centralblatt*, 76, 491, 1905. Also reprinted in 'Vorträge über thermodynamisches Potential,' pp. 6–8; published by Vieweg u. Sohn, Braunschweig, 1906.

chloride and copper bromide in methyl and ethyl alcohol and in acetone are also studied. Photographic records showing the absorption bands in the different solutions are presented. These photographs show admirably how the absorption bands gradually change as the various dehydrating agents mentioned are added to the solutions. In the discussion of the results the difficulties met in interpreting spectroscopic observations of this kind are clearly set forth. In fact the second part of the monograph is more carefully and judiciously written than the first part, the latter containing practically no reference to the experimental work of others, whereas in the second part the work and opinions of other investigators receive due consideration. Attempts to deduce the approximate quantitative composition of the hydrates in solution from the study of the absorption spectra are, of course, not made, for such observations do not lend themselves for that purpose. It is simply claimed that the results bear out Jones's ideas of hydrates in solution in a satisfactory manner.

Louis Kahlenberg
University of Wisconsin,
May 10, 1907

Electrons or the Nature and Properties of Negative Electricity. By Sir Oliver Lodge. London, George Bell and Sons. 1906. Pp. xvi + 230.

Up to the time of the publication of Maxwell's 'Treatise on Electricity and Magnetism' in 1873, in which the ideas of Faraday were subjected to mathematical analysis, and greatly extended, the preponderating rôle which the luminiferous ether plays in electrodynamic phenomena had been almost entirely overlooked by theoretical physicists. Attention had been concentrated upon electrical charges and currents, and the influence of the medium which transmits electric and magnetic forces was quite neglected. The steady progress of Maxwell's theory, its evident superiority to its older rivals, and its final triumphant verification by Hertz, naturally caused the pendulum to swing to the other extreme; so that in the early nineties students of physics learned much of tubes of force and ethereal displacement, but had little to do with electric charge, except as a rather old-fashioned idea still useful for certain purposes, but really only a short name for a certain mathematical function of the ethereal displacement.

Time, however, has wrought its revenges. When Maxwell's theory was applied to the finer details of the electrical and optical behavior of matter, especially of moving matter, it was found necessary to reintroduce the definite conception of electric charge in very concrete form; its atomic structure was recognized, and the name 'electron' was given to the atom of electric charge. These theoretical conclusions of Lorentz and Larmor have been strikingly confirmed by a great number of experimental results in widely different fields of investigation. The discovery of the Zeeman effect, the investigation of the nature of cathode rays and of the ionization of gases by J. J. Thomson and his followers, the phenomena of radioactivity and many other facts new and old find their natural explanation in terms of the electron theory. Indeed we have some ground for anticipating a much wider extension of the theory; it is not impossible that we may come to believe that all matter is made up of electrons, which will thus form the raw material out of which the material universe is constructed.

In the book under review, Sir Oliver Lodge has given a simple and lucid account of this theory, of its triumphs and difficulties, and the possibilities of its future development. He tells us that the book "is intended for the student of general physics, and in places for specialists, but most of it may be taken as an exposition of a subject of inevitable interest to all educated men." It is in fact not quite a 'popular' exposition of science in the ordinary sense, and one will look in vain for the exaggerations and over-statements which are too often characteristic of such works. It appeals rather to the discriminating *amateur* of scientific knowledge who has some knowledge of electricity and who will not be hopelessly frightened or repelled by an occasional simple algebraic or trigonometrical expression.

Readers of this class will doubtless find this book of great interest, and the general view and perspective of the subject will also be of value to the professional physicist. It is written with the spirit and enthusiasm which we have learned to expect from Sir Oliver Lodge since the publication of his 'Modern Views of Electricity'—nearly twenty years ago—that remarkable little book which, by a masterly use of mechanical analogies and models, gave an exposition of Maxwell's theory that was understood and enjoyed by many non-mathematical readers and was at the same time capable of exciting the admiration of Helmholtz. It can not be said that the present work is the equal of the earlier book; but taken together they afford a view of the progress of electrical theory during the past thirty years which can hardly be got elsewhere in the same compass.

H. A. BUMSTEAD
YALE UNIVERSITY

SOCIETIES AND ACADEMIES

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 191st meeting of the society, held on Wednesday, April 10, Mr. F. E. Wright exhibited four new attachments for the petrographic microscope and gave a brief description of each: (*a*) Double screw micrometer ocular by means of which the optic axial angle of any section under the microscope can be measured in convergent polarized light, if one optic axis, at least, appears within the field of vision. (*b*) A special cross-section ocular, which consists of a Ramsden-Czapski ocular with a fine coordinate scale in the focal plane and which serves the same purpose as the double-screw micrometer ocular, although slightly less accurate. (*c*) An improved Fedorow-Fuess universal stage on which new hinged graduated circular scales have been added and found to increase the general applicability of the stage considerably. (*d*) A new condenser-lens system combining the advantages of the ten Siethoff system with the quantitative movements of the universal stage.

Regular Program

Mr. Bailey Willis discussed the geological problem of the Alps especially from a structural standpoint and compared the conclusions reached by him with those held by the majority of European students of the Alps.

At the 192d meeting of the society, on April 24, Mr. Bailey Willis presented and briefly explained a diagram on the possible development of recumbent folds as a consequence of thrust faults of great magnitude. This paper will be published in SCIENCE.

Mr. F. L. Hess exhibited informally specimens of gypsum from Lost Hills, fifteen miles south of Tulare Lake, California.

Regular Program

A Peculiar Form of Metamorphism in Siliceous Sandstone: Mr. GEO. P. MERRILL.

Mr. Merrill described a peculiar and apparently very local form of metamorphism of the siliceous sandstone which underlies the Aubrey limestone in the vicinity of Canon Diablo, Arizona. The materials shown and discussed were from the so-called Coon Butte Crater, and were of unusual interest, not merely on account of the character of the phenomena, but as bearing upon the question of the origin of the crater itself.

It was shown that the sandstone, composed of very pure quartz sand, passed by gradations into an almost wholly crystalline rock composed of remolded quartz granules with a well-developed rhombohedral cleavage, and thence into glassy and pumiceous forms closely resembling a bleached liparite pumice. The metamorphism was accompanied by no apparent chemical changes that could be considered constant or essential. Mr. Merrill discussed the bearing of this metamorphism upon the origin of the crater, but refrained from committing himself upon the subject.

Normal Faulting in the Bullfrog District: W. H. EMMONS. This paper will be published in SCIENCE.

Some Problems Concerning the Formation of Coal: DAVID WHITE.

Under this title Mr. White presented an outline of the more important questions relating to the deposition of vegetable matter and its transformation into coal. He observed that the 'transportation' theory appeared to be fully vindicated in a number of instances, though in the vast majority of cases the plant material seemed to have undergone little, if any, appreciable drifting. The great extent and regularity in thickness, including partings and benches, of the Pittsburg bed in the Appalachian trough appeared not to find a completely satisfactory explanation in either the transportation or the peat-bog theories. Referring to the fact that the process of coal formation is marked chiefly by the progressive deoxygenation of the vegetable matter, especial attention was directed to the most important rôle played by anerobic bacteria in the decomposition of the plant tissues and their incipient coalification. Though it is not yet determined how far bacterial action may have gone in changing vegetable matter into bituminous coal, etc., the function of these deoxygenating organisms in accomplishing the primary stages of coal formation, including at least the breaking down of the vegetable hydrocarbons and the development of the fundamental jelly, is now rapidly gaining acceptance on the part both of geologists and of chemists. Concerning the successive development of lignites, bituminous coals and anthracites there still remain radical differences of opinion, it being maintained by many, including several paleobotanists, that the higher grades of coals have never passed through a peaty, lignitic or other lower stage. The solution of this problem, whose principal point is the origin of the bituminous coals, is awaited with the greatest interest. The speaker urged that the cause of anthracitization in the Eastern Appalachian regions rested primarily not in local folding, but in the deeper-seated metamorphism due to the great post-Carboniferous Atlantic thrust. This is shown by the progressive devolatilization of the coals in passing eastward across the Appalachian basins. The folding was merely incidental and relieving.

Another group of problems involves the determination of the very intimate and important relations existing between the kinds of original contributary organic matter and the kinds and qualities of the resultant fuels. It is known that certain groups of coals owe their principal distinctive features to the nature of their original constituents. In this connection the speaker suggested that the coking property in some of the Appalachian coals was probably due to a combination of 'sapropelic' (fatty) or strictly bituminous organic matter, with the ordinary 'humic' (jetty, brilliant) or so-called 'bituminous' coal-forming material, which is largely composed of the remains of the higher types of plants.

Emphasis was put on the urgent need for new and more complete experiments in synthetic coal-making, concerning which the testimony of previous experimenters is highly conflicting. Such experiments carried out with new and more adequate methods and equipment, comparable to those employed in the Geophysical Laboratory of the Carnegie Institution, would not only furnish most valuable data relating to coal transformation, but they would also probably furnish a conclusive answer to the question of the development of bituminous coals or anthracites through peat, lignite, or subbituminous coal stages.

The Jamaica Earthquake: Mr. J. W. Spencer.

Jamaica is separated from the volcanic and earthquake zone of the Windward Islands by the broad deep basin of the Caribbean Sea, nearly 1,000 miles across, and from that of Central America by about 600 miles. Jamaica is on a plateau connecting this latter region with Haiti; but much the greater part of the intervening distance between the Central American volcanic and earthquake zone and the island is occupied by low plains, or these only slightly submerged beneath the sea, and crossed by one relatively narrow channel reaching to a depth of nearly 3,000 feet. The trend of this Jamaican ridge being at right angles to the lines of the earthquake actions of the two ends of the Caribbean Sea, it would seem that the seismic effects could not have any relationship with others of the region so far away. Moreover, Jamaica is not volcanic, with only the remnants of one Pliocene volcano upon the northern coast. On its southern side, which is mostly a plateau from one to two thousand feet high, capped by white Oligocene limestones much denuded, a coastal plain extends some ten miles seaward under less than 100 feet of water. Above the sea the plain is only five or six miles wide to the base of the mountains. Kingston is situated on the side of a long narrow bay separated from the sea by a beach ridge called the Palisades, on the point of which is Port Royal. A short distance east of Kingston the mountains come to the sea, which suddenly deepens to 500 feet within a half mile of the shore. This is the head of an embayment in the mass of submerged land, and plunges rapidly to a depth of 4,000 feet. Beneath the limestones mentioned are Pteropod oozes, which are exceedingly plastic when wet. The earthquake, while felt over the island, was only intense about Kingston, and still more so to the east at the head of the submarine embayment mentioned. As this is the second great earthquake which has occurred here within recent times, Mr. Spencer was inclined to regard this local feature of the deep embayment as its direct cause; and suggested that it might possibly be due to a gigantic submarine landslide originating perhaps at a depth of 4,000 feet or more. The land appears to have shifted and part of the beach of Port Royal has sunken beneath the sea, which is a repetition of the features of the former earthquake. While part of the possible submarine shifting is due to this material, as at Port Royal, a greater cause might be found in the movement of the consolidated beds over the Pteropod ooze similar to gigantic creeping over similar beds in Barbados.

FRED E. WRIGHT,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 635th meeting was held on May 11, 1907.

Mr. Becker read a paper on current theories of slaty cleavage which will soon appear in the *American Journal of Science*. The speaker first mentioned very briefly the views of various writers concerning the origin of cleavage, but dwelt at some length upon the theories of Sharpe, Van Hise and Leith. Sharpe's theory was that cleavage is due to pressure normal to the cleavage plane. Sedgwick and others held cleavage mainly a phenomenon due to crystallization or recrystallization of minerals in an appropriate orientation. Messrs. Van Hise and Leith have sought to combine and amplify these theories. They hold that minerals, especially mica, tend to crystallize perpendicularly to the greatest stress and also perpendicularly to the least axis of the strain ellipsoid.

Mr. Becker pointed out that the chances of a pure strain, such as called for by Sharpe's theory, are extremely small, not more than one in 10,000 and probably much less. The surfaces perpendicular to the greatest internal stress in a homogeneous mass subjected to progressive uniformly distributed normal external pressure are the equipotentials and are well known to be hyperboloids of revolution. They are thus surfaces of double curvature corresponding to no known phenomena in slates. In the case of rotational strains the equipotentials would be still more complex and become intersecting systems of surfaces of double curvature, and thus still less available to explain cleavage.

Mr. Becker took up one by one the lines of evidence presented by Mr. Leith to establish the position of the strain ellipsoid and the hypothesis that the least axis is normal to the cleavage. The elongation of pebbles is not applicable to this purpose, for the final form and attitude of a pebble result from the superposition of the strain ellipsoid and the rotation of the strain on the original ellipticities of the pebble. Its elongation does not even tend to coincide with that of the strain ellipsoid excepting in highly exceptional limiting cases. Numerical examples were given showing that pebbles of ordinary forms may be elongated in directions 20° or 30° from the major axis of the strain ellipsoid. Fossils as hitherto treated afford no better guide.

Mr. Becker showed that the slicing of pebbles could be satisfactorily explained on the theory that cleavage as well as jointing occurs on planes of maximum slide. He rejected Mr. Leith's inferences from the evidence and concluded:

It appears to me that Mr. Leith is in duty bound to make public exact reasons for his assertions, to give precise methods for determining the position of the strain ellipsoid or the equipotentials in a slate, to show why there is no cleavage on planes of maximum slide, and to explain thermodynamically how it happens that the planes on which the entire energy of deformation is expended are not those on which feldspar is converted into mica. We are past the stage in which mere opinions or general impressions should be allowed decisive weight.

At the conclusion of Mr. Becker's paper its discussion was taken up by Mr. Bailey Willis with special reference to other theories of rock cleavage. Mr. Willis called attention to the very broad observational basis of the explanations of rock cleavage given by Messrs. Van Hise and Leith and pointed out that Mr. Becker approached the problem as one of pure mathematics and solved all the complex phenomena by a single theoretical application of principles. Granting the correctness of the principles and their appropriate application to a purely mechanical distortion, one may very reasonably question their adequacy to explain the facts emphasized by Leith, namely, the rearrangement and recrystallization of minerals with parallel orientation of crystalline cleavage and major axes of particles.

Leith recognizes a variety of cleavage (fracture-cleavage) developed in the planes of maximum slide, as Becker calls them, but limits that development to the zone of fracture. Willis took exception to this limitation and expressed the view that in regions of great tangential movement displacement on planes of maximum slide (*i. e.*, on planes oblique to the direction of tangential pressure) may occur in a deep-seated zone, where fracture is impossible and flow cleavage develops by recrystallization and orientation in those oblique planes. The Appalachian region of North Carolina appears to present such displacement with appropriate cleavage.

On the other hand, there are regions where the movements of large masses are essentially vertical, as is shown by warping of the surface. Such an area is that of the Canadian Highlands. In so far as the vertical swelling may be attributable to tangential stress it indicates vertical elongation of the mass, which may be and probably is accompanied by recrystallization and orientation of mineral particles, producing flow cleavage in the normal planes.

Thus it seems probable that cleavage involves molecular rearrangement, of which Becker does not take sufficient account, and that flow cleavage may develop in oblique planes, which Leith does not consider, as well as in the normal planes, which he does recognize.

R. L. FARIS,
Secretary

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

THE annual meeting of the Washington Anthropological Society was held May 28, 1907, with Mr. J. D. McGuire in the chair. The program consisted of the annual report of the treasurer; preliminary communications; notices of deceased members; and annual election of officers.

The treasurer's report showed that during the fiscal year ending December 31, 1906, the total receipts amounted to $587.78, the total expenditures to $398.22, leaving a balance of $189.56.

Mr. Hewitt presented an extract of the monthly field report of Dr. J. R. Swanton, bearing on the scattered remnants of the Indian tribes in Louisiana.

The obituary notices were delivered as follows: (1) Dr. Cyrus Thomas on Dr. Ernst Foersterman, of Germany, honorary member of the society, born 1818, died 1906. Special reference was made to his contributions to the knowledge of the Maya hieroglyphics. (2) Professor W. H. Holmes on Sr. Alfredo Chavero, born in Mexico, 1822, died 1906. He was equally eminent as litterateur, statesman and archeologist. (3) Mr. J. Mooney on Dr. Albert S. Gatschet, born in Switzerland in 1832, died in Washington, 1907. Dr. Gatschet was for many years on the staff of the Bureau of American Ethnology, and devoted himself principally to the linguistics of the American aborigines, in which field he was a successful pioneer. (4) Dr. A. Hrdlička presented a notice on Professor Emil Schmidt, born in 1837, died in 1906, in Germany. Professor Schmidt's special field was physical anthropology, in which he did great service in Germany. He contributed important studies to the anthropology of Egypt, South India and Ceylon, and particularly to that of the prehistoric times in North America, which country he twice visited.

The election of officers for 1907–8 resulted as follows:

President—Aleš Hrdlička.

Vice-presidents—Section of Somatology, D. S. Lamb; Psychology, J. W. Fewkes; Esthetology, W. H. Holmes; Technology, Walter Hough; Sociology, James Mooney; Philology, J. N. B. Hewitt; Sophiology, Alice C. Fletcher.

General Secretary—Walter Hough.

Secretary to Board of Managers—J. R. Swanton.

Treasurer—Geo. C. Maynard.

Curator—Marianna P. Seaman.

Councilors—J. W. Fewkes, J. B. Nichols, James Mooney, J. N. B. Hewitt, J. R. Swanton, W. E. Safford, F. W. Hodge, J. M. Casanowicz, Sarah S. James, P. Beckwith and F. O. Stetson.

Committee on Communications—W. H. Holmes, Alice C. Fletcher, James Mooney.

J. M. CASANOWICZ,
Acting Secretary

THE CHEMICAL SOCIETY OF WASHINGTON

THE 175th regular meeting of the Chemical Society of Washington, and joint meeting with the American Society of Biological Chemists was held at the Cosmos Club, May 9, 1907, Dr. Peter Fireman, of the Chemical Society, and Dr. W. J. Gies, of the Biological Society, presiding.

The following program was carried out:

Chemical and Bacteriological Standards now in Use in Water Analysis: JOSEPH H. KASTLE.

Ammonia in Milk and its Development during Proteolysis under the Influence of Strong Antiseptic: H. C. SHERMAN, W. N. BERG, L. J. COHEN, and W. G. WHITMAN.

Studies on Apple Juice: H. C. GORE.

Sugar Metabolism: HUGH MCGUIGAN. (Read by title.)

The Presence of Secondary Decomposition Products of Proteids in Soils: OSWALD SCHREINER and EDMUND SHOREY.

On Lysylglycin: P. A. LEVENE and W. A. BEATTY.

Negative Evidence of the Development of Ferments of Dog's Saliva in Adaptation to Diet: WALTER E. GARRY. (Read by title, but results of investigation were discussed by Dr. L. B. Mendel and Dr. W. J. Gies.)

Some Azolitmin Compounds of Mucoids, Nucleoproteins, and other Proteins, with Exhibition of Products: JACOB ROSENBLOOM and W. J. GIES.

On the Quantitative Determination of Mucoid in Urine, Blood and Tissue Extracts: C. E. MAY and W. J. GIES.

J. A. LECLERC,
Secretary

THE TORREY BOTANICAL CLUB

THE meeting for May 14, 1907, was called to order at the American Museum of Natural History with President Rusby in the chair. One hundred and four persons were present.

The scientific program consisted of a symposium of four papers on the subject of 'Trees.' Each paper was illustrated by lantern views. The symposium was opened by Professor L. M. Underwood, who spoke on 'Some Historic American Trees.' Mr. William Solotaroff discussed 'The Planting and Care of Shade Trees,' giving an interesting account of the preparation for planting in the nursery, methods of transplanting along highways and streets, the dangers that threaten shade trees, and briefly of the means of protecting against these dangers.

Dr. E. B. Southwick spoke on 'Trees in Winter,' showing views taken in Central Park, and elsewhere in and about Greater New York. The last number was by Dr. N. L. Britton, who showed a selection of colored lantern slides from the Van Brunt collection, illustrating the flowers and fruits of common trees.

At the conclusion of the regular program, Mr. Edward R. Taylor, of Pen Yan, N. Y., exhibited some beautiful, and botanically interesting, samples of 'vegetable silk,' and fabrics woven from it, together with the raw material of which it is made. The process of its manufacture from cotton-seed-cellulose was briefly described, and samples of 'artificial horse-hair,' made by treating ordinary cotton thread with the dissolved cotton-seed-cellulose, were also exhibited and the process of its fabrication briefly described.

C. Stuart Gager,
Secretary

IOWA ANTHROPOLOGICAL ASSOCIATION

The fourth yearly meeting of the Iowa Anthropological Association was held at the Davenport Academy of Sciences, Davenport, Iowa, on Friday and Saturday, May 17 and 18, the sessions being well attended. On Friday evening Professor Frederick Starr, of the University of Chicago, delivered an address on 'The Field of Folk-lore in Mexico,' illustrated with stereopticon views. On account of his numerous trips to Mexico and his intimate acquaintance with the people of that country, Professor Starr's address was of unusual interest.

On Saturday morning, after some introductory remarks by the president, Professor B. F. Shambaugh, of the University of Iowa, and the report of the secretary, J. H. Paarmann, curator of the Davenport Academy of Sciences, there were a number of papers by members of the Davenport Academy of Sciences dealing with different aspects of the explorations recently made near Albany, Whiteside County, Illinois. These were as follows:

Edward K. Putnam: 'Bibliography of the Albany Mounds.'

Professor Samuel Calvin, University of Iowa: 'The Geology of the Region in the Vicinity of Albany.'

W. H. Kimball: 'Report of the Survey of the Albany Mound District.'

J. H. Paarmann: 'Report of the Opening of Two Mounds Near Albany.'

Dr. A. W. Elmer: 'Iron found in the Albany Mounds.'

J. E. Calkins: 'Some Puzzles at Albany.'

After luncheon at the Davenport Commercial Club, various papers on current anthropological topics were presented as follows:

Richard Herrmann, Dubuque, Iowa: 'Mound Builders of the Mississippi Valley.'

Professor Bohumil Shimek, University of Iowa: "The Loess and the 'Nebraska Man.'"

Professor C. C. Nutting, University of Iowa: 'Urn Burial on the Island of Ometepe, Nicaragua.'

Rabbi W. H. Fineschriber, Davenport, Iowa: 'Some Facts of Jewish Anthropology.'

Professor Frederick Starr, University of Chicago: 'The Davenport Academy's Collection of Objects from the Upper Kasai, Congo Free State.'

The following officers were elected:

President—Edward K. Putnam.

Vice-President—Bohumil Shimek.

Secretary—J. H. Paarmann.

Treasurer—A. G. Smith.

Executive Committee—F. J. Becker, I. A. Loos, G. T. Flom, C. C. Nutting and A. W. Elmer.

On Saturday evening a meeting was held to organize an Iowa branch of the American Folk-lore Society, the program being as follows:

Edward K. Putnam, Davenport Academy of Sciences: 'Introductory Remarks on Folk-lore.'

Professor Charles Bundy Wilson, University of Iowa: 'German-American Folk-medicine.'

Professor G. T. Flom, University of Iowa: 'The Myths of Creation and Doom in Norse Mythology.'

Professor Jules Mauritzon, Augustana College, Rock Island, Ill.: 'Traces of Old North Heathenism in Swedish Christmas Customs of To-day.'

Professor Charles Bundy Wilson, of the University of Iowa, was elected president and Edward K. Putnam, of Davenport, secretary.

J. H. Paarmann,
Secretary

DISCUSSION AND CORRESPONDENCE

THE FIRST SPECIES RULE: AN OBJECTION

To the Editor of Science: The rules that have been drawn up by a committee of American zoologists, with a view of determining the genotype of every genus in a manner that can admit of no uncertainty, may perhaps not yet have been published, but, to judge from the manuscript copy which I have been permitted to see, they are admirably adapted to secure their object. The question remains whether the object is a good one; and it is the purpose of this letter to point out a set of cases in which the selection as genotype of the first species mentioned would be likely to introduce confusion.

When genera are based on recent species it may be assumed that, as a rule, no one species is more obscure than another, so that, *ceteris paribus,* there is no objection to taking as genotype the first mentioned. Indeed the first mentioned is in most cases likely to be the best known, although every one is aware that it is not so in all cases. But when genera are based mainly or entirely on fossils there is a great difference between the values of the different species. The common-sense thing to do in such a case is to select the most completely preserved and best-known species as genotype.

If now one considers the general action of previous revisers in dealing with paleontological material, one notes a general tendency either to select as genotypes, or at all events to regard as representative, those species which are the least obscure. In other words, the historical development of the science has resulted in the common-sense method of interpreting genera by their best known species.

To leave the selection to an arbitrary rule that is as likely as not to fix on an obscure fragment would in itself be contrary to common sense; but the first species method now proposed not merely ignores these important factors of clearness and familiarity, but actually tends in the case of fossils towards the greatest obscurity. For this reason, many new species established by paleontologists have been introduced in monographs or papers dealing with series of fossils from various horizons; and it has generally been the custom of paleontologists, in discussing the species under review, to follow a stratigraphical order, beginning with the oldest rock. Consequently, when a paleontologist founds a new genus, the first species that he mentions is generally the oldest, and for this very reason it is generally also the most obscure. In many cases then the first species rule would lead to the inevitable selection of the most obscure species as the type of a genus. A rule of which this can be said may work with mathematical exactness and automatic precision, but its final result must be to introduce, or rather to force, into zoological nomenclature fresh elements of uncertainty and change.

F. A. BATHER

LONDON, S. W., ENGLAND,
April 29, 1907

THE GREAT INFERIOR TUSKED MASTODON OF THE LOUP FORK MIOCENE

IN 1882 I discovered a jaw of this mastodon on the Sappy, in Decatur County, while in the employ of the Museum of Comparative Zoology at Cambridge. A single jaw was present measuring four feet to the end of the tusk. Last year while on an expedition for the Royal Museum of Munich in the same beds on the Prairie Dog, I collected a very perfect set, without the tusks; length of the preserved jaws two and a half feet, height at condyles fourteen inches, height of grinding surface of the single last molar nine and a half inches. Last season my son was so fortunate as to discover in Scott County, near the Gove County line, a very complete and well-preserved set of lower jaws of a huge specimen so different in several respects from the other two mentioned, a separate form may be represented. The peculiarity lies in the low condyle that is only thirteen and a half inches high, and in the great depression of the rostrum, thirteen inches lower than the teeth at its distal end. The length of the jaws are four feet and one inch long. The distance between the condyle and distal end of rostrum or beak, four feet three and a half inches. But one well-preserved molar, the last, in each jaw their greatest height is ten inches; height of crown two and a half inches, length nine and a half inches, width three inches, distance between the molars four inches. This is the largest specimen in my experience ever taken from the Loup Fork Miocene beds of Kansas, and point to an animal of large proportions.

In this connection I would like to put on record the description of the largest tusk of *Elephas columbi,* or the great Columbian mammoth, of which such a fine example is now mounted in the American Museum, New York City. I discovered this tusk with a lot of teeth; several of them are now preserved in the State University Museum of Kansas and others in the American Museum in Lane Co., Kansas.

It measured fourteen feet in length and was eight inches in diameter at its proximal end where it was broken from the upper jaw. Dr. Matthews assures me that it is the largest specimen so far recorded, I regret to say that it was not saved owing to its friable nature.

Charles H. Sternberg

CONCERNING STENO

To the Editor of Science: It happens to all of us to fancy that what is new to us must be new to the world; and if we fail to look in the right places we do not become disenchanted.

The recent reference in Science (May 10) to Steno's noted work on crystallography and a newly-discovered English translation of it, led me to wonder if all the great bibliographers had overlooked the latter. The four authorities that came to hand first were Brunet, the British Museum Catalogue, Watts's 'Bibliotheca Britannica' and Poggendorff's 'Biog.-lit.-Worterbuch'; all of these include H. O.'s translation of Steno, except Brunet, who has few scientific titles and does not include this in any language. The translation is also cited in the 'Catalogue' appended to Young's 'Natural Philosophy,' about 1805.

The guess and conclusion that H. O. was Henry Oldenburg is confirmed by the article about him in the 'Dictionary of National Biography.'

This incident will strengthen the views of those who think that a prerequisite to any advanced degree should be a short course in bibliography; for, whatever Pope meant by his lines, they are increasingly true to-day:

> * * * Index-learning turns no student pale,
> Yet holds the eel of science by the tail.

C. K. W.

Washington, D. C.,
June 4, 1907

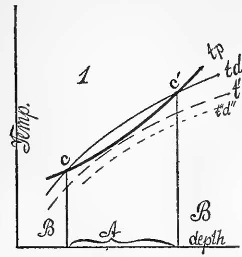
SPECIAL ARTICLES

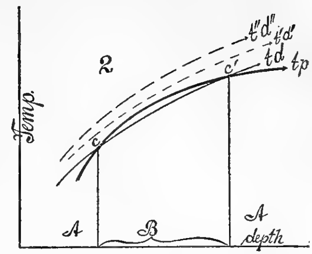
ON SUN SPOTS

Apropos of certain recent discussions on solar activity to which I listened with pleasure in Philadelphia, I have wondered whether a possible analogy between geyser-like action and periodic solar disturbance has been suggested. For instance, let the line *td* in the diagram represent the distribution of temperature and depth below the solar surface, or, from some points of view, the distribution of temperature relative to pressure. Let the line *tp* represent the condition of transition, referred to temperature and depth, from an atomic form *A* to an atomic form *B*. Below the *tp* line the element *B* is stable, above it *A* is stable. At depths corresponding to *c* or *c′*, therefore, neither form is persistently stable, but as the spherical shells are thin there need be no marked consequences. To make the engine work, two points of intersection, *c*, should occur.

I shall assume that the transition of *A* into *B* takes place along a doubly inflected intrinsic isotherm for the system *AB*, after the manner explained by James Thomson and Van der Waals. It therefore requires a certain amount of 'supersaturation,' or an excess of heating, to affect the transfer from *A* to *B*, in the absence of special external interferences. I shall also assume that the transfer *A* to *B* is accompanied by an evolution of heat, *B* to *A* by an absorption of heat, and that the *A* matter is eliminated from the whole active region by gravitational convection. Finally different atomic forms are arranged between concentric spherical shells, according to their density.

Suppose, therefore, as a first alternative, that after a sun-spot period, the *td* line has been depressed by the sudden cooling of all active strata to the position *t′d′* in Fig. 1. The points *c* have been displaced towards each other and have quite vanished from the curve. *B* matter only is present. In the lapse of time, however, the line *t′d′* again rises to reach *td*, due to heat arriving from below, within the depths bracketed in the now unstable state *A*. It is agreed that the *td* position will have to be very closely approached, or a considerable 'supersaturation' will be required, before another eruption occurs, which drops the *td* line to *t′d′* in turn. Whereas the depression of this line is relatively sudden, its gradual rise together with the prop-





erties of the AB system determines the frequency of the sun-spot period: the element A escapes (let us say) in gaseous form. As the curves are drawn there is not sufficient 'supersaturation' to transmute A to B. This would occur at $t''d''$ for instance.

The relation of the curves td and tp will vary with the solar latitude, for td is a mere graph of temperature and depth while tp is inherently a graph of temperature and pressure. On passing from pole to equator tp moves bodily from left to right in diagram. At the poles the region A may thus be permanently absent, while at the equator $t'd'$ may never fall below tp. Hence, an intermediate sun-spot zone may be inferred.

Finally it is clear that the cycle of changes may be reversed as in Fig. 2. Let the transition of A into B be a source of heat, and let the ensuing eleven years of the sun-spot period be an interval of cooling. Immediately after eruption, the temperature depth line will have some high position, $t'd'$, and A matter only is in occurrence. As the sun cools superficially, $t'd'$ tends toward td, and between the depths c and c', B matter is potentially present. When, after sufficient 'supersaturation,' the transition A to B ultimately does occur, tp is again raised to $t'p'$; and so on in turn. The engine works at the expense of atomic energy, supposing that B matter[1] is continually eliminated from the active region by gravitation, as the $t''d''$ condition is not reached.

CARL BARUS

[1] If corpuscles leave A to transfer it into B, an absence of these may prohibit the reversed transfer BA.

DOES THE MAMMALIAN HEART OBEY THE LAW FOR CHEMICAL REACTION VELOCITIES AS INFLUENCED BY TEMPERATURE?

IN a recent paper[1] it was shown that the temperature coefficient for the velocity of the heart-beat in mammals is the same as that for a purely chemical reaction. From the results of previous workers who experimented upon the influence of temperature upon the mammalian heart this fact was shown in a clear and decisive manner. At that time the writer had not yet seen Herlitzka's paper.[2] This author takes data out of only three of the fifteen to eighteen tables in Langendorff's paper, and determines a few constants. In one case, table XIV., in a range of temperature from 23° to 39° C., he exhibits only five constants. Between 29° and 35.2° there are no constants shown, but for these degrees he finds the constants 3.45 and 10.8, and then complains that these constants do not represent the curve of the formula for a chemical reaction velocity for temperature, but rather a straight line. In tables VII. and VIII. he says that the values observed correspond well enough with the values of K calculated from the formula, but that one, for 35° (3.74), is too high!

Herlitzka has looked at a few individual cases of Langendorff's results. On the other hand, the present writer put all of Langendorff's results (from fifteen cat hearts) together into one table; put the figures in round numbers and determined the constants from

[1] Snyder, Charles D., *Amer. Jour. of Physiology*, December 1, 1906.

[2] Herlitzka, 'Richerche sull' azione della temperatura sul cuore isolato di Mammifero,' *Zeitschrift f. Allgemeine Physiologie*, V., 264, 1905.

10° to 46°. Of these constants only those at the extreme limits of temperature vary from the 2–3 limit set for the chemical reaction constant. As to his own experiments on dogs, Herlitzka studied but few hearts, and these apparently from dogs of all sizes and weights, and compares results of hearts in apparently all stages of nutrition, age, size and state of injury. He then comes to this conclusion (p. 286):

> 2°. L'aumento della frequenza delle contrazioni cardiache non è una funzione costante della temperatura, ma varia da un cuore all' altro ed anche nello stesso cuore in varie fasi dell'esperimento. A volte si tratto di una funzione lineare; raramente e mai completamente la frequenza seque la legge a cui soggiaciano le comuni reazioni chimiche, gli enzimi, l'accrescimento ecc.

As to what value should be placed upon this author's work and his conclusions, this must be left to the decision of the intelligent reader.

In the paper already referred to the writer has shown from Martin's results that the dog heart obeys the law for chemical reactions as closely as if one were dealing with pure chemicals. Not only when studied statistically, then, but also when the individual heart of the mammal is studied, is this found to be the case. The same relation was shown to hold for the rabbit heart and, in an unexpected way, also for the human heart.

While the thesis seems to be proved beyond doubt the writer will be pardoned if he adds still other evidence.

Baxt[3] studied the effect of temperature upon the dog heart primarily to find out the influence of temperature upon the action of the vagus and accelerens nerves. But in recording his results he always put down the temperature and rate of the heart before stimulation. So it happens we have in his paper considerable data for further study of the temperature coefficient of the 'normal' heart of the dog. Under the conditions of his experiment the temperature of the animals had a tendency to fall off. By surrounding the dogs with a double-wall chamber in which water circulated the temperature could be lowered and increased at will. Dogs of middle weight and in good condition were selected for the experiments. The hearts were left intact in the body, and artificial respiration was maintained, the animals being poisoned with curare. The temperature was varied between 27.15° and 42.8° C.

Wherever a number of observations were made, at about the same time, of temperature and rates of very nearly the same value, the writer determined averages for them. These averages were compared with rates at lower or higher temperatures and their coefficients interpolated by the formula, $10R_1/R_2(t_1 - t_2)$, where R_1 and R_2 are rates at the corresponding higher and lower temperatures, t_1 and t_2. These coefficients are: (1) From pages 339–340, for 'A'—2.18, 2.3, 2.0; for 'B'—2.3, 2.7. (2) From page 341, for 'A'—2.2, 2.6; for 'B'—2.5, 2.0, 4.3, 3.1, 3.6, 4.0. (3) From page 342, for 'A'—2.2, 2.0, 3.1, 2.5, 3.1, 2.6, 2.4, 3.9; for 'B'—1.7, 2.6, 2.7, 2.2, 2.2, 2.8, 2.6, 2.5. (4) From page 356—1.2, 1.8, 2.5, 2.5, 2.0 1.7, 2.4, 2.6.

Of the individual coefficients shown above the lowest is 1.2, the highest 4.3; 71 per cent. of them range between 2 and 3. The mean average is 2.43. Verily, the mammalian heart does follow the law for a chemical reaction velocity as influenced by temperature.

CHARLES D. SNYDER

BERLIN,

[3] Baxt, *Berichte d. K. Sächsischen Gesellschaft der Wissenschaft*, Leipzig, Math. physik. classe, 1875.

THE FLANKING DETRITAL SLOPES OF THE MOUNTAINS OF THE SOUTHWEST

General Aspect and Grade of Detrital Slopes.—The attention of travelers in the southwestern portion of the United States is arrested by the long and regular slopes of gentle grade flanking the rocky ridges of the mountains, and stretching in unbroken lines, often for ten or twenty miles, across the line of vision. Such slopes are most distinctly developed in the Great Basin of Nevada and the semi-desert Piedmont region of Arizona. They give a striking and unique character to the scenery, producing upon the beholder the impression that the wide ocean must have been the agent causing such sweeping outlines and regularly inclined surfaces.

The great table lands, or plateaus, so-called, of the Great Basin region are, in fact, a series of long slopes and not tabular surfaces. This is most notable in the southern portions of the basin, where the general surface is inclined from an altitude of about 4,000 feet downward to the sea-level, and even below that level, as at Death Valley, Nevada. The bordering mountains, and each detached mountain ridge or peak in that region, known as 'Lost Mountains,' are flanked by detrital accumulations from the high rocky ridges.

A fine example of a flanking slope is found on the north side of San Bernardino Mountain, California, extending from the summit of the Cajon Pass to the Mohave River, a distance of 19 miles, with a grade, or ascent, of about 105 feet per mile. It is traversed by the old Spanish Trail and seems like a plain to the traveler when riding in a Pullman car.

In the region of Tucson, Arizona, there are fine examples of long slopes of great regularity flanking the Santa Catalina Mountains, the Santa Ritas and the Sierritas.

The length downward of the Santa Catalina slope on the south side, measured from the 3,500-foot contour down to the Rillito, or to the 2,500-foot contour, is five miles, and the difference of elevation is 1,000 feet, giving a fall of 200 feet to the mile. This slope may be regarded as the upper portion of the former slope, the lower portion having been swept away by the Rillito and Santa Cruz drainage. If, as we may believe, the slope originally extended as far as the Santa Cruz River, or ten miles in length, the average fall would be 100 feet per mile, thus agreeing with the grade at the Sierritas Slope, and at the Santa Rita western slope.

This western flanking slope of the Santa Rita mountains is a good example of the regularity of descent, and of continuity. From the 4,000-foot contour near McCleary's camp down to the 3,000-foot contour above the Santa Cruz river, the distance is eight miles, and the fall is 1,000 feet, or about 125 feet to the mile; but if the lower portion of this slope had not been cut away by the river the length would be ten miles, and the average grade 100 feet to the mile.

The slope of the Sierritas range, southeast of Tucson, is remarkable for its average grade of about 100 feet to the mile for the distance of ten miles. The grade increases nearer the mountains in this as in other examples.

The north side slope of the same range is equally or more remarkable for its extent and even grade.

The valley of the Sonoita from Calabasas eastwards to the summit affords fine examples of the ancient slopes and their deep erosion by the river, leaving terraced banks.

Origin of Detrital Slopes.—The form and origin of detrital slopes, especially those of the southern portion of the Great Basin, are discussed in my report of Explorations in 1853.[1] The broad general molding of the slopes by oceanic action is there recognized as, also, the present modifying action of streams and flood-waters, which are described as spreading out over the lower portions of the slopes 'fan-like' in a multitude of channels, but at lower levels than the upper portions of the slopes which are cut through and left high above the existing stream. Such slopes are in strong contrast with the localized phenomena of modern deltaic detrital deposits at the mouths of mountain gorges or canyons, known as 'alluvial fans' or 'alluvial cones.' While doubtless such delta-like deposits, and the long slopes, have initially the same origin in this respect, at least, that all are made up of débris from the high rocky ridges; the flanking slopes to which I wish to direct attention are less localized, are without the deltaic form, are much more extensive and broader, and are generally without evident relation to any particular canyon or gorge of the mountain, and do not exhibit the scalloped outline of intersection claimed for alluvial fans. They show the operation of a widely extended distributing or leveling agency which it would appear could not have been other than tides, waves

[1] 'Report of a Geological Reconnaissance in California,' 1855, pp. 214–217.

and currents of the ocean during submergence of the land.

But these views of the subaqueous distribution of the detritus have not found favor and are not accepted. The consensus of geologic opinion appears to be that the slopes are wholly subaerial in origin; the result of stream action and distribution alone, without oceanic aid. Their continuity is explained by Gilbert and others, by their contiguity and the coalescence of many adjoining alluvial cones, each with its apex at the mouth of a gorge or a canyon, producing about the mouth of such gorge a symmetric heap of alluvium. Gilbert writes:

> Rarely these cones stand so far apart as to be completely individual and distinct, but usually the parent gorges are so thickly set along the mountain front that the cones are more or less united, and give to the contour of the mountain base a scalloped outline.

This view was accepted by Russell, who quotes it in his paper, in the *Geological Magazine*.[2]

It is accepted also, by Geikie, Dana and others.

In geological literature the slopes are usually described as alluvial fans, alluvial cones, or talus fans. We are indebted for most of these terms to Dr. Drew, who, describing the alluvial and lacustrine deposits and glacial records of the Upper Indus Basin, uses the terms 'fan talus' and 'alluvial fans' for the detrital accumulations at the mouths of canyons. In the illustrations these accumulations of detrital material are represented as local and very steep in inclination, with radii perhaps a mile in length. The 'amalgamation' or union of fans is recognized, and examples are given of the denudation of fan slopes.[3]

Dr. Ida H. Ogilvie, describing the topography of the Ortiz Laccolith in New Mexico, proposes the name 'conoplain' because of the outward slope of the surface in all directions. This sloping[4] surface is regarded as the result of subaerial stream action, under substantially present conditions, and it is suggested that the broad areas of the great plateaus have been produced by a similar process.

Evidences of Submergence.—The great dominant fact showing that the slopes in their original integrity of form were deposited and shaped during submergence, and that they were molded by oceanic currents, rather than by the waters such as now exist, is that the slopes, or the portions to which I refer, are older than the streams. They antedate the streams and the valleys of the existing drainage. The conditions under which the continuous higher slopes were formed are obviously very different from those now existing. The action of the streams of the present is one of redistribution of the material of the slopes. It is destructive of the higher and older slopes, and not upbuilding or constructive except at lower levels of deposition. It is true that the materials of portions of the slope are carried downwards and onwards and are deposited at lower levels, where the volume of water is diminished by wider distribution and by absorption. We thus have what may be called primary and secondary slopes or, if preferable, initial and derived slopes. The secondary slopes, now forming, are in washes and arroyos below the surface of the primary slopes, remnants of which are left on each side sufficient to preserve the grand topographic features and scenic effects far above the reach of modern floods. The higher parts of the flanking slopes do not to-day receive accessions from the canyons, they are not being built higher and are not the product of modern streams. The conditions of deposition were evidently very different from those of to-day. The phenomena all point to the formation of the primary slopes during a period of subsidence before the great Post-Miocene continental uplift. The amplitude of this movement was doubtless greater than is indicated by the height of the initial slopes.

Generally throughout the southwest, the upper margin of the slopes is between the

[2] *Geological Magazine*, N. S., Decade III., Vol. VI., No. VII., July, 1889, p. 290.

[3] Frederic Drew, Esq., LL.D., F.G.S., *Quarterly Journal*, Geological Society of London, 1873, p. 441.

[4] *American Geologist*, Vol. XXXVI., July, 1905.

3,000 and 5,000-foot contour line. The upper margin of the slopes of the mountains of the southern part of the Great Basin has an average height of 3,466 feet, the mean of the height at eight different localities ranging from 3,000 to 4,000 feet.

The Santa Catalina range of mountains north of Tucson affords an excellent example, especially along the south side, where, for a distance of nearly fifteen miles, the slope commences at the base of the cliffs at about the contour line of 3,000 feet. The change of the topography may be followed by the eye. It has the semblance by its horizontality of a beach line or old shore line, but the usual characteristics of an old beach or ocean border, such as shingle and upright sea-wrought cliffs, are wanting, having probably been obliterated by long-continued weathering. The same contour line of 3,500 feet may be followed similarly around the western point of the range to the north side. In other words, the 3,500-foot contour line follows approximately the beginning of the rocky outcrops above and of the slopes below. From this elevation up to 4,000 feet appears to have been the height of the old ocean level for a long time. It can not yet be fixed with precision. Careful search along this general level may result in finding unequivocal evidence of the former level of the sea. In the Huachuca Mountains the upper margin of the slopes is higher, being nearer the 5,000-foot contour than the 3,000-foot.

We do not find along the Catalina range, a distinctly scalloped outline of intersection of the slope with the higher ridges. The slope is very even and regular in its continuity and in the approximate parallelism of the contour lines, broken only by the arroyos or channels of modern drainage, leaving remnants of the former slope on each side. The same observation applies to the slopes bordering the Santa Rita and other ranges north and south of Tucson. These conditions are well illustrated by the contour lines on the map by the U. S. Geological Survey of the Tucson and Santa Rita quadrangles to which reference is made.

Slopes Older than Existing Drainage.—That these ancient detrital slopes are older than the present streams and watercourses is made more evident and in a striking manner by the fact that existing streams and valleys cut across the slopes at right angles to their line of descent, in some instances bisecting the slopes as on the San Pedro River, between its junction with the Gila river and the town of Mammoth. One part of the long slope from the Catalinas is found on one side of the stream, and another portion on the opposite side.

The slope of the southern side of the Santa Catalina, a few miles north of Tucson, is cut across for miles at right angles to its southward descent by the east and west stream, known as the Rillito, which has left an abrupt bank on its north or right bank, a truncation of the slope, while on the south side or left bank we find upon the 'mesa' the coarse detrital deposits characteristic of the 'wash' or slopes of the Catalinas. It is clear that these deposits were once continuous and that they were laid down before the Rillito valley was cut out. Semi-rounded blocks of large size, a foot or more in diameter, of the peculiar augen-gneiss of the Catalina ridges, and horizontal beds of gravel and sand from the mountains are found in digging the wells of the mesa south of the Rillito. Obviously this distinctly detrital slope was formerly unbroken in its extension to the Santa Cruz, and was formed before the Rillito valley, which has been cut out of the slope by the river.

Similar conditions are found farther south where the Santa Cruz River cuts across the lower portion of the slope from the Santa Rita mountains, north and south of Tubac. The cutting out of this valley leaves a steep bluff-like ending of the long regular slope. This also has many deep arroyos parallel to the line of descent, which show the rude stratification of the detrital materials of the slope, portions of which are soft and clay-like, apparently deposited under water. In some sections, the stratification is very regular and horizontal. The portions of the ancient slope left between the arroyos have practically the same angle of descent and the same elevation and are clearly remnants of a once continuous slope or inclined plane.

Antiquity of the Slopes.—In further support of the view of the great geological antiquity or Tertiary age of the initial slopes, reference may be made to the high auriferous gravels of the Sierra Nevada of California, which probably were coincident in formation. Like the ancient detrital slopes of Arizona, they are cut through in all directions by existing rivers. Their great antiquity is undoubted, and is generally regarded as late Tertiary. Russell, in his Quaternary History of the Mono Basin[5] (1889) records his opinion that the excavation of many of the valleys of the Sierra Nevada began long previous to the Quaternary, and are, in fact, relics of a drainage system which antedates the existence of the Sierra as a prominent mountain range.

The detrital slopes of the mountains around the ancient lake Bonneville were found by Gilbert to be older than the lake deposits and to extend below the old shore lines and lighter deposits of the lake. He writes:

> The alluvial cones do not find their bases at the level of upper shore lines, but extend downward continuously to the bottom of the valleys, while the shore-lines are wrapped about them.[6]

So, also, Russell found similar conditions at Lake La Hontan. He uses the term 'alluvial slopes.'[7]

Turner, following Russell, has recognized the early Pleistocene or Sierran age of a portion of the materials filling the Great Valley of California and of portions of the alluvial fans of the Great Basin.[8]

Red Earth Deposits.—Upon the surface of the flanking slopes of the southwest, the earth is sandy and gravelly, and there is a general absence of vegetable mold or soil containing humus.

The presence of a large amount of red earth in many places is a notable fact, especially as it is more or less argillaceous, stratiform, and interstratified with coarse gravelly layers. It is found in quantity in some of the sections of the general slope, exposed by erosion of surface waters. At the northern end of the Santa Catalinas, red earth constitutes a large part of the upper slope at about 4,000 feet altitude. It is there largely in terrace form and is suggestive of the red-clay formation of the ancient Lake Quiberis in the valley of the San Pedro, evidently an ancient estuary, or landlocked valley; a good evidence of former submergence.

Conclusion.—If it is objected to these views of the origin of the broad slopes under water that we do not find strongly marked sea-cliffs and beach-shingle in connection with the slopes, an explanation may be found in a gradual but continuous uplift, so that all accumulations of shingle were leveled off by the retreating water. And in regard to the shore lines, if any were sculptured, the great antiquity of the formation has permitted their effacement.

The absence, so far as known, of any marine remains is readily accounted for by the earthy and gravelly nature of the detritus, its rapid formation and its constant disturbance by the tides and ocean currents, preventing the local development of marine life.

William P. Blake
University of Arizona

[5] Eighth Annual Report, U. S. Geological Survey, p. 350.

[6] 'Contributions to History of Lake Bonneville,' Second Annual Report, U. S. Geological Survey, p. 184.

[7] Israel Cook Russell, 'Geological History of Lake La Hontan.'

[8] 'Origin of the Yosemite Valley,' *California Acad.*, Vol. I., No. 9, p. 269, 1900.

QUOTATIONS

THE PRESIDENCY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY

The difficulties of the Massachusetts Institute of Technology in finding a president are deserving of more than local interest. President Eliot remarked in substance to one of the trustees recently, "You offered the place to a Latin professor and he declined, and now I see that you have offered it to a professor of Greek," an observation suggesting that for a school of theoretical and applied science the institute does not object to going considerably afield in its search for a president. The trustees answer, however, that the great college administrator is the great president, and that it is relatively immaterial which branch of higher learning he may make his specialty.

President Benjamin Ide Wheeler, of the University of California, does not at present intend to accept the position. In the strictest sense he has never been formally offered it, but he did agree, in response to the request of the committee, to come here and talk with the members concerning it. Were he willing to accept the place it would be pressed upon him, an arrangement which amounts to about the same thing as a formal tender. Presidents of state universities are becoming increasingly aware of the advantage that they possess in being freed from the necessity of 'begging,' except, of course, from the single source to which they regularly go for funds. Seeking pecuniary aid among friends of an institution is not an agreeable task. This part of the Boston work is believed to be quite as responsible for Dr. Wheeler's present state of mind as anything else, although he in addition realizes his personal unfamiliarity with a considerable range of the institute's educational interests.

Dr. Pritchett, who resigned the presidency a year and a half ago, and has since been carrying on the leadership of the institute while attending to his other duties, thus shouldering a very heavy responsibility, will definitely terminate his work on his departure for his vacation this summer. It is hardly likely that any serious attempt will be made to secure a president between now and that time, but instead a member of the faculty, or perhaps of the executive committee, will be designated as acting president, to carry on the work until the place can be formally filled. Dartmouth College will be next year in the field looking for a president, too. Increasingly difficult it seems to be in the present complexity of university affairs to get the right men for positions of this character.—The Boston *Transcript*.

CURRENT NOTES ON METEOROLOGY AND CLIMATOLOGY

MEISSNER'S 'METEOROLOGISCHE ELEMENTE'

A recent book by Otto Meissner, published in the 'Sammlung naturwissenschaftlich-paedagogischer Abhandlungen,' deals with the most essential facts of meteorology in a clear and simple way. The full title of the volume is "Die Meteorologische Elemente und ihre Beobachtung, mit Ausblicken auf Witterungskunde und Klimalehre. Unterlagen für Schulgemässe Behandlung sowie zum Selbstunterricht." The publisher is Teubner (Berlin, 1906, 8vo, pp. 94, figs. 33). The object of this new book, as stated in the title, is to promote meteorological instruction in schools and, by treating the subject in an elementary way, to make it possible for the reader to instruct himself. Special emphasis is laid, in the final chapter, on the relations between the meteorological elements and organic life, and the simple rules for weather forecasting are adapted to the use of individual observers who wish to make local forecasts. The origin of important technical terms is indicated in foot-notes. Meissner's little volume is hardly adapted for use in teaching systematic meteorology—it is too disjointed and too superficial for that—but there are numerous suggestions and illustrations which are not found in other books on the same subject. We regret the wholly inadequate statement as to the deflective force of the earth's rotation (p. 39), which gives the impression that north and south winds only are deflected, although later paragraphs state that Ferrel's Law acts in the case of all winds. We note (p. 42) that the anti-trades are stated 'wenigstens teilweise' to descend at the Horse Latitudes, in accordance with Hildebrandsson's view. Undue emphasis is laid (p. 52) on the effect of dust in promoting condensation, no mention being included of the effect of ionization. And it appears (p. 89) as if the effect of deforestation and reforestation were much greater than the best observations lead us to think. Finally we observe one misprint, and a bad one at that. On page 90 Hann is spelt *Hamm*—and that in a meteorological textbook.

'INTERNATIONALER METEOROLOGISCHER KODEX'

An official 'Internationaler Meteorologischer Kodex' has been prepared by Drs. Hellmann and Hildebrandsson (large 8vo, Berlin, Behrend and Co., 1907, pp. 81), in which the resolutions passed at the successive meetings of the International Meteorological Congress and of the International Meteorological Committee are systematically classified by subjects, with references to the meetings at which they were adopted, and to the publications in which the discussions on these resolutions may be found. Hitherto it has been difficult, without a considerable expenditure of time, to ascertain exactly what the official action has been in reference to the numerous subjects which have been considered at these meetings. Dr. Hellmann proposed the preparation of this *Kodex* at the Southport meeting of the International Meteorological Committee in 1903, and the volume has now been published in accordance with the authorization given to Drs. Hellmann and Hildebrandsson by the committee. Resolutions covering matters which have been disposed of (*e. g.,* the international cloud year) are omitted, as are those which have been superseded by later votes. A very full index (subject and author) to the reports of the international meteorological meetings is appended to the *Kodex,* and adds greatly to the value of the work done at these important gatherings. The *Kodex* is not a book for general reading, but it would be well for writers of future text-books on meteorology and climatology to study it carefully, in order to give their readers the official opinion on such matters as the exposure and corrections of instruments; the hours and methods of observation, etc.

R. DeC. Ward

Harvard University

ACADEMIC SALARIES

Mr. Edward M. Shepard, the eminent lawyer and statesman, has addressed the following letter to the editor of the New York *Times:*

The 'Taxpayer' who has made by letter to you an attack upon the purchase of a house to be occupied by the president of the College of the City of New York, would have done better to have ascertained the facts before making anonymous criticism. There is no intention whatever to donate a house to President Finley. The college, for the purposes of the more efficient performance of its work, already great, with its 4,000 students, and fast becoming greater, is to acquire a house close to its buildings to be used by President Finley so long as the college and the city are fortunate enough to enjoy the enormous advantage of his very able and high-minded administration, and, when that good fortune of the college and the city shall end (may the Greek Kalends come first) President Finley's successor will occupy the house.

The city itself acquires the house, without suggestion or hint from President Finley, as an economical method of making some part of a suitable increase to the compensation of the President (now $8,250) and as a means to facilitate the performance of his many and varied duties. Even with the rent of a house, his salary will be less than that of any one of the police magistrates of the city or any one of the civil magistrates of the city; it will be less than three fifths the salary of any one of the supreme court justices. Yet every well-informed citizen knows that, in difficulty of his work and in its supreme importance to the general welfare of the city, President Finley's place is not surpassed by that of any judge, or, indeed, by any in the city, unless, perhaps, the places of the mayor, the controler and one or two others.

The plain fact is, and your correspondent 'Taxpayer' may take notice of it, that, even with the increase in salary which this house will provide President Finley, his salary and the salaries of the professors of the college are inadequate and not in fit proportion to the salaries at Columbia University or the High Schools and public schools. The Normal College recently extended an invitation to a distinguished educator to become its president at a salary $2,000 greater than the salary of President Finley will be when enlarged by the rent of his house. The president and professors receive the same salaries which they had very many years ago. The professors' salaries ($4,750) are to-day what they were thirty or thirty-five years ago; while the salaries of all other teachers and professors and of all others in the service of the city have been largely increased, and living expenses at New York have enormously increased.

I do not know the view of my associate trustees, but I shall ask them to submit to the Board of Estimate next fall a proposal to increase the salaries of the professors of the college, and, perhaps, to still further increase the salary of the president. Nor shall I object to the attendance of 'Taxpayer' before the Board of Estimate to oppose, if he see fit, but there in the open that we may know what interest he represents.

EDWARD M. SHEPARD,
Chairman Board of Trustees, College of the City of New York

NEW YORK,
June 10, 1907

SCIENTIFIC NOTES AND NEWS

PRINCETON UNIVERSITY has conferred its doctorate of laws on President Alexander Humphreys, of the Stevens Institute of Technology, and Dr. Edward G. Janeway, professor of medicine and dean of the University and Bellevue Hospital Medical College.

COLUMBIA UNIVERSITY has conferred the doctorate of laws on Dr. Elmer E. Brown, U. S. commissioner of education, and on Dr. Henry F. Osborn, Da Costa professor of zoology in the university and curator of vertebrate paleontology in the American Museum of Natural History.

AT the recent meeting of the American Medical Association at Atlantic City officers were elected as follows: *President,* Dr. Herbert L. Burrell, Boston; *first vice-president,* Dr. Edwin Walker, Evansville, Ind.; *second vice-president,* Dr. Hiram R. Burton, Lewes, Del.; *third vice-president,* Dr. George W. Crile, Cleveland; *fourth vice-president,* Dr. J. Blair Stewart, Atlantic City; *secretary,* Dr. George H. Simmons, Chicago; *treasurer,* Dr. Frank Billings, Chicago. Drs. T. J. Happel, Trenton, Tennessee, W. W. Grant, Denver, and Philip Marvel were reelected to the board of trustees.

DR. THOMAS E. DAVIS, of Pittsburg, has been elected president of the American Academy of Medicine.

AIDED by another grant from the Hodgkins Fund, held by the Smithsonian Institution, Professor A. Lawrence Rotch, director of the Blue Hill Observatory, will execute at St. Louis a sixth series of experiments with *ballons-sondes* next October, a season when observations at great heights in the free air are lacking in America.

DR. FRANK M. ANDREWS, who has been promoted to an associate professorship of botany at the University of Indiana, has been given leave of absence, and will spend the coming year at the German universities and the Naples Zoological Station.

DR. N. A. COBB, formerly pathologist to the government of New South Wales, Australia, more recently director of the division of pathology and physiology, Sugar Planters' Experiment Station, Honolulu, Hawaii, is now connected with the Bureau of Plant Industry, Department of Agriculture, Washington, D. C.

MR. H. A. BUEHLER, assistant state geologist of Missouri, has resigned to engage in professional work. His resignation will take effect on July 1, 1907. Mr. Buehler's successor has not been selected.

PROFESSOR CHARLES N. GOULD, of Norman, Oklahoma, reports the discovery, in the Arbuckle Mountains, of immense deposits of glass sand. Analyses made in the laboratories of the State University of Oklahoma indicate that the sand is more than 99 per cent. pure silica with no trace of iron. The discovery of this sand so near the extensive gas fields now being developed in that region is a matter of economic as well as of scientific interest.

ASSISTANT PROFESSOR GEORGE A. REISNER, now on leave of absence from Harvard University during archeological investigations in Palestine and Egypt, has been appointed by the Egyptian government archeologist-in-charge of the government excavations which are being commenced in the Nile Valley to the south of Aswan. The work will consist essentially in carrying out the excavations necessary to insure the thorough subterranean examination of that portion of the territory which will be submerged by the Aswan Reservoir when at its full height of 113 meters above sea level.

AT a meeting of the Jackson County Medical Society, held in Jefferson, Ga., on April 10, a monument to the memory of Dr. Crawford W. Long was presented by the society to the city of Jefferson and Jackson County.

The monument is to be erected on the spot where Dr. Long first performed a surgical operation under anesthesia.

Professor Alfred Newton, F.R.S., who held the chair of zoology and comparative anatomy at Cambridge and eminent as an ornithologist, has died at the age of seventy-eight years.

Dr. Edward John Routh, F.R.S., the eminent mathematician of the University of Cambridge, died on June 7, at the age of seventy-six years. Dr. Routh made important contributions to mathematics, but was best known as a coach. From 1861 to 1885, with the single exception of 1883, the senior wrangler each year was one of his pupils, besides twice before that date and once afterwards—in all twenty-seven times. In the thirty-one years of his teaching career, from 1857 to 1888, he coached nearly seven hundred young men through the mathematical tripos, of whom more than five hundred took rank as wranglers.

Dr. Maxwell Tylden Masters, F.R.S., the well-known English botanist, from 1866 to the time of his death editor of *The Gardeners' Chronicle,* died on May 30, at the age of seventy-four years.

We learn from *Nature* that a special meeting of the proprietors of the London Institution, Finsbury Circus, was held on May 8 to consider a proposed scheme of rebuilding, having for its objects "such an increase of revenue as would enable the committee to carry out the objects of the charter on a wider basis than at present, and at the same time to give improved accommodation to the proprietors." The scheme provides for the removal of the present lecture theater and smoking room, thus rendering vacant 10,612 superficial feet of land, to be let on a building lease for eighty or ninety years. The alterations would include a new theater, a storage room for 200,000 volumes, refreshment and other rooms, and the dividing of the present reference library into a reading room, small lecture room and a committee room. The cost is estimated to be about £15,600. Strong criticism of the scheme led to the adjournment of the meeting for four weeks.

The Cardiff public telescope and observatory are proving a decided success. During the last few weeks, in response to an appeal from Mr. Albert Taylor, a large number of teachers in the locality have applied for permission to use the instrument. The attendance of the general public also has been such as quite to warrant the corporation in the expense to which it went in connection with the observatory.

The Electrical World says: It is stated that so unprofitable have the insurance companies found risks on college buildings that there is prospect of a general increase in rates. The entire May issue of *Insurance Engineering* is devoted to an analysis of school and college conditions. From reports of 322 institutions the editor says: "We learn the lesson from the schools and colleges that precautions against fire have been generally neglected." In eighteen years the figures gathered show that 784 fires in college buildings have caused a loss of $10,500,000 and a heavy loss of life. The average money loss has exceeded $13,300.

Last autumn Mr. A. Trevor-Battye made a tour on the Continent, and visited the principal zoological gardens of Holland, Germany and Austria for the purpose of observing the houses, cages and enclosures, and comparing them with those in the Regent's-park Gardens. Recently at the scientific meeting of the Zoological Society, he embodied his results in a paper, which was illustrated by a series of official plans and diagrams. According to the London *Times* two points specially impressed him—the care given to the preparations of plans of a house as part of a general scheme, and the tendency to get rid, so far as it could be done with safety, of bars and wiring. As prominent examples he cited the fine masses of rockwork for wild sheep and goats and the rocky areas for lions and tigers, separated from the spectators by a wide ditch, hidden by greenery, at Carl Hagenbeck's Tierpark at Stellingen. The method of shifting ostriches from the house to the paddocks in use at Hamburg was commended, as was the house for small rodents at Berlin, where the animals are seen burrowing, separated by glass from the visitors. A full description was given of the monkey-house, where the anthropoids are also kept, at Rotterdam, and the details were explained by means of the working plan; and other places selected for praise in the garden were the stores, workshops, and infirmary for sick animals. The monkey-houses at Breslau and Berlin were referred to as showing how the difficulty of access to the open from central cages was got over by a passage the doors in which could be opened by the animals. The fine new block-house for deer at Breslau was mentioned as one of the best in Europe. In conclusion Mr. Trevor-Battye referred to the greater use of glass on the Continent for the protection of the animals, to the supply of water to bears other than the Polar species, and the better arrangements for lighting, and said that when one considered the conditions governing gardens in Great Britain the wonder was, not that they should be surpassed in some points, but that they should be carried on half so well.

A FRIDAY evening lecture at the Royal Institution was given recently by Sir James Crichton Browne, on 'Dexterity and the Bend Sinister.' According to the report in the London *Times* he said that during the last 2,000 years there had been innumerable eruptions of ambidextral enthusiasm, and some five years ago a new crusade on behalf of ambidexterity had been started. He held, however, that on the large scale ambidexterity was impossible and undesirable, that it was by the superior skill of his right hand that man had got himself the victory, and that to try to undo his dextral preeminence was simply to fly in the face of evolution. Right-handedness was a very old story; it was plainly discernible in the art of Greece, Assyria and Egypt, glimpses of it could be found among our ancestors in the Bronze age and in Paleolithic times, and some observers had detected foreshadowings of it even among the lower animals. All nations, tribes and races, civilized and savage, had in all times preferentially used not only one, but the same hand, and it was impossible to point to any civilized race manifesting any degree of either-handedness; the statement that the Japanese were by law and practise ambidextrous, he could say, on the authority of Baron Komura, was without foundation. It was doubtful, indeed, whether, strictly speaking, complete ambidexterity existed in any fully developed and civilized human beings, though sometimes very close approximations to it occurred; but among microcephalic idiots, in whom the small headedness was due to arrested development, left-handedness and ambidexterity had been found to reach a proportion as high as 50 per cent. The source of right-handedness was much deeper than voluntary selection, and must be sought in anatomical configuration—in the structure and organization of the brain that initiated, directed and controlled all voluntary movements. The brain had two hemispheres, of which the right presided over the left side of the body, and the left over the right side, and it was clear that functional differences in the two hands were in some way connected with differences in the two hemispheres—differences not of weight or blood supply, as had been suggested by some inquirers, but of convolutional development. Study of the speech center in the third frontal or Broca's convolution had thrown a flood of light on the subject of right-handedness, for it had shown that damage to this convolution in the left hemisphere deprived the right-handed man of speech, but left the left-handed man with speech unimpaired, while in the left-handed man the contrary held good. Here, then, there was one-sidedness of the brain, assuredly not due to use and wont, or to any acquired habit or mechanical advantage. But the hand and arm centers in the brain were intimately linked with the speech centers, and therefore it was only logical to infer that the preferential use of the right arm and hand in voluntary movements was also due to the leading part taken by the left hemisphere. We could not, he believed, get rid of our right-handedness, try how we might. It was woven in the brain; to change the pattern the tissues must be unravelled. Ambidextral culture, useful enough in respect of some few special movements in some few specially employed persons, must on the large scale tend to confusion; and pushed towards that consummation which its ardent apostles said was so devoutly to be wished for, when the two hands would be able to write on two different subjects at the same time, it must involve the enormous enlargement of our already overgrown lunatic asylums.

UNIVERSITY AND EDUCATIONAL NEWS

The Michigan Legislature on June 13 passed the bill increasing the appropriation for the University of Michigan from a one quarter mill tax to a three eighths mill tax.

The Mackay School of Mines is the name given to the department in the University of Nevada endowed by Mr. Clarence H. Mackay. Mr. Mackay has given money for a building and $120,000 for endowment.

Mrs. John Hay, widow of the former secretary of state, and her sister, Mrs. Samuel Mather, have given Adelbert College, Cleveland, a memorial chapel in the memory of their father, Amasa Stone. Adelbert College was named after Mrs. Hay's brother, Adelbert Stone.

Medical journals state that it is planned to rebuild the medical building of McGill University, which was recently destroyed by fire, on a plot of ground immediately opposite the Royal Victoria Hospital. The ground is owned by Lord Strathcona, who bought it for the purpose of preventing residences being built opposite the hospital, and who is believed to be willing to place it at the disposal of the university. The laboratory, which practically escaped the flames, will remain where it is, but the plot on which the medical building proper stood will be cleared and left for the general beautification of the grounds.

The Carnegie Building and the Walker Chemical Laboratory of the Rensselaer Polytechnic Institute were dedicated on June 12. The addresses were made by Mr. Emil Swensson and Dr. William McMurtrie. The address in connection with the conferring of degrees was made by Dr. W. H. Wiley.

The University of Wisconsin will this year give 476 baccalaureate and 49 higher degrees.

The New York *Medical Record* states that at a recent meeting of the board of supervisors of the Louisiana State University, held in Baton Rouge, the charter and by-laws of the new medical college of the university, to be established in New Orleans, and the contract of assimilation between the university and the medical department were submitted to the supervisors and approved. It is the general understanding that this medical department is to be ready for opening during 1908.

During the past year the students in the department of geology at the University of Michigan have increased from 124 to 281. The regents of the university have met the demand for a larger instructional staff by making the following new appointments: E. C. Case, Ph.D., assistant professor of historical geology and paleontology; I. D. Scott, A.M., instructor in geology; L. P. R. Willoughby and W. E. Bliss, assistants in geology. An appropriation has also been made to equip an earthquake station at the university.

George V. Wendell, associate professor of physics at the Massachusetts Institute of Technology, has been appointed head of the physics department in Stevens Institute of Technology.

Mr. Francis J. Seery has been promoted to an assistant professorship of civil engineering at Cornell University.

Dr. William Salant, of the department of biological chemistry of Columbia University, has accepted an appointment to the position of adjunct professor of physiological chemistry and pharmacology at the University of Alabama.

Mr. H. H. Severin, A.M., of the University of Wisconsin, and Mr. S. Morgulis, A.M., of Columbia University, have been appointed to fellowships in zoology and entomology in the Ohio State University for the year 1907–8.

Mr. A. D. Imms, of Christ's College, Cambridge, has been appointed professor of biology at Allahabad University.

SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, JUNE 28, 1907

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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

AMERICAN UNIVERSITIES[1]

MANY of the great industrial concerns of the present day are said to earn their dividends by means of their by-products. Not that their main work is the less essential, but that the keenness of competition has forced the managers to pay close attention to every source of revenue. If this is the case with industry it may be said with equal truth that the benefits of institutions among men often consist chiefly of their indirect effects; and I want to speak to-night of one great indirect influence for good of American universities. By an indirect effect I mean one which is not a conscious object, or at least not one of the prime conscious objects, of existence. The prime objects of a university may be grouped under four heads: (1) giving a general education to a large number of young people; (2) fitting students by a special training for the practise of a particular occupation or profession; (3) maintaining a body of scholars who add to the sum of human knowledge; and (4) recruiting the men who are to succeed them—for with a really great scholar the problem is not so much to teach him as to discover and stimulate him. Now for the attainment of these four objects various organs of the university have been established.

[1] Address delivered at Yale University on April 19, 1907, being the third of the annual Harvard lectures. The fund for this course of lectures was provided by an anonymous Harvard graduate. The first lecture was given in 1905 by President Eliot, and the second in 1906 by Professor George H. Palmer.

These are the academic department, the professional and graduate schools, and the instructing staff. Not that each organ pursues a single aim, or that the ends desired are always logically, nor perhaps most effectively, promoted by the present subdivision of our institutions, but each organ purports, at least, to lay particular stress on one of these objects.

In addition to these objects our universities and colleges are yearly rendering another great service to the whole country, by helping to create a universal standard of citizenship, to diffuse an American ideal, and it is of this that I wish to speak.

Men whose lives have passed the half-century mark can remember, after the reconstruction of the south was over, loose talk about the next split coming between the east and west; and there were people who really felt that the Atlantic seaboard and the prairie differed so much in material interests and methods of thought that the republic might not improbably be hereafter rent along the line of a meridian. The suggestion would only provoke a smile to-day. The railroads and the growth of manufactures in the west have so blended the interests of the different sections of the country that such an idea has become an absurdity. Nor has the nation become more homogeneous on the material side alone; for the intellectual and moral bonds have been drawn closer also, and in this the universities have had no small share.

But if the sectional specter no longer frightens timid souls, we still suffer acutely from a lack of uniformity of national standards in other directions. Any candid observer of the business methods revealed by the investigations of the New York life-insurance companies, and of the discrimination on the part of the railroads, can hardly fail to perceive that the misconduct which shocked the public was due at least as much to the lack of moral standard as to the deliberate violation of recognized rules of honesty. Two men quarreled publicly over the control of a great insurance company, both of them clearly unconscious that there was anything reprehensible in the methods of administration; but when the management was exposed to view, the community was shocked and a cry was raised for reform. In the same way discriminating freight rates were demanded and given a score or more years ago without a sense that the transaction differed essentially from a reduction made to a large customer by a private manufacturer. Every one, indeed, who has had anything to do with the centers of business life in this country must have been struck with the lack of a fixed code of principles in the management of corporations; and he must have seen both the snare this spreads for the unwary, and the explosion of lack of confidence caused by the uncovering of an extreme instance of a kind of thing that everybody knows is constantly occurring in a milder form. The rules that ought to govern the relation of a director of his corporation in matters of underwriting, in the matter of financial operations with bankers or brokers with which he is connected—many things of this kind are to-day in a state of uncertainty, and the series of business transactions goes by imperceptible degrees from a perfectly honorable act at one end of the scale, to an act at the other end that excites general indignation when it is revealed; and yet there is no point at which a line can be drawn and a warning given 'thus far shalt thou go and no farther.' Often in human affairs it is more important that a line should be drawn than it is at what precise point that line is drawn; for 'where there is no vision the people perish.' The fact is that the new business methods, the new possibilities of combination on a gigantic scale, have made old canons inadequate, and brought the need of a new code of business morality. The nature, and still more the size, of modern transactions have affected the relations of men to one another, and made things wrong that were not wrong before. It is right for each man to trade or work where he pleases, but when a great combination is used to wreck a rival or coerce an industry we are brought face to face with another problem. The boycott and the trust have unsettled many old convictions; and when the accumulation of capital in a corporate form throws into the hands of the managers the power to enrich themselves by imperceptible tributes levied on innumerable stockholders, as in the case of a reorganization or a loan effected for the general benefit of the company, we feel the need of stronger rules fixing the rights and duties of directors. One naturally looks to the courts to lay down the principles of fair dealing in such cases, but while the rapidity with which vast business transactions are negotiated has increased beyond precedent, the machinery of our courts has by no means kept pace. A momentous trial takes longer than ever before, and in affairs of great magnitude, justice, if not uncertain, is at least remote. Thus it happens that the courts have not been fully able to cope with the present industrial conditions, and the law's delay is an impediment to the maintenance of sound business principles in one part of the country, as it is to the prevention of crime in another.

An orgy of political misrule, fostered by the moral lassitude following the strain of the civil war, stirred the national conscience a generation ago. Earnest men, mainly college graduates, spoke and wrote on the subject until something like a standard of political probity was generally recognized. It was not at once carried out. Politicians, especially in municipal affairs, do not live up to it yet. But the standard must come first, its enforcement afterwards, and there can be no doubt that our public life is far purer than it was at the time of the Credit Mobilier, the whiskey frauds and the Tweed ring. The public, indeed, demand to-day a stricter standard in political than in commercial life. They demand of a legislator or an executive officer a more scrupulous avoidance of dealings in which he may have a pecuniary interest, than they do of the director of a corporation; and that is one reason why they regard with complacency a degree of government control of industry that would have been repudiated with disgust a generation ago. One of the crying needs of our time, therefore, is a well-recognized code of commercial honor.

Now, it would be absurd to suppose that universities can manufacture standards of business morality. But they can help to diffuse the fundamental principles on which all morality must rest; and, what is far more important, they can bring men into that intellectual sympathy, that common way of looking at things, which must exist before an agreement on principles of any kind can be reached. Our universities do, in fact, help to set up standards of life and thought, and thus create a national unity of principles in several ways. In the first place, the instructors themselves tend to be more and more alike in their aspirations and methods of work in all our widely scattered universities. The graduate schools, where they study their special subjects, and prepare themselves for their future career, have had a marked effect in this direction. To use a highly inappropriate metaphor, these schools shuffle the pack very thoroughly. One of the pleasantest features, indeed, of the growth of the graduate schools has been the migration of students. A young man from Missouri, for example, who has graduated—let us say —at Oberlin, may come to Yale for a graduate course. He may then get an instructorship at the University of Texas; and if he does well he may be offered an assistant professorship in the University of California; and finally be called to a full professorship at the University of Michigan. Nor is such an imaginary career an exaggeration of what is continually happening. Every considerable graduate school has planted its sons in universities throughout the land; and everywhere that such a man goes he spreads the light from the place he has come from, and gets more light from the men that he meets in his new place. Methods of work, and tone of thought, as well as pure knowledge and ideas, are diffused by such men far better than they can be through books, which contain only the final results of scholarship. The many learned bodies that have adopted the habit of meeting during the Christmas recess, composed almost wholly of professors, are most suggestive illustrations of how homogeneous in tone our universities are from one end of the country to the other. The college faculties form, indeed, one great brotherhood, with common aspirations and tone of thought, a brotherhood that has its ramifications everywhere, and everywhere has charge of the great sources of the intellectual life of the future.

But the work of the universities in bringing all parts of the nation into intellectual harmony is not due solely, or perhaps mainly, to similarity in the corps of instructors. It results also from the fact that the students themselves come from all parts of the land. To take first the purely professional schools: A student coming from wheresoever it may be, who learns his medicine at Johns Hopkins, his engineering at Yale, or his law at Harvard, goes back to his home not only with a knowledge of his profession, but a broader man than he was before; and he never loses the inspiration that comes from study in a really great school. He has a higher ideal, a broader outlook, than he could have acquired in a merely local institution, however good the purely technical teaching might be; and everywhere that he practises in after-life he helps to maintain the principles of his profession, and to advance its higher interests better, because he has studied in a school of national reputation, where students come from every part of the United States. In such a case it is clearly an advantage that the men from different places should be brought into as close contact as possible; and, therefore, I have regretted that the graduates of Yale, who come to the Harvard Law School in considerable numbers every year, should have fallen into the habit of living together in a remote dormitory, instead of being scattered among the other students of the school. Their classmates would benefit greatly by being thrown with them more intimately, and no doubt they would also gain something themselves. From the same point of view the policy on the part of all ambitious universities of establishing schools to fit for every conceivable profession may be open to serious objection, especially in cases where the total demand of the community for members of that profession is small. Are we not sometimes in danger of sacrificing the general welfare to the desire of an institution to be complete in itself?

But, after all, the strictly professional schools—and for that matter the graduate school, which is mainly a professional school for teachers of secondary or university grade—all these schools touch men mainly at one point. The thoughts of the students there are so centered in the subjects they are studying, that while there can be no doubt that they get much good from their fellows in many other ways, nevertheless the broadening influence of the university upon them is brought to bear chiefly through their special line of work.

It is in the undergraduate department—the college, as we like to call it—that the university can best exert its broadening power, can bring men from different places into closer fellowship and thereby give them common ideals and a national type of manhood. So far as college students are serious-minded—and they are so more than they always like to admit themselves—they are thinking not of training themselves to earn a livelihood, but in a more or less conscious way of some portion of the riddle of the universe. Moreover, they come to college at an impressionable age, when their ideas and characters are easily molded into new shapes which are often retained through life. At such a time an acquaintance with men from distant states, and especially a real friendship with one or two of them, will have an effect in slackening prejudices, and creating common ideals, that might come later only after years of experience, if at all. Some time ago one of my students from the far south said that it would doubtless have been pleasanter for him to have gone to a southern college, but he knew that if he did so he would be a southern man, whereas he wanted to be a national man, and therefore had come to us. Before he left Cambridge he became very intimate with a student from the state of Washington, and they are now both practising law in New York. It is needless to say that my friend is a national man, and the kind of man whose presence will be a benefit to New York, or any other place where he may live; to say nothing of the fact that his being in New York will also be a benefit to the southern state from which he came, by helping the people of the north to understand the feelings of the south. That such an object should be deliberately sought and followed is perhaps unusual, and proves rare perception, or maturity, on the part of my friend; but that it tells unconsciously with many men, and that thousands more have got the benefit of the principle without ever having thought of it, is undoubtedly true.

At one time the Italian government was in the habit of sending the young recruit in the army to a garrison town in a distant part of the country. The Piedmontese might go to Calabria, the Neapolitan to Tuscany and the Venetian to Rome; the object being to break down sectional feeling, and bring about a stronger national sentiment and a greater sympathy between the different provinces. The same object on a far higher plane is attained to some extent by our own colleges.

The value to civilization of the great European universities at the close of the middle ages, with their swarms of students from every part of Christendom, can hardly be overestimated. They brought all educated men into a single intellectual fellowship, and prepared the way for the reception of new ideas, and the expansion of thought. It is not easy for us to realize how great a part they played in the life of their time, because thereafter the universities dwindled in size, and only in recent times have they begun again to rival in numbers the crowds that gathered from every nation in Europe at Salerno, at Bologna, and at Paris. Hence one finds it hard to shake off the impression that they are a modern growth. In America the aggregate body of students at institutions of higher learning has been increasing rapidly for many years, not only in actual numbers, but also in proportion to the population. Our universities are not only growing larger, but their influence is extending in wider and wider lines through the body politic. Instead of being merely places where young men of means could enjoy the luxury of a liberal course of study, and where the youth aiming at a professional career could get their training, with such a general education as was thought necessary as a foundation therefor, they aim to-day at diffusing learning directly or indirectly through all strata of society, helping to bring light to any one who wants it. This is especially true of the western universities, which are truly institutions founded by and for the whole community, and which enjoy a marvelous popularity. The opening of a wider door to the influence of the university makes it all the more important that that influence should be exerted in the best possible way, and hence that so far as possible it should not be a local institution, but one which brings together young men from all parts of the country. Looked at from the point of view of the public welfare rather than that of the individual student alone, this is so much one of the chief objects of our colleges that should a general custom arise for every man to attend exclusively the university in his own neighborhood, it would be a great misfortune to education in America. One may, therefore, question whether the part of the sums which the General Education Board is proposing to spend in fostering local institutions all over the country could not be more wisely spent in assisting young people to go to the greater seats of learning. The strongest among them would find larger opportunities of pursuing their studies, and they would all go back with a wider outlook, a better intellectual horizon, than they could get at a small college nearer home.

It may be interesting in this connection to note the geographical distribution of students in some typical American universities. It is difficult to give the figures with perfect precision. In the first place because, as found in the publications of the university itself, they sometimes include the summer school, for example, and sometimes do not; and there are inaccuracies arising from duplicate registration which are not easy to eliminate. Nor is it always perfectly clear what ought properly to be included in comparing the work of the different universities. That the students of a summer school stand on a different footing from those who take the regular course is evident. As a matter of fact, they usually come in the main from no great distance; and hence it would be manifestly misleading in the case of a university whose permanent students were very widely distributed to reduce that degree of distribution by including the figures for the summer school. The same thing is true of the short-term agricultural courses given by many of the western institutions. In comparing different universities, therefore, an attempt has been made to leave out courses of this sort, as well as merely evening schools, while including all the branches of the university where the period of study covers the regular term. But as I have already said, it is difficult to get figures exactly, and those cited here must be looked upon as merely approximate.

Most of the largest American universities have at least one student from almost every state and territory in the Union; but the proportion in which they come from the different parts of the country varies a good deal. Taking Yale, Princeton and Harvard as types of the older institutions, which are large and have flourishing undergraduate departments, we find that the proportion of the students who come from a single state runs from 25 per cent. to 50 per cent., 25 per cent. being the case of Princeton, which, situated in New Jersey, has naturally less students from any one state than Harvard in the populous commonwealth of Massachusetts. In fact, Princeton draws her largest number of recruits from Pennsylvania. Now, these figures mean that in those three universities from one half to three quarters of the students come from states other than the one that supplies the largest contingent.

The proportion of men from outside the state is, indeed, large in almost all the great institutions that are not supported by taxation. Cornell, for example, gets 56 per cent., and Oberlin only 50 per cent., of their students from their own states, in spite of the great size of the states of New York and Ohio. This is true even of some of the smaller colleges of this kind. Amherst and Williams, for instance, receive from a single state only 36 per cent. and 40 per cent. of their students, the former drawing her largest number from Massachusetts, the latter from New York.

I have spoken of all these institutions as not supported by taxation, for among the most valuable of the experiments in education that we are trying in America, is that of two classes of universities side by side, striving for the same ends, doing the same work, but supported and governed on a radically different plan. All the best of the state universities have outgrown the stage when politics hampered their usefulness, and are now very close competitors of the older institutions. The most flourishing of them are in the central and western states, but the line between the two classes being by no means strictly geographical, I shall refer to the institutions that are not maintained and controlled by the state as endowed universities. Not that the state universities are wholly without endowment from private generosity. Some of them have received considerable sums in this way, but as President Pritchett has pointed out in the first bulletin of the Carnegie Foundation, the most vigorous of the state universities have been as a rule the ones that have thrown themselves most completely upon the state and obtained the smallest fraction of their support from private benefaction.

With state and endowed universities running a very close race, with their professors constantly interchanging places, with little advantage on either side except what may flow from the different method of support and government, one approaches with interest a comparison between the two in any field; and not least this question to what extent they draw their students from afar. We have seen that Yale, Princeton, Harvard, Cornell, Oberlin and even Amherst and Williams, receive only from 25 per cent. to 56 per cent. of their students from any one state. Now Michigan appears to be the only state university that attracts any such proportion of her students from outside the state to which she belongs. In her case 54 per cent. of the students come from her own borders; while in the universities of Wisconsin (80 per cent.), Minnesota (89 per cent.), Illinois (83 per cent.), Missouri (78 per cent.), Kansas (91 per cent.) and California (91 per cent.), the proportion that comes from the state itself runs from 78 per cent. to 91 per cent.; the average for these six institutions being 85 per cent. The figures are certainly significant. So far as they go they seem to show that in the endowed universities, or at any rate in several of the principal endowed universities, less than one half of the students come from any one state, but that in the state universities four fifths, or more, commonly come from the state itself.

We should, no doubt, reach a similar result if instead of inquiring how many students came from a single state, we neglected political divisions altogether, and found how many came from a geographical area or zone within a given distance from each university; but to do so would naturally involve a very elaborate investigation. The same object may, however, be roughly attained by taking the percentage of students who come from the six states most largely represented in the university. As a result of this computation we find that Yale draws 24 per cent. of her students from parts of the United States outside of the six states that furnish her largest contingent of recruits; and in fact of all American universities Yale is the one whose students, in proportion to their number, are most widely distributed over the nation. Princeton draws outside of her six leading states 22 per cent.; Harvard 21.5 per cent.; Oberlin 19 per cent., and Cornell 16 per cent.; while both Amherst and Williams, in spite of their small size, draw 18 per cent. Michigan, the state university whose students are by far the most widely distributed, draws 14 per cent. of her students from parts of the union lying beyond the six states that give her most recruits. The University of Missouri, with her wide southern clientele, draws 12 per cent.; Illinois, 8 per cent.; Wisconsin, 4 per cent.; California, less than 3 per cent., and Minnesota and Kansas, only 1 per cent. each.

These figures make no pretense to be a statistical abstract of the distribution of students in our universities. They are taken from a few typical institutions alone, and, as I have already pointed out, they are not perfectly accurate. Still they are trustworthy enough to form a basis for some conclusions, not indeed very new or startling, but by no means without interest. They show that on the whole the endowed universities draw their students from a much greater area than the state universities, or rather draw a larger fraction of their students from all over the country, for, as we have seen, almost every great university draws at least one student from almost every state and territory.

Such a difference is important in view of the large size and high degree of excellence which the state universities have attained. No doubt it is in part due to the greater age of most of the endowed institutions, for the graduate of a college planted in a remote region becomes a recruiting agent for his alma mater. He advises the young fellows that he meets to go to the place where he received his own education, and in due time he sends his sons there. If he achieves success, that also works as a perpetual advertisement of his college. This suggestion of the cause of wide distribution of students is to some extent confirmed by the fact that the University of Michigan, which is the oldest of the state universities referred to, and far the oldest of them in reputation, draws her students from all parts of the country much more than any of the others. But the University of Wisconsin, founded only twelve years later, draws 80 per cent. of her students from within the state and only 4 per cent. outside the six states that give her the largest support. Moreover, if we compare Cornell with the universities of Illinois and Minnesota, which were founded at almost the same time and are about the same size, we find that Cornell draws only 56 per cent. of her students from the state of New York, while the Universities of Illinois and Minnesota draw 83 and 89 per cent., of their students from their respective states; and that from the parts of the union outside of the six principal states Cornell draws 16 per cent. of her students, while the two state universities draw only 8 per cent. and 1 per cent.

The apparent difference in the area from which students are drawn may also be due in part to the fact that all the state universities are coeducational, while the great colleges on the Atlantic seaboard which have been cited are not; and it is undoubtedly true that parents are more reluctant to send their daughters than their sons to a distant place for their education. But this motive does not apply to Cornell and Oberlin, which have admitted women freely from the start. It may, indeed, be urged that these two institutions stand in a somewhat peculiar position, and a position that has tended in the past to extend their renown and their influence over the nation.

Each of them was in her own way a pioneer in education. Each of them stood for an idea, and was in her early years radical, aggressive and militant, a condition that tends to attract persons of a like way of thinking wherever they may live. It is, of course, impossible to estimate the effect of conditions of this kind, or to allow for them in making a computation of results. Nor is it easy to select endowed and state universities whose circumstances have been so completely identical as to make comparison absolutely fair.

Perhaps the best example to be found is that of the two great universities on the Pacific slope—Leland Stanford, Jr., and the state university in California—and a comparison of these two is instructive. The University of California has been in existence more than twice as long as Leland Stanford, and is nearly twice as large; so that she ought to have an advantage on the score of both age and size, and both institutions virtually admit citizens of the state free, and charge fees of $20 a year to non-residents. Yet the University of California draws over 90 per cent. of her students from the state alone, while Leland Stanford draws 62 per cent. from that state. The students at the University of California came, according to the catalogue of 1901–2 from which these statistics are compiled, from only 29 states all told, while to Leland Stanford they come from 42. Moreover, less than 3 per cent. of the students at the former came from parts of the country outside of its six chief tributary states, as against 13 per cent. in the case of Leland Stanford. Some slight allowance must, no doubt, be made for the fact that Leland Stanford limits the number of women to 500, while at the University of California they form a decidedly larger proportion of all the students. But this is a trifling matter that could hardly affect the result seriously.

If all this array of figures does not warrant any precise quantitative comparison of the distribution of students in the different kinds of institutions, it is surely definite enough to prove that, so far as drawing from a wider area is concerned, the endowed universities are doing a more fully national work than those which are supported by state governments; and it suggests, at least, a probability that in the future they will continue to do so. There are a number of reasons why this should be the case. About half of the state universities now make some distinction in fees between citizens of the state and other people; and with the continual increase in the cost of instruction the practise is likely to grow rather than decline, for it seems unreasonable that taxpayers should be burdened to provide education for outsiders. They have been certainly extremely liberal hitherto, and a young man can go to-day more cheaply to a state university in another state that charges a differential fee, than to one of the eastern colleges.

Then there is the matter of state pride. If a boy intends to go to any state university there is a motive of local patriotism for going to his own. Is not his own as good? Is it not established, paid for and conducted, for just such as he? Is it not striving to swell its numbers? Finally, there is the question of the allegiance to his college of the graduate living in a distant state, the affection with which he looks back to it, and the eagerness with which he sings its praises. This sentiment is said to be less strong in the case of the state, than of the endowed, universities. Such a difference, if it exists, may be due to the fact that a state university, like a high school, is taken as a matter of course, as one of the regular organs of the government, and not as an institution in which a member has acquired a privileged proprietary right. Something may be due to the more common custom of endowed universities of lodging their undergraduates in dormitories, thus giving them a real community life; and something to the practise, common, though not universal, of allowing the alumni a voice in the selection of the governing board. If such a difference in sentiment exists, whatever the cause may be, it will exert a potent influence in favor of recruiting the students of endowed universities from a wider area. But the state universities have already astonished and delighted the world too much to make safe any predictions about what they will or will not achieve in the future.

It would appear, however, that bringing young Americans together for a common education from every section of the country is at this present day preeminently the problem of the endowed universities, and especially of the larger ones; for, while some of the smaller colleges draw their students from a wide area, the larger institutions are peculiarly fitted to work on a national scale. Their very size means a wider constituency, and hence a more complete mingling of young men from all over the country. They are best adapted for the great function of helping to form a national type of manhood, because they have a better chance of drawing students in large numbers from every part of the land. But if size gives opportunities, it involves also difficulties. In a small college the individual is in less danger of being lost; the young man without aggressive personality is less likely to be ignored or submerged. Character and self-reliance are more developed by being a man of mark in Ravenna than by belonging to the mob in Rome; and what is more to our purpose, a body that is too large for general personal acquaintance tends to break up into groups whose members see little of one another. The citizen of a good-sized town has usually a wider acquaintance than the dweller in a big city.

In the social life of a college, as in other things, there is for any one form of organization an economic scale which gives the best results. Beyond that the social body becomes fissiparous, and thereby loses the benefits of size. What is worse, the lines of cleavage naturally follow the associations formed before coming to college; and hence a man from a distance, who has no friends already there, may well fail to become intimate with the men whom it is most important that he should know both for their sake and his own. In many places the social life of the students is regulated by fraternities, to some one of which almost every undergraduate belongs; and such a system may work well enough in a small college, or in one where the students come from a limited area, so that every one has a chance of being known. But in the large endowed universities that system, or any system of societies or clubs, is incapable of supplying an opportunity for the best kind of social life to the great mass of students. Nor if it could include them all would it be a fortunate arrangement, because here again we should be met by the tendency to divide on the lines of previous association, and one of the chief advantages of the great university, that of throwing together men from every part of the country, would be in great part lost.

Now, the larger colleges grow, the more pronounced this difficulty must inevitably become. In the largest of them it is already felt; in others it can be foreseen; and before many years have passed it will present a very pressing problem. With the rapid growth of the number of people who can afford to send their sons to college, with the ever greater need of education as a prerequisite to getting a good start in life, and with the tendency to require a college degree before beginning the study of a profession, there is every reason to believe that the total number of college students in the United States will increase very fast. If, therefore, the undergraduate departments of the larger endowed universities maintain their hold upon public confidence, it seems not unreasonable to suppose that in the space of a generation they may triple or quadruple in size; and before that happens the question of student life must be solved, or will have solved itself, for better or worse.

The problem is so to organize the students as to mix together on an intimate footing men of all kinds from all parts of the country. The obvious solution is to break the undergraduate body into groups like the English colleges, large enough to give each man a chance to associate closely with a considerable number of his fellows, and not so large as to cause a division into exclusive cliques. It must be understood, of course, that this applies only to the social life, not to the instruction, which would remain a university matter as heretofore. Such a suggestion of breaking up the student body has often been made, and something of the kind must be done sooner or later if we are to maintain our old ideas of the value of college life. Incidentally, it would have the effect of provoking internal emulation which we sorely need. The socialistic, or for this purpose it is more appropriate to say the Christian, spirit that has come over the world has affected profoundly our undergraduates. Of late years an appeal to purely individual objects has less effect upon them than it did formerly. A student likes to feel that he is striving not for his own selfish fame alone, but for the glory of the organization to which he belongs, and hence a rivalry between a number of colleges would add a powerful incentive to effort in many lines.

But it is not enough to suggest that the undergraduate body can advantageously be divided into groups; the difficulty comes in arranging how the groups can be formed; and here we get very little light from European experience. The German universities, and those that have followed their model, are collections of professional schools training men to be clergymen, lawyers, physicians and teachers or professors. They have nothing corresponding to the liberal culture at which our college purports to aim. That phase of education is supposed to be completed at the gymnasium. In England, on the other hand, where the universities have developed the ancient traditions in a very different way, the social conditions are such as to preclude the chief difficulty that confronts us. Oxford and Cambridge are doing a work of the same nature as our undergraduate departments, and they are made up of colleges such as I am now discussing; but the bulk of their students are drawn from a single class in the community. Men of a different class who want that kind of education usually go to London, or to one of the provincial universities. This fact, together with the inducement of scholarships, and the tendency to be guided by inherited associations, causes the students to distribute themselves among the colleges in a very satisfactory way. The men who have grown up together as boys, or who come from the same region, do not collect unduly in one college. In America we should have quite a different result if we allowed the boy to select his group on first coming to the university. He would almost certainly go to the group or college where the men were that he knew already, or at least to a place where he would not feel too much of a stranger. The students would be mainly segregated on the basis of origin, of geographical sections, of preparatory schools, of home surroundings; and thus we should have—as people have said—a college for western men, a college for southern men, a college for millionaires. Now this is the very worst scheme of divison that could possibly be devised. It would accentuate and intensify the unfortunate lines of cleavage in the student body that are now beginning to appear. It would stereotype and perpetuate them. It would erect barriers, to prevent a student from associating readily with the very men that he ought to be thrown with. What we need, on the contrary, is a system of grouping that will bring into each group men from different parts of the country, men with different experience, and as far as possible social condition. In short, what we want is a group of colleges each of which will be national and democratic, a microcosm of the whole university. This may not be an easy feat to accomplish, but I believe it can be done. Perhaps the freshman year, which is in any case a period of transition, could be advantageously used as a time for mixing the students together, and bringing out their natural sympathies and affinities before they make their final selection of a college. But whether this solution be adopted or not, the problem is one that is, or shortly will be, common to the leading endowed universities in the eastern states, and they must all solve it sooner or later in some way if they are to maintain their undergraduate departments, and make them of the highest value to the nation.

A. Lawrence Lowell

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THE ANNUAL MEETING OF THE AMERICAN ASSOCIATION OF MUSEUMS

The second annual meeting of the American Association of Museums was held at the Carnegie Institute in the city of Pittsburg June 4–6, in pursuance of an invitation which had been extended to the association in May, 1906, by the trustees of that institution. The local committee of arrangements consisted of the trustees of the Carnegie Institute and a number of distinguished gentlemen representing various educational and commercial organizations in the city of Pittsburg, including the chancellor of the Western University of Pennsylvania, the president of the Chamber of Commerce, the president of the Pittsburg Stock Exchange and the presidents of the Academy of Science and Art of Pittsburg and the Botanical Society of Western Pennsylvania and the heads of various schools and colleges. An executive committee, presided over by Hon. James Macfarlane with Mr. C. C. Mellor as secretary, attended to all details.

Although the time fixed for the meeting unfortunately coincided with the commencement season in many institutions, and many members of the association were also compelled to be absent because already the work of exploration in distant regions had been begun by the museums which they represented, there were over sixty members of the association present, and almost all of the leading museums and art galleries of America were represented by one or more delegates.

The council convened in advance of the meeting of the association at the Hotel Schenley on the evening of June 3, and after dining, as the guests of Dr. Holland, the director of the Carnegie Museum, transacted the routine business which came before them in connection with the coming meeting.

The sessions were held in the lecture hall of science in the institute. President Hermon C. Bumpus, the director of the American Museum of Natural History, presided until the morning of Thursday, when the chair was taken by Dr. W. J. Holland, the second vice-president. Dr. George A. Dorsey was the secretary.

At the opening on the first day, June 4, at 10 A.M., an address of welcome was delivered by J. Rodgers McCreery, Esq., who took the place of his honor George W. Guthrie, Esq., the mayor of the city, whose business engagements prevented him from being present.

In the absence of W. N. Frew, Esq., the president of the board of trustees of the Carnegie Institute, who was prevented from being present by illness, Dr. Holland extended a cordial welcome to the association on behalf of the institute.

Dr. Hermon C. Bumpus gracefully responded in well-chosen words.

The remainder of the forenoon of June 4 was taken up with the transaction of routine business, the hearing of the reports of committees and the reading of a paper by Mr. Henry L. Ward, the director of the Milwaukee Public Museum, upon 'Museum Labels,' which led to a very interesting discussion. At 12:30 the association adjourned for luncheon, which was provided in the restaurant of the institute by the trustees.

From 1:30 until 2:30 the time was occupied in a tour of inspection of the new buildings of the institute, the various gentlemen on the entertainment committee and on the staff of the Carnegie Library, Art Gallery, and Museum acting as guides.

At 2:30 the reading of papers was resumed. Mr. Frank Collins Baker read a paper entitled 'Some Instructive Methods of Bird Installation,' which was illustrated by photographs. He was followed by Professor E. S. Morse, director of the Peabody Institute of Science, Salem, Mass., who presented a paper on 'A New Method of Mounting Ethnological Objects.' Dr. W. M. R. French, director of the Art Institute of Chicago, next addressed the association on 'The Advantages of Installation in Swinging Frames for the Exhibition of Textiles, Photographs, Prints and other Flat Objects.' A paper on 'The Exhibition of Large Groups in Museums,' by Professor Henry L. Ward, of Milwaukee, followed. Mr. P. M. Rea, director of the Charleston Museum, presented a paper on 'Museum Records.' All of these papers, which were admirably conceived and illustrated, led to pleasant and interesting discussions.

The association adjourned at 4:30 P.M., when the members were tendered an excursion by automobile through the parks and residential portions of the city of Pittsburg, a number of prominent gentlemen having kindly placed their automobiles at the service of the committee of entertainment.

At 8 o'clock the association reconvened. A symposium upon 'The Evolution and Aims of Museums of Art and Science' was opened by a paper by Mr. F. A. Lucas, curator-in-chief of the Brooklyn Institute of Arts and Sciences. He was followed by Mr. Benjamin Ives Gilman, the director of the Boston Museum of Fine Arts, who presented a scholarly paper on 'The Aims of Museums.' After the reading of these opening papers a general discussion in which all of those who were present were invited to participate took place.

Dr. George A. Dorsey, of the Field Museum; Dr. Henry L. Ward, director of the Milwaukee Public Museum; Professor E. S. Morse and many others spoke interestingly.

On June 5, at the opening session, a general discussion on 'The Present Jury System in Connection with Exhibitions of Contemporary Art' took place. The discussion was opened by Dr. W. M. R. French, director of the Art Institute of Chicago, who was followed by Dr. Charles M. Kurtz, director of the Albright Art Gallery of the Buffalo Fine Arts Academy. Mr. John W. Beatty, director of the department of fine arts of the Carnegie Institute; Mr. John Caldwell, who for ten years was chairman of the International Jury at the Carnegie Institute, and others spoke on the theme. Considerable diversity of opinion was expressed, and the debate, while lively, was thoroughly good-natured.

Following this discussion an interesting paper was presented by Mr. T. L. Comparette, the curator of the cabinet of coins in the United States Mint, Philadelphia, Pa., on 'The Installation and Exhibition of Coins.' Dr. Comparette was followed in his remarks by Dr. Kurtz and Dr. Holland.

At 11:30 the association adjourned and repaired to 'The Meadows,' the country seat of Mr. Robert C. Hall, at Ross Station, Pa., where they were treated to a barbecued ox with savory accompaniments. A brief session was held, at which the question of museum support was discussed and papers were read by Dr. W J McGee, director of the St. Louis Public Museum; Dr. George A. Dorsey and others. On the return, which took place at five o'clock, a visit was paid to the East Liberty branch of the Carnegie Library, under the guidance of Mr. Anderson H. Hopkins.

In view of the large number of papers to be read invitations to visit the Homestead Steel Works, the Allegheny Observatory and other interesting points were declined and the association resumed its sessions at 8 P.M. in the Carnegie Institute.

Election of officers for the ensuing year took place, resulting in the election of Dr. W. M. R. French (first vice-president) as president; of Dr. W. J. Holland (second vice-president) as first vice-president, and of Mr. F. A. Lucas as second vice-president. Mr. P. M. Rea, of Charleston, S. C., was elected secretary; Dr. W. P. Wilson, of Philadelphia, was reelected as treasurer; Dr Hermon C. Bumpus and Dr. George A. Dorsey were elected councilors for three years.

A very interesting paper, illustrated by photographs, was read by Miss Delia I. Griffin, director of the Fairbanks Museum of Natural History, St. Johnsbury, Vermont. Her theme was 'The Educational Work of a Small Museum.' She was followed by Miss Anna B. Gallup, curator of the Children's Museum of the Brooklyn Institute, who gave an interesting account of the work of the Children's Museum, illustrated with lantern slides.

On Thursday morning at nine o'clock the reading of papers was resumed. Dr. W. P. Wilson, of the Philadelphia Museums, gave a most interesting account of the work along educational lines of the great institution over which he presides, illustrated by examples of different collections which are prepared for the purpose of being loaned to the schools of the city of Philadelphia. He was followed by Mr. A. R. Crook, who presented a paper on 'The History of the Illinois State Museum of Natural History.' A paper by Mr. Edward K. Putnam, on 'The Educational Work of the Museum of the Davenport Academy of Sciences,' was read by title; a paper by Dr. Charles H. Hitchcock, curator of the Butterfield Museum of Dartmouth College, Hanover, N. H., on 'Ichnological Researches in Museums,' was likewise read by title in the absence of both authors. A scholarly paper, on 'Some of the Advantages of an Ecological Organization of a Natural History Museum,' was presented by Professor C. C. Adams, curator of the museum of the University of Cincinnati.

The association adjourned for luncheon at the Pittsburg Golf Club, where an excellent luncheon for all who were present had been provided.

The day being beautiful and sunny and

the skies clear, the stroll through the park to the golf club was greatly enjoyed by the members, many of whom visited the Phipps Conservatory on their way.

In connection with the postprandial exercises resolutions of thanks to the officers and trustees of the Carnegie Institute and the various generous citizens of Pittsburg who had provided for the comfort of the association were passed, and the association finally adjourned to meet in Chicago on May 5 as the guests of the Art Institute of Chicago, the Field Museum of Natural History and the Chicago Academy of Sciences, which united in tendering a joint invitation to the association to hold its next meeting in Chicago.

A committee on the publication of the 'Proceedings' of the meeting, consisting of Dr. W. J. Holland, Dr. George A. Dorsey and Professor P. M. Rea, was appointed. A full account of the meeting, together with the papers read will be issued.

The meeting is declared by all who were present to have been most enjoyable, and the American Association of Museums is undoubtedly firmly established as one of the national societies which is destined in coming years to exert a very beneficial influence upon the wide field of activities which is represented by the museums of art and of science in America.

SCIENTIFIC BOOKS

Clays; Their Occurrence, Properties, and Uses, with especial reference to those of the United States. By Heinrich Ries, Ph.D., Assistant Professor of Economic Geology in Cornell University. Pages xvi and 490. New York: John Wiley & Sons; London: Chapman & Hall, Limited. 1906.

This comprehensive and well-balanced treatise on clays devotes the first five chapters, 276 pages, to the origin, the chemical and physical properties and kinds of clays, and to the methods of mining and manufacture. Chapters VI. and VII., 183 pages, describe the distribution of clays in the United States. The last chapter, of seven pages, is devoted to an account of the fullers' earth, including its properties, methods of mining and uses, and distribution in the United States.

In the special state descriptions the subject matter is set forth in the order of the geological formations as permitting the greatest uniformity of treatment, and as involving the least amount of repetition. In those states with which the present reviewer is most familiar, the descriptions are adequate and well presented, though necessarily in a concise manner.

For this part of the work the author is peculiarly well prepared from first-hand acquaintance, since he has personally examined and written reports on the clays for the geological surveys of a number of states, as well as for the United States Geological Survey. In these reports will naturally be found much of the matter of a general nature of the present treatise, since a general discussion of the origin, properties, and varieties of clays would be an appropriate and desirable introduction to the detailed description of their occurrences in the state reports, yet it must not be inferred that the general or introductory part of the volume before us is a mere compilation and repetition of the material already published in the several state reports by the author. As a matter of fact there is much in the introductory chapters and in the illustrations that has not appeared in any of these reports.

The clay resources of the different states are not equally well known, hence some inequality in the state descriptions, through no fault, however, of the author, because some of the important clay states, like Illinois for instance, have very little literature bearing on their clays.

In discussing the origin of clays the author emphasizes the facts that in the decomposition of feldspar, CO_2 is not an essential factor, since the mineral is decomposed by water alone; and that other feldspars besides orthoclase yield kaolinite. He also opposes the recently advanced view of Rösler that the kaolinization of feldspar is never due to atmospheric action, but always to post-volcanic pneumatolytic and pneumato-hydatogenic processes.

> The very fact that many of our kaolins pass into undecomposed feldspar or feldspathic rock when the limit of weathering is reached, shows the incorrectness of such a broad statement (p. 7).

The discussion of the origin of clays is followed by a fairly complete description of the secondary changes both mechanical and chemical, in clay deposits, a feature often omitted in similar works.

In most treatises the statement is made that kaolinite is the basis of *all* clays, which are thus to be regarded as mixtures of kaolinite with other minerals. The incorrectness of this statement, the author thinks, will be made evident by an examination of any series of kaolin analyses, which will show that the alumina-silica ratio is often higher than that required for kaolinite, and this seems best accounted for on the supposition that some of the other clay-like minerals, such as pholerite or halloysite, are present. The fact is also pointed out that neither the chemical nor the rational analysis can in every case be relied upon to give certain information concerning the mineral composition and other characters of a clay. Thus, a washed kaolin might have as much as twenty per cent. of white mica, and yet on analysis show a composition approaching rather closely to that of kaolinite. And, moreover, kaolinite, pholerite, halloysite, and muscovite are all decomposed by hot sulphuric acid, and in a rational analysis would be reported as clay substance. This is unfortunate, since mica is not refractory and should not, therefore, be grouped with the other three (p. 167).

In Chapter II. the various minerals occurring in clay and their influence on its behavior, especially as regards shrinkage and fusibility, are quite fully treated. It has long been known that titanic oxide is of universal occurrence in clays, though seldom shown in analyses. Experiments of the author recently carried out with mixtures of kaolinite and titanic oxide up to 5 per cent., prove that even small amounts of this substance lower the refractoriness of clays, one half per cent. bringing the fusing point down half a cone. The importance of determining the titanium in a refractory clay is thus made clear.

Of the physical properties of clays, plasticity is in many respects the most important, and perhaps the most difficult to explain. The author considers the conception of plasticity as expressed by many as too narrow. He remarks:

> A broader conception, and probably a more satisfactory one to the physicist, would be to define plasticity as the property which many bodies possess of changing form under pressure, without rupturing, which form they retain when the pressure ceases, it being understood that the amount of pressure required, and the degree of deformation possible, will vary with the material (p. 94).

This definition would include many bodies besides clay, which are excluded by the narrower definitions.

The various theories put forward to explain the cause of plasticity are passed in review, with the conclusion that plasticity is not dependent upon any single cause advocated by these theories, but by a combination of them. The relations of texture to tensile strength are illustrated by experiments of Orton, Beyer and Williams, and the author, which go to show that in those clays having the highest tensile strength, the percentages of fine, medium, and coarse particles are nearly equal, and that an excess of either coarse or fine grains lowers the tensile strength.

In Chapter IV. the various kinds of clays, in the order of the purposes for which they are used, and beginning with the highest grades, are described as to their chemical and physical properties. These characters are further illustrated by typical physical tests and chemical analyses. The latter part of this chapter, devoted to descriptions of methods of mining and manufacture, might by some be criticized for lack of sufficient detail, but the book is not intended to be a treatise on the manufacture of clay products alone, and too much technological detail would be clearly out of place.

The illustrations are well selected, not for ornamental purposes, but for the real illustration of the text. The book is well printed and bound and with no undue share of typographical errors.

While the different topics discussed in this work have been treated in more or less detail in official reports and in special articles, Dr. Ries's book will be welcomed by all interested in the subject of clays, as being certainly the most comprehensive and evenly balanced, if not the only, presentation of the subject as a whole that we have. And though written primarily for American geologists, chemists, and engineers, the introductory part, being of a general nature, should be equally useful to men of all nationalities.

EUGENE A. SMITH
UNIVERSITY OF ALABAMA

Biochemie der Pflanzen. Bd. II. By FRIEDRICH CZAPEK, Ph.D., M.D. Gustav Fischer in Jena. 1905. Pp. xii + 1027.

The second volume of this important work on the chemistry of plants has fully sustained the high expectations excited by the first volume. In something over a thousand pages, the author brings his account down to the state of our knowledge as it existed in June, 1905. It is impossible in the space available for this purpose to give more than a most meager outline of the contents of this volume of this truly great work. A general discussion of the biochemistry of plant albuminoids is followed by a treatment in some detail of the phenomena connected with this class of bodies as seen in the physiological processes of various groups of plants from bacteria to phanerogams, and as seen in the various organs and structures of these plants.

The second large division deals with the nitrogen-containing end products of plant metabolism. The discussion is one of rare interest, especially as dealing with the chemical physiology of hydrocyanic acid and with the plant alkaloids. We have had chemical discussions and botanical discussions on these subjects, but the author has here succeeded in making the facts of either category illuminate those of the other, an observation that applies to a remarkable degree to all parts of the book.

The chapters on the physiology and chemistry of the relation of plants and plant products to oxygen is succeeded by a treatment of the part played by ash constituents in plant metabolism in its widest relations. A chapter of unusual interest on the chemical aspects of plant irritability concludes the body of the work.

It would be hard to speak in too high praise of this work. It comes into a place in botanical literature that has never been filled heretofore, and as the drift of plant study in recent years has been strongly in this direction, the need of such a work has been more and more keenly felt. This book will go on to the same shelf of indispensables on which Pfeffer's 'Physiology,' Goebel's 'Organography' and Haberlandt's 'Anatomy' are to be found.

It is to be strongly hoped that the author may find opportunity from time to time to revise the work as the progress of science makes necessary, and thus provide investigators with a ready means of keeping in close touch with the progress of physiology. The author gives evidence of a desire to do this by providing in an appendix references to literature appearing after the body of the work was completed.

This book should be translated into English, and that at an early date.

R. H. TRUE

SCIENTIFIC JOURNALS AND ARTICLES

The American Naturalist for May opens with an article by Herbert W. Rand on 'The Functions of the Spiracle of the Skate,' the conclusion being that it serves chiefly as an in-take for the respiratory stream, and that the reversal of the stream, or spouting, may serve to clear out the gill chambers and be analogous to taking a deep breath. F. H. Pike presents 'A Critical and Statistical Study of the Determination of Sex, Particularly in Human Offspring.' Among the conclusions are that in man there is a slight excess in the number of male offspring; that sex determination probably occurs before the fertilization of the ovum and that sex is hereditary. Alfred W. G. Wilson gives some interesting accounts of 'Chubs Nests,' probably made by *Semotilus corporalis*. These 'nests' are small mounds of pebbles and sometimes reach a diameter of five or six feet and a height of 14 to 24 inches. In correspondence Dr. Jordan alludes to the flying-fish problem, stating his opinion that the fins are *not* moved. But when a flying-fish is laid on a vessel's deck the fins are flapped vigorously, and why not in the air?

The Museums Journal of Great Britain for April notes that the eighteenth meeting will be held in Dundee. F. A. Bather discusses 'Interchangeability in Cases,' with special reference to those in the geological department of the British Museum. It is soothing to find that Dr. Bather has met the ever-occurring irritating facts that the cabinet-maker shows a diabolical ingenuity in frustrating the work of the designer, and that no carpenter with any proper pride will make a drawer or a shelf that will run freely. The interchangeability of large cases is rather a difficult matter, but was to a great extent applied by Dr. Goode in the U. S. National Museum. Dr. O. Lehmann describes some interesting features in 'The Altona Museum Exhibit at Dresden, 1906,' wherein he so arranged the specimens as to give the visitor the idea that nature works in much the same manner as the artist and that the form is the shortest artistic expression of the whole life of the animal. From this and previous articles it is evident that Dr. Lehmann has expended much thought in making his museum attractive and instructive to the ordinary visitor.

SOCIETIES AND ACADEMIES

THE NEW YORK ACADEMY OF SCIENCES—SECTION OF ASTRONOMY, PHYSICS AND CHEMISTRY

A joint meeting with the Physics Club of New York City was held at the American Museum of Natural History on Friday evening, March 22. The following demonstrations were given:

F. J. Arnold: Finding the weight of an irregular body by means of its center of gravity. An irregular body consisting of a 50 cm. wooden rod with a bar of metal attached at one end to give the combination a low center of gravity. Location of center of gravity, fulcrum and known force marked on paper strip fastened to bar and lever arms measured directly by means of these points.

R. H. Cornish: (1) Method of projection on screen of lines of force surrounding a conductor carrying a current. (2) Mechanical illustration of beats in sound.

J. Stewart Gibson: New piece of apparatus for showing the relation between intensity of illumination and distance.

W. R. Pyle: (*a*) Dip-needle demonstration. (*b*) Magnetizer for magnets.

E. R. von Nardroff: An apparatus for determining the moment of inertia in gm.-cm.^{2} units.

Chas. Forbes: (*a*) The osmosescope. (*b*) The centrifugal railway.

W. M. Campbell read a paper on the effect of pressure on magnetization of iron. The paper referred briefly to the Kirchoff theory on the effects of stress deduced from the strains due to magnetization, to the experimental work done by Wassmuth, Tomlinson, Nagaoka and Honda and Miss Frisbie, and the contradictory results they obtained. Then followed a description of the apparatus used by the writer, the method of conducting the experiment and the results. Higher pressures were used in magnetizing fields stronger than those used by other investigators. Keeping the pressure constant and changing the field, the results showed an increase in intensity up to about eighteen units of field, then a decrease with a change of sign at about $H = 90$ units, and a continual decrease with increase of field.

J. Stewart Gibson read a paper on the results of a series of experiments on the critical angle; its effect on vision from underneath the surface of water.

At a meeting of the section held on Monday evening, May 20, F. M. Pedersen read a paper on the influence of molecular structure upon the internal friction of the vapors of certain isomeric ethers. The viscosity coefficients of various ether vapors at 100° C. were obtained by the well-known transpiration method. In the apparatus used the capillary was perfectly straight and the driving pressure obtained by a column of mercury descending under gravity. The most interesting substances examined were eight ethers, some of them extremely rare, divided into three groups of isomers. The results show the same fact for these propyl compounds that was observed by Lothar Meyer[1] and Steudel for butyl compounds, viz., the molecules of a tertiary compound are smaller than those of a secondary, which in turn are smaller than those of a primary.

William Campbell read a paper on the iron carbon series of alloys. The various published equilibrium curves of the series, by Roberts-Austen, Rooseboom, Le Chatelier, Benedicks and others, were reviewed. A series of lantern slides showed the various changes of structure which take place (*a*) by variation in composition; (*b*) by annealing at different temperatures. Two systems were shown to occur: I. austenite (mixed crystals) and cementite; II. austenite and graphite. The former is unstable, the latter stable.

WILLIAM CAMPBELL,
Secretary

COLUMBIA UNIVERSITY

[1] *Pogg. Ann.*, 1882, Vol. 16, p. 394.

DISCUSSION AND CORRESPONDENCE

THE CLOCK OF THE U. S. NAVAL OBSERVATORY

TO THE EDITOR OF SCIENCE: While not desiring to appear to enter into any controversy with the author of the article on 'The Clocks of the Greenwich and the U. S. Naval Observatories' which appeared in your issue of May 31, it would seem that certain facts should be stated to clear up the misunderstanding that has occurred.

I think no one will disagree with the statement that the value of an astronomical clock is to be measured by the degree of accuracy with which its correction can be predicted from observed corrections or interpolated between those corrections: If a series of clock rates extending over several months can be shown to follow such a simple law as that given on page 451 of SCIENCE for March 22, 1907, for the Naval Observatory clock, *viz.*:

$$\text{Daily rate} = +0^s.0161 - 0^s.00103\,(T - \text{Mar. }29.0) - 0^s.0456\,(t - 27^\circ.0),$$

and when both these terms have such a probable explanation in physical phenomena, it would be folly to refrain from the use of this formula in investigating the running of the clock. It follows that the mean residual $0^s.015$ is what really indicates the performance of the clock and not $0^s.035$ as deduced by Mr. Lewis.

The statement by Mr. Lewis that in my article "the Greenwich clock rates are spread over a period of one year" is somewhat misleading as they were divided into twelve monthly groups and each group was considered by itself as is clearly shown on page 450 of SCIENCE for March 22, 1907. That would seem to be as fair a method of treating them as the published data would provide.

In conclusion attention may be called to the article in SCIENCE for April 12, 1907, page 570, 'A Riefler Clock and a Self-registering Right Ascension Micrometer,' in which it has seemed to the writer that the Naval Observatory clock runs even better than was indicated by the mean residual $0^s.015$.

W. S. EICHELBERGER

U. S. NAVAL OBSERVATORY

VARIATION IN THE COROLLA OF LINARIA VULGARIS MILL

TO THE EDITOR OF SCIENCE: In examining the *Linaria vulgaris* Mill., with a class in botany I found the following remarkable variations in the corolla which may be of interest to some of your readers. The flowers in which the variations appeared were all on the one specimen.

In the corolla of two of the flowers in which the variations occurred the spur was absent, as was also the usual orange-colored palate. The corolla in both these flowers consisted of five petals, but in one case there were four petals in the upper lip and one in the lower, while in the other all five petals were in the position usually occupied by the upper lip.

The corolla of a third flower was tubular, about three fourths of an inch long, of greatest diameter at the base and tapering to the apex. At the base of this peculiarly formed corolla there were three spurs about one third of the circumference of the corolla apart. The apex of the corolla terminated in a circular crown, which was orange-colored, like the palate in the ordinary flower. At the upper end of the tubular corolla, close to the orange-colored crown, there were three petal-like tips equally distant from one another.

J. B. Turner

THE INDIAN BEDBUG AND THE KALA AZAR DISEASE

It is not generally known by the entomologists of this country that the common bedbug of India is not *Cimex lectularius* Linnæus, but *Cimex rotundatus* Signoret (= *macrocephalus* Fieber). Captain W. S. Patton, of the Indian Medical Service, has recently published important papers on this insect, especially in regard to its pathogenic relations. In a brief note on the distribution of these two house-infesting bedbugs published in the *Indian Medical Gazette,* XLII., February, 1907, he points out the above-mentioned fact, and leads us to form the opinion that enough observations have not been made along that line. *Lectularius* is apparently distributed mainly throughout the North Temperate Zone, while *rotundatus* is tropical or subtropical; and though until very recently known from Burma only, it is now recorded by Dr. Patton as occurring throughout India, Assam, Malay, Aden, Mauritius and Réunion (Patton, *ibid.*) and still more recently (Patton, April 4, 1907, *in litt.*) it is recorded from St. Vincent, Sierra Leone and Porto Rico. I have specimens from Madras Presidency (South India), Réunion, Mauritius and St. Vincent, kindly sent by Dr. Patton.

These facts in regard to the distribution of the Indian bedbug become of economic importance in view of the now definite evidence which Patton presents that the dreaded kala azar disease of India is carried by that insect. This evidence is published as No. 27, new series, *Scientific Memoirs by Officers of the Medical and Sanitary Departments of the Government of India,* Calcutta, 1907, and is entitled 'Preliminary Report on the Development of the Leishman-Donovan Body in the Bedbug.' By the means of extensive experiments with bedbugs, it is fully demonstrated that these bodies, the cause of the disease, are ingested from patients and go through considerable development. In a postscript to this paper, Patton states that all of the intermediate stages of development and fully developed flagellates have since been found in the insect, and he states his belief that 'it is beyond all doubt that this insect transmits the disease.' Owing to conditions, it is impossible for him to test this directly by exposing healthy persons to the attack of infected bedbugs, but as it is, the evidence is complete and all of the facts point to the conclusion reached by Dr. Patton.

The establishment of this relation of the Indian bedbug to the transmission of a much-dreaded disease naturally directs our attention again to the pathogenic relations of our own common household pest, *Cimex lectularius* Linnæus, which is now under investigation by some of the medical profession.

A. Arsène Girault

Washington, D. C.,
May 25, 1907

SPECIAL ARTICLES

THE SOLENODON OF SAN DOMINGO: ITS EXTERNAL CHARACTERS AND HABITS

A specimen of this rare and curious insectivorous mammal (*Solenodon paradoxus*) recently obtained by Mr. A. Hyatt Verrill in San Domingo and preserved in formol, has been submitted to me for study. Owing to the introduction of the mongoose and other causes this creature has become very rare and local. It is, without doubt, on the verge of extinction. At present, it is scarcely known in the great museums of Europe, and no specimen is known to be preserved in any American museum. A single skeleton is said to exist in the museum of Berlin. The only other *Solenodon* (*S. cubanus*), of eastern Cuba, is said to be nearly or quite extinct. It is a smaller and more hairy species, with shorter tail

The San Domingo specimen is about 14 inches long, to base of tail; the tail is 13 inches long, round and scaly, like that of a rat. The long, tapered, flexible snout is naked and pinkish white. The body is mostly covered with long, coarse, brown hair, which becomes finer and light yellowish brown or tawny on the head, shoulders and neck. The hind quarters and thighs are partly naked and covered with rough, wart-like excrescences and irregular coarse wrinkles. The fore legs are strong, with large stout claws, which are less curved than in the Cuban species.

It is nocturnal in its habits, living by day in the deep holes and crevices of the cavernous limestone. It feeds, in the wild state, largely on insects and their larvæ, tearing old logs and stumps in pieces to obtain them. But it will also eat the eggs and young of birds, as well as various fruits, and sometimes it is destructive to young poultry, it is said. In confinement it is almost omnivorous and will eat meat freely. This specimen is a female. It gave birth to three naked young ones soon after its capture, but very soon devoured them. It is said to be very stupid when pursued by dogs.

A. E. VERRILL

A NOTE ON THE HAMMERHEAD SHARK (SPHYRNA ZYGÆNA) AND ITS FOOD

DURING the third week in July, 1906, several large sharks were seen, at high water, in various parts of the harbor of Beaufort, N. C. On the twentieth Captain Ed. Robinson, of the sharpie *Gladys,* harpooned one which was chasing large sting-rays (*Dasyatis say* is the form most common at Beaufort) over some sand flats. The harpoon tore out, but, when the fish came up again, another throw was more successful and the shark, which proved to be a hammerhead, was secured. Practically all those who have recorded the capture of hammerheads have noted that when hooked they made violent efforts to escape. This one, when harpooned, made so little resistance that Captain Robinson in describing its capture expressed considerable disappointment at the tameness of the affair. This capture was made in a narrow channel within two hundred yards of the wharves of the business part of Beaufort. Eighteen hours later I secured the fish, towed it over to the laboratory wharf and swung it up by a block and tackle to a davit, where it was photographed, measured and dissected.

This was the largest shark ever captured in Beaufort Harbor, and it was carefully measured. Thinking that these measurements may be of interest and value, I give the most important. Length all over, 12 ft. 6 in.; length of 'hammer' between eyes, 3 ft.; girth at first gill-slit, 4 ft. 2 in.; girth in front of pectorals, 4 ft. 2 in.; in front of pelvics, 4 ft. 1 in.; at root of caudal, 1 ft. 6 in.; length of right pectoral fin, 2 ft. 1 in.; of dorsal, 2 ft. 6 in.; of right pelvic, 1 ft. 1 in.; of second dorsal, 10 in.; of ventral lobe of caudal, 1 ft. 7 in.; of dorsal lobe of caudal, 3 ft. 6 in. There being no means at hand for weighing the animal, estimates only could be made, but, judging by the number of men required to hoist it with a tackle having three pulleys, it must have weighed at least 800–1,000 pounds.

This shark was a female and was dissected with the hope that embryos might be found in the uteri, but all the generative organs were practically unrecognizable. This was due to the stones thrown down the gullet by boys while it was hanging overboard the sharpie (the head being above water), and to the churning brought about by the movements of the tide and by its being successively hauled up for people to inspect and let go into the water again. The posterior cardinal sinuses were in good condition and were as large as a man's thigh. Their walls were cavernous by virtue of the extraordinary development of the tendinous prolongations of the lining membrane.

The stomach contained, in addition to the stones above mentioned, an almost perfect skeleton of a fair-sized sting-ray together with many cartilaginous fragments plainly having the same origin. However the most interesting thing found in the beast was the great number of sting-ray (*Dasyatis say?*) stings present in the body and mouth. In the process of skinning the fish, several were found in the neck region and in the back. However, in cutting out the jaws for a museum specimen, I found the mouth parts to be a perfect mine of stings. In all fifty were extracted, more than forty of which were imbedded in the flesh adherent to the jaw cartilages. These stings varied in size from perfect specimens four or five inches in length to broken-off tips hardly more than one inch long. These broken-off stings were especially abundant at the angles of the jaws where as many as three or four tips were frequently found in a cube of flesh one inch square and two inches long. One can only conjecture how many could have been found by a careful dissection of the flesh of the mouth and throat. The dissection of the jaws was not gotten at until about forty hours after the capture of the shark and its condition consequently was such as to prevent the minute dissection necessary to extract all the spines in the throat region. The lower jaw cartilages were scarred and ridged from angle to symphysis, evidently by stings received in former combats. Some of the stings were manifestly but newly implanted, for the flesh around them was still red from congested blood, in other cases the redness had all disappeared, while some of the stings were plainly old, being imbedded in cysts. Especially was this latter condition true of those piercing the membrane surrounding the cartilages.

Not knowing that anything has been discovered as to the especial food of the hammerhead shark, I wish to advance the suggestion that this is to a certain degree made up of its not far distant kinsman, the sting-ray. This suggestion is based on three facts. First, that this shark was chasing sting-rays when harpooned, and was so eager in the chase that, when the first 'iron' pulled out, it kept up the chase in the neighborhood of the boat until harpooned a second time. Secondly, on the finding of the skeletal remains of rays in its stomach. The whole skeleton of a long-tailed ray found there seems to prove that the remains were not those of the butterfly ray (*Pteroplatea maclura*), which is abundant at Beaufort, but of *Dasyatis say,* the most common armed form. Thirdly, the very large number of stings found in and around the mouth parts indicates the number of rays which this shark caught and which had been successful in stinging it.

Since writing the above paragraph, my attention has been called by Dr. Theodore Gill to a note by Dr. Edwin Linton in which the latter states that from dissections of fifteen small specimens of another species of the same genus of shark, *Sphyrna tiburo,* at Beaufort, he found that the food consisted chiefly of Crustacea—blue crabs, *Mantis* and other shrimps, and acorn barnacles—of seaweed, and of pieces of fish used for bait; but not of whole fish such as they might be expected to prey upon.[1]

E. W. Gudger

State Normal College,
Greensboro, N. C.,
January 24, 1907

[1] These observations were made while I was acting as a temporary assistant in the Fisheries Laboratory at Beaufort. For permission to publish them here, I wish to thank the Commissioner, Hon. George M. Bowers.

DO OFFSPRING INHERIT EQUALLY FROM EACH PARENT?

The alleged fact that offspring inherit equally from each parent, together with the striking parity, in number, size and form, between the chromosomes of the mature male and female germ cells, notwithstanding the great disproportion in size between the egg and the spermatozoon themselves, is frequently advanced as one of the strongest supports of the chromosome theory of inheritance.

But do we inherit equally from each parent? What seems obvious at first glance becomes very doubtful upon closer inspection. The vast majority of the characters of a given organism, such, for example, as make it an animal and a vertebrate of a given genus and species, are obviously characters which are common to both parents. The only characters which we can measure are the minor ones of individual or specific peculiarity, and while these, apparently, may be derived equally from either parent, this conclusion is very different from one which would affirm that the qualities of the offspring as a whole are derived equally from each parent. The hybridist can help us little here, because his so-called 'unit characters' are only these same superficial individual and specific qualities, and the fact that the crossing of any but comparatively closely related forms is impossible, precludes any final solution along this line.

The facts at our command, indeed, indicate that the female germ cell determines the fundamental animal form together with its more constant characters. Driesch, for example, found that in hybrid echinoderms, no matter from what species the sperm were taken, the manner and rate of cleavage, the character of the mesenchyme formation and of gastrulation always followed the *maternal* type. Standfuss has shown that young butterfly hybrids first resemble the maternal species, but diverge more and more towards the male species with each metamorphosis, as individual qualities begin to assert themselves, until the hybrid comes to its ultimate degree of intermediacy.

If we accept the idea of 'formative substances' as advanced by Conklin or Lillie, whether we regard these substances as really specific organ-forming materials or as indicating corresponding cytoplasmic localizations, then the fundamental organology is already laid down in the egg before the spermatozoon enters, and the egg passes on to cleavage without waiting for an equal amount of 'formative substance' to pass out from the male pronucleus and take joint possession of the already localized areas. For example, Lillie[1] states that at fertilization the presence of the sperm involves no important changes in the topography of the several formative substances which are distributed throughout the cytoplasm of the egg. Since, then, there is no evidence of such a biparental mechanism in early stages, is it not decidedly far fetched to postulate one, especially when the single one that already exists is adequate?

If it is true that offspring do not inherit equally from each, but more from the maternal parent, then the argument for the morphogenetic nature of chromatin based upon the striking parity between the chromosomes of male and female germ cells after reduction loses its force, if we still cling to the idea that the chromosomes are the *exclusive* bearers of hereditary qualities.

Contrary to the view that fertilization is primarily concerned with the mingling of hereditary qualities, many zoologists have come to regard it as satisfying some periodic physiological need of the organism, and while one conception is perhaps as hazy as the other, still the latter opens up an equal possibility that the reduction divisions have to do with maintaining a metabolic equilibrium of some kind, instead of acting as a mechanism for the casting out of heritable morphological units.

But granted that the chromosomes are the bearers of specific morphogenetic substances, even then the facts (1) that there is apparently a persistence of the individuality of the chromosomes through successive cell-generations, and (2) that the final behavior of the chromosomes in reduction division is in seeming accordance with the demand of the Mendelian principles—even these facts do not necessarily restrict us to a purely chromosomal hypothesis of heredity nor to the idea that offspring inherit equally from each parent. However, since this is one of the most significant lines of argument for this hypothesis, it merits more detailed discussion.

Various hybridists, judging from the external visible characters of hybrids, came to the conclusion that in the germ cells of hybrids there must be a separation of parental qualities so that with respect to a given quality, half of the germ cells returned to the maternal, half to the paternal types. Conversely, I had in the meantime, from studying the germ cells of hybrid doves and pigeons, called attention to the fact that there is apparently a segregation of maternal and paternal chromosomes at the reduction period which, if the chromosomes bore hereditary qualities, would lead to the establishment of pure germ cells, and thus afford a possible explanation of how the offspring of hybrids come to show returns to grandparent types.[2]

[1] *Jour. of Exp. Zool.*, III., 2, p. 178.

[2] SCIENCE, February 16, 1900.

However, I had previously noted and recorded[3] this fact of the reversion of the offspring of hybrids to the grandparent types.

Later Montgomery (1901) and Sutton (1902), working on non-hybrid forms, which have chromosomes of varying size, presented striking evidence of the fact that in early germ cells there are pairs of homologous chromosomes, one of paternal and one of maternal origin, and that these homologous chromosomes unite in synapsis. The ensuing reduction division simply brings about their separation and segregation in different cells.

Although I had elaborated the idea of the relation between the chromosomal phenomena of hybrids and the reversion of their progeny, in my doctoral thesis of 1900, the published statement of the details did not come out until November, 1902,[4] but these details show that I had hit upon practically the Mendelian idea. Very soon after the appearance of my paper in 1902, Cannon, Wilson and Sutton published conclusions of a very similar nature, endeavoring to offer a cytological interpretation of the Mendelian principles, which had had their renaissance in the meantime. Only Cannon's conclusions, however, were based upon the actual study of hybrid material. In this way it became established that there is a mechanism in germ cells which, assuming that the chromosomes are concerned in heredity, could possibly lead to a segregation of paternal and maternal qualities.

But if we accept the idea of Boveri and of Sutton that each chromosome contains only a certain pro rata of adult characters, and that reduction is simply the separation of homologous mates, then besides facing the unproven though necessary implication that whole blocks of grandparental characters are barred from the third generation, we must also recognize, as pointed out by Davenport and others, that this theory is not in harmony with certain facts of non-alternative (non-Mendelian) inheritance. How, for example, are we to account for the well-authenticated instances of (1) blended inheritance, (2) of pronounced atavism, such as is seen in mongrelized breeds of pigeons, which tend to return to the primitive ancestral blue-rock type with apparently none of the components missing, (3) of exclusive inheritance or prepotency, (4) of the later outcropping of dominant characters in a recessive individual, which has sprung from a gamete that, according to this chromosomal theory, has been purged of such dominants, or, in other words, the contamination of a given character by its allelomorph, and (5) of the 'fixation' of a hybrid mosaic gamete, of which the qualities do not segregate in subsequent generations?

Even though we have visible evidence of the separation of homologous chromosomes, I fail to see why this necessarily means that each has retained all of its original qualities, or has taken on no new ones. It is easily possible that there can be persistence of form in these chromosomes without persistence of qualities. Supposing that the chromosomes differ from one another in their physical properties (and they doubtless do, more or less, since we can recognize constant types), the form might easily be dependent upon the physical consistency of each of the particular chromosomes, which, in fact, is made up of at least two substances, viz., chromatin and linin. It is evident that in pro-synaptic stages when the chromosomes are characteristically present as hazy granular masses, numerous conjugations or exchanges of individual granules could occur, and yet the matrix (linin) bearing the qualities, persist and appear finally in its characteristic form.

In my own study on the spermatogenesis of hybrids, I was finally led to consider the strict individuality of the chromosome as persisting only in hybrids derived from widely divergent forms. In the case of fertile hybrids my idea was expressed as follows: "The cases in which the entire plasmas are segregated are then probably but magnified images of what occurs among the specific qualities of the milder crosses."[5] Furthermore, I have suggested that the occasional inequalities in the divisions of individual chromosomes might

[3] *Zoological Bulletin*, Vol. II., No. 5, 1899.

[4] 'Hybridism and the Germ-cell,' *University of Cincinnati Bulletin*, No. 21, November, 1902.

[5] *Cincinnati Lancet-Clinic*, May 9, 1903.

account, in part at least, for the fact that all germ cells are not absolutely pure in hybrids, and it may be thus that in cases of apparently complete return to one parent type, characteristics of the other parent may, nevertheless, crop out from time to time in succeeding generations.[6]

In the foregoing attempt to reconcile the behavior of chromosomes with the Mendelian principles it is clearly assumed that the chromosomes are the mechanism, or at least the chief mechanism of heredity. But turn them and juggle them as we may, we see that it is difficult to make the conclusions of the cytologist who clings to an exclusively chromosomal theory fit into the facts of heredity, and the difficulty becomes all the greater if it is true that the male and the female do not contribute equally in heredity. The latter difficulty, together with an attempt to explain reversions, led me to the suggestion, in a former paper, that both cytoplasm and nucleus are involved specifically in inheritance, the cytoplasm of the germ cell representing the more stable and constant form of the animal, and the chromatin the more individual and variable characteristics. If the male contributes chiefly individual qualities, and if we accept these as being borne in the chromosomes, then since these chromosomes are so strikingly paralleled by those of the female in both number and form, we might infer that the chromosomes of the female likewise bear only individual and variable qualities, while the cytoplasm bears the fundamental qualities.

The material starting points (or *inceptors*) of definite characters were conceived of as arising through the activity of chromosomal emanations on cytoplasmic materials at different stages in the development of the organism. Since the chromatin was looked upon as the more variable cell constituent, it, rather than the cytoplasm, was regarded as conditioning the more variable characteristics, without, however, precluding some nuclear activity at all stages of development. There is every reason to believe that both cytoplasm and nucleus in a given species are *distinctive* of that species and there is no conceivable reason why the cytoplasm any more than the nucleus must be made so anew in each generation. The variable nature of chromosomes is evidenced in the pronounced irregularities which are induced in them through hybridizing or drugging. Furthermore, the intimate mingling of the chromatin from the two sexes, the known fact that definite substances (enzymes, etc.) emanate from the nucleus, and the highly unstable nature of nucleo-proteids, would point to the chromosomes as, at least, an adequate source of variability. Lastly, the facts of differentiation, before fertilization, etc., mentioned above, seem to indicate that offspring do not inherit all characters equally from parents but rather only the more superficial ones, and that, therefore, chromosomes, which are derived equally from each parent, are connected in some way with such superficial characters.

Furthermore, such an interpretation would harmonize better with the fact that the numerical variation of chromosomes in closely related groups may be out of all proportion to the character differences of the groups. For example, Miss McGill, working on *Anax*, has found 28 chromosomes, while in some recent studies on a *Libellula* I find 23 or at most not over 24 chromosomes. How, then, on the exclusively chromosomal hypothesis of heredity, are we to account for the discrepancy in the number of chromosomes, such as exists between two members of so restricted a group as the Odonata, which, despite their individual differences, must have the vast majority of their fundamental features in common? Or how account for the pronounced numerical differences in chromosomes as recorded by Wilson in different genera of the Heteroptera. For example, *Anasa tristis* has 21 in the male and 22 in the female, while *Protenor belfragei*, or *Alydus pilosulus*, has 13 in the male and 14 in the female. Even related species of the same genus may differ in the number of chromosomes, and there are indications that this may be true of different individuals of the same species. The numerical differences would

[6] See 'Spermatogenesis of Normal and of Hybrid Pigeons,' p. 48.

seem to be out of all proportion to the actual differences between the adult genera or species.

The facts would seem to force us, if we still cling to the idea that in chromosomes we are to find specific morphogenic substances, to some such postulate as the one already suggested, that only the more superficial and fluctuating qualities are borne in the chromosomes, for then we might appreciate how considerable fluctuation in the number of chromosomes in different species might be reconcilable with the relatively smaller differences between the adults.

However, this idea of the respective rôles of cytoplasm and nucleus in inheritance was offered only as a suggestion, and not as a theory and such it must remain, unless some more convincing evidence is forthcoming. Its only value in this connection is to show that we are not restricted to a consideration of the chromosomes as the sole vehicle of heredity, and that equality in the amount of chromatin contributed by each parent, even should we succeed in assigning heritable qualities to the chromosomes, does not necessarily imply that we inherit equally from each parent.

Michael F. Guyer
University of Cincinnati,
March 12, 1907

THRUSTS AND RECUMBENT FOLDS, A SUGGESTION BEARING ON ALPINE STRUCTURE[1]

Let it be assumed that as a result of a shearing strain a thrust fault, *A*, is initiated in the crystalline basement of a sedimentary series, and that displacement occurs along a plane which rises at a gentle angle to the plane of stratification. Let it be assumed further that the strata comprise a soft shale overlain by a limestone of moderate thickness, above which is a thick series of sediments.

The immediate effect on the shale will be to thicken it in front of the overthrust crystallines (as was repeatedly observed in the case of soft layers under similar conditions of pressure in experiments on folding described in the Thirteenth Annual Report of the U. S. Geological Survey). In swelling the shale

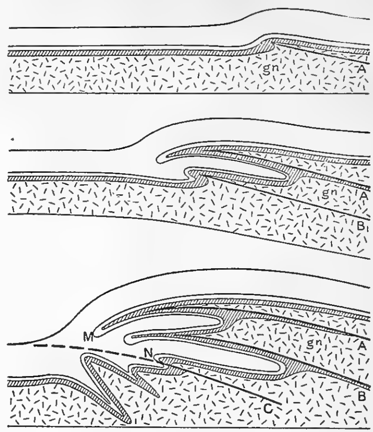

Fig. 1. Hypothetical Development of Recumbent Folds from Overthrust Faults.

will have somewhat the effect of a laccolithic intrusion and will raise the overlying limestone, forming an anticline.

If the plane of the thrust forms an acute angle with that of the bedding, the shear may follow the bed of shale, thus separating the limestone and overlying strata from the underlying; and the stress transmitted by the advancing tongue of gneiss to the limestone may roll the latter back upon itself. The overlying strata would become more or less involved in the overfold.

The recumbent fold will develop to a length determined (1) by the competency of the strata to transmit thrust; (2) by the effect of the resistance, which increases as the strata pile up; and (3) by the conditions favoring the development of a second thrust fault, *B*.

If the movement continues to the production of a second thrust, *B*, the plane of the thrust, *A*, and the recumbent beds will be raised into an anticlinal attitude, as the flat strata were in the first instance. Thus the point of the overturn, *M*, will appear to be bent downward, and if movement continues on the thrust, *A*, it may become bent under. Such movement could not, however, continue

[1] Read before the Geological Society of Washington, April 10, 1907.

far, before the thrust, *B*, would take up all the stress.

There is no obvious reason to limit the development of the schuppenstruktur, *A* and *B*, to two thrusts. A third and possibly a fourth might be produced. It might, however, occur that the accumulated thickness of piled-up strata should become so great, or the conditions of slight resistance in the plane of shear so favorable to displacement, that the whole recumbent mass would move forward upon a great major thrust, *C*, to an indefinite distance.

It is apparent on inspection of the diagrams that the length of the strata involved in the recumbent folds between *M* and *N* bears a relation to the total displacement on the overthrusts. The two are not equal, for the strata in the overturned limbs of the recumbent folds are stretched; but the two lengths may be said to be of the same order of magnitude.

The preceding hypothetical relations of recumbent folds to overthrusts were developed from a study of the extraordinary recumbent folds of Mont Joly as described by Ritter.[2] It followed from measurement of Ritter's sections that the length of strata involved in the recumbent folds was about 130 kilometers, whereas that included in the supposed roots was but 16 kilometers. If the folds and roots bear the relations attributed to them the lengths of the strata when developed should be at least of the same order of magnitude. The further analysis of the problem led to the conclusion that the recumbent folds represent a sequence of thrusts, as described above. The probable total displacement on thrust planes might be as much as 80 kilometers. The present position of the thrust planes would be above the Alps, or in the alpine summits, on the assumption that they have been raised during later elevation of the range. Their extent would carry them over the range to the southern slope.

At first sight this conclusion appears to accord closely with the phenomenal nappes des recouvrement of Lugeon, but there is a distinction. Whereas the nappes are supposed to have been projected over the Alps at their present elevation, the thrusts are believed to have developed in a shear zone beneath the surface, before the Alps were the visible mountains of to-day, and to have risen from a considerable depth toward the surface, as is the habit of thrusts. On independent evidence the date of thrusting is assigned to a pre-Eocene (pre-Flysch) date. The elevation which has raised the fully developed, one might say exhausted and dissected, thrust planes to the height of the summits of the Alps is, on the other hand, assigned to the middle Tertiary epoch of diastrophism, which also caused folding and the more obvious overthrusting.

BAILEY WILLIS
U. S. GEOLOGICAL SURVEY

[2] 'La bordure sud-ouest du Mont Blanc,' Bull. des Serv. de la carte de la France, No. 60, profile I., Plate I.

QUOTATIONS

TEACHERS' SALARIES AND MINNESOTA

THERE has been recently among the alumni in various eastern colleges and universities, notably of Harvard and Princeton, a movement to secure funds to advance the salaries of instructors. The movement has appealed with some success to the loyalty of alumni and the benevolence of interested friends. A similar movement was actively started among the alumni of the University of Minnesota last fall, with the result that the regents of the university now have at their disposal an annual appropriation of $165,000, made by the legislature of the state, available for current expenses. Salaries of instructors have already been materially advanced in some cases to a level which places the University of Minnesota in active competition with the leading universities of the country so far as the compensation of its instructors is concerned. This must be regarded as a very important advance in the development of state universities. Its economic importance as bearing upon the general problem of the compensation of teachers is even greater, for the alumni of Minnesota appealed not only to loyalty and benevolence, but to a legislature responsible for the proper support of the first educational institution in its state. They carried through their appeal with energy and success, in the face of strong opposition and without the heartiest cooperation on the part of the board of regents. The campaign was conducted by a committee of alumni, ably seconded by the *Minnesota Alumni Weekly*. The sum originally desired was $200,000, but the regents voted to ask for only $150,000. Yet the amount granted was $165,000. The issues of the *Minnesota Alumni Weekly* ever since the movement began have contained a quantity of valuable material bearing on the general subject. Statistics were gathered the country over, and the relation of teachers' salaries to incomes from other professions and to the cost of living was set forth. In addition, the *Weekly* published letters from various sources expressing hearty interest in the campaign. From all parts of the state committees of alumni and individuals addressed the members of the legislature on behalf of the movement. As a result, when the legislature came to make up its appropriations for the university fund, it was confronted not with the question whether salaries should be raised, but with the question to what extent they should be raised. We believe that educators generally have grounds for congratulations on this successful movement. Here is a case where the needs of higher education have been put definitely before the people of one of our most important states, and they have responded in a way which indicates a high appreciation of their economic responsibility. Their action is a challenge to emulation.—*Columbia University Quarterly.*

THE AWARD OF THE BOYDEN PREMIUM BY THE FRANKLIN INSTITUTE

IN 1859, Uriah A. Boyden, Esq., in his day an eminent mechanical engineer, of Boston, Mass., deposited with the Franklin Institute the sum of one thousand dollars, to be awarded as a prize to any resident of North America, who should determine by experiment whether all rays of light and other physical rays are or are not transmitted with the same velocity.

The Franklin Institute has religiously advertised the proposition of Mr. Boyden since that time until the present, inviting investigators to compete for the premium. During this period, which covers almost fifty years, a large number of essays, possibly as many as 25 or 30, have been presented by investigators for this award, but after careful investigation by a competent committee, appointed in each case, none was found sufficiently meritorious to warrant the institute in granting the prize, until the recent investigation by Dr. Paul R. Heyl, assistant in the department of chemistry of the Central High School of Philadelphia, which, in accordance with the prescribed conditions, was submitted anonymously. This communication was referred to a special committee, consisting of Messrs. Hugo Bilgram, mechanical engineer; Professor Arthur W. Goodspeed, of the department of physics of the University of Pennsylvania, and Dr. George Flowers Stradling, of the department of physics of the Northeast Manual Training School of Philadelphia, who reported unanimously in favor of awarding the Boyden prize for an essay submitted under the pseudonym 'Algol.' The name of the author was only disclosed after the investigators had upon careful examination proved its merits. An abstract of the committee's report follows, which will indicate the extremely delicate nature of the tests required in the investigation.

The applicant 'Algol' for the Boyden premium has succeeded in demonstrating, by experiment, that those of the ultra-violet rays of light, for which glass is transparent, have the same velocity as the light rays proper.

He reasons that if the velocity of these rays were different, they would not arrive, from a distant source, at the same time. For his test he selected 'algol,' a well-known variable star in the constellation Perseus, as the source of light. By means of a diffraction grating he eliminated all but the ultra-violet rays of a known frequency, and by focussing them on a sensitive plate, obtained photographs of the star.

For the purpose of identifying the rays so recorded with the visible rays, regarding the time of their emission, he selected, for the time of his test, the time during which the light of this star shows the peculiar phenomenon of fading and recovering. The period of this variation is known to be about six hours. During this period he took a number of photographs, one half hour apart, each exposure being twenty minutes, the remaining ten minutes being employed for making the necessary preparations for the next exposure. He thus obtained a number of exposures of the star on the same sensitive plate, but shifted in position. After developing the plate, the successive images plainly showed a fading and recovering, and although the exact location of the minimum brightness could not, in the nature of things, be absolutely determined, the approximate coincidence of the time of the minimum brightness of the visible and the photographed rays was obvious. These tests were repeated a number of times to eliminate the possibility of error and also to take in a certain range of the ultra-violet rays, and since favorable opportunity for making these tests is not frequent, the investigation extended over a period of two years.

The applicant then reasoned as follows: Assuming that the photographic minimum did not exactly coincide with the observed visual minimum, their difference did certainly not exceed an hour, and since the distance of Algol is no less than forty light years, the difference of the velocities of the ultra-violet, and the visual rays could not exceed one part in 250,000. This close approximation establishes equality to all intents and purposes.

At the stated meeting of the Franklin Institute, held on June 19, 1907, the recommendation of the board of managers that the committee's report be approved and the Boyden prize awarded to Dr. Paul R. Heyl was unanimously adopted, and the author will consequently receive this long-delayed and much-coveted award.

SCIENTIFIC NOTES AND NEWS

THE council of the British Association for the Advancement of Science has nominated Mr. Francis Darwin, F.R.S., foreign secretary to the Royal Society, author of important papers on physiological botany and of the 'Life and Letters of Charles Darwin,' to be president of the meeting next year, when, for the fourth time, the association will assemble in Dublin.

ON the occasion of the celebration of the bicentenary of the birth of Linnæus, the Linnean gold medal of the Royal Swedish Academy was awarded to Sir Joseph Hooker.

AT the annual meeting of the American Academy of Arts and Sciences, on May 8, 1907, it was voted to award the Rumford premium to Mr. Edward Goodrich Acheson for the application of heat in the electric furnace to the industrial production of carborundum, graphite and other new and useful substances.

M. H. LE CHATELIER has been elected a member of the section of chemistry of the Paris Academy of Sciences in the room of the late M. Moissan.

PROFESSOR THEODORE W. RICHARDS, the present holder of the Harvard professorship in the University of Berlin, gave an address upon 'Neuere Untersuchungen über Atomgewichte,' to the German Chemical Society in the Hofmann Haus in Berlin, on the evening of June 1. At the address and at the dinner following there were present among many others: Professors Emil Fischer, Landolt, Nernst, Warburg, Planck, Ladenburg, Graebe, Liebermann, Gabriel, Le Blanc, and Brauner.

A PORTRAIT of President Eliot by Mr. John P. Sargent has been unveiled in the Harvard Union in connection with the commencement exercises of the university. The portrait was a gift to President Eliot on his seventieth birthday, chiefly by the alumni of the class of 1904.

PROFESSOR A. E. VERRILL, who has held the chair of zoology and the curatorship of the zoological department of the Peabody Museum at Yale University since 1864, will retire from active service at the close of the present year.

PROFESSOR GEORGE C. COMSTOCK, director of the Washburn Observatory, University of Wisconsin, was honored with the degree of doctor of laws by the University of Illinois at its commencement on June 12. A week later the University of Michigan conferred upon him the honorary degree of doctor of science.

AMONG the recipients of the honorary degree of doctor of laws, at the commencement commemorating the fiftieth anniversary of the foundation of Washington University, was Professor William Trelease, who has held the chair of botany in the institution since 1885.

THE Western University of Pennsylvania has conferred its doctorate of laws on Daniel Webster Hering, professor of physics in New York University, and its doctorate of science on Arthur Arton Hamerschlag, director of the Carnegie Technical Schools at Pittsburgh.

MR. JOHN FRITZ, pioneer in the iron and steel industry in the United States, has received the degree of doctor of engineering at the Stevens Institute of Technology.

ON the occasion of the sixty-third annual commencement of the University of Michigan the honorary degree of doctor of laws was conferred upon the Count de Montessus de Ballore, the distinguished French seismologist.

ON the occasion of the celebration of the seventy-fifth anniversary of the foundation of Lafayette College, the addresses were made by Professor J. McKeen Cattell, of Columbia University, Professor W. B. Owen, of the college, and Professor Hugo Münsterberg, of Harvard University. The degree of doctor of letters was conferred on Professor Münsterberg, and the degree of doctor of laws on Professor Cattell. The degree of doctor of laws was also conferred on Dr. E. W. Morley, formerly professor of chemistry at Western Reserve University, and the degree of doctor of science on Dr. Frederick Starr, associate professor of anthropology in the University of Chicago.

BEGINNING on Monday, June 24, Professor Louis Kahlenberg will deliver a course of fifteen lectures on modern chemistry at the University of Washington, Seattle. In addition he will also give two lectures of a popular nature on the subjects, 'The Human Side of Some Great Chemists' and 'The Pure Food Movement.'

DR. FRIDTJOF NANSEN, the president for the current year of the Social and Political Education League, delivered his presidential address, in the botanical theater of University College, Gower Street, on June 26, his subject being 'Science and Ethical Ideas.' Sir Oliver Lodge presided.

IN accordance with the standing agreement between Harvard University and the Cultusministerium of the German government the university has designated as visiting professor at Cambridge, for the year 1907–8, Paul Clemen, Ph.D., professor of the history of art at the University of Bonn, and provincial conservator of the Rhine Province.

CHANCELOR E. B. ANDREWS, of the University of Nebraska, has been given a leave of absence for four months, during which he will travel in Europe. Dean Charles E. Bessey will again be the acting chancelor.

DR. L. O. HOWARD, chief of the Bureau of Entomology of the Department of Agriculture and permanent secretary of the American Association for the Advancement of Science, who has been in Europe to secure parasites to prey on the gypsy and brown-tail moths, has sailed from Liverpool to New York.

PROFESSOR CHARLES SCHUCHERT, of Yale University, curator of the geological collection and head of Peabody Museum, will spend the summer, with two assistants, in an extensive geological excursion. The party will start early in July and will examine, in order, the coast formations and marl beds of New Jersey, the Appalachian Mountain formations in Western Maryland, above Harper's Ferry, and the fossil formations of the Devonian and Silurian Age in Western Tennessee. The last part of the summer will be spent in the Arbuckle Mountains of Oklahoma, tracing the sequence of the geological formations.

DR. ERNST A. BESSEY, pathologist in the U. S. Department of Agriculture, in charge of the Subtropical Laboratory at Miami Florida, has been making an extended tour of inspection through the gulf states, and across Texas, New Mexico and Arizona to California, Nevada, Utah, Colorado, Kansas and Nebraska, for the purpose of studying certain plant diseases in the field. He returned to Florida in the latter part of June.

F. C. LINCOLN, lately fellow in geology at Columbia University, has gone to Alaska for the summer in his professional capacity of mining engineer.

MR. CHARLES LOUIS POLLARD, formerly assistant curator of the division of plants in the United States National Museum, and more recently botanical editor for the G. and C. Merriam Company, of Springfield, Mass., has been appointed curator of the Staten Island Association of Arts and Sciences. Pending the removal of the museum of the latter to the quarters assigned to it in the new Richmond borough building the temporary office of the curator is in Room 18 of the Staten Island Academy at New Brighton. Mr. Pollard recently returned from a lecture tour in New England and Canada in aid of the cause of plant protection, the trip being made under the auspices of a grant from the Stokes fund of the New York Botanical Garden.

HENRY G. HANKS, at one time state geologist of California, and author of contributions to geology and chemistry, died at Alameda, Cal., on June 19, aged eighty-one years.

DR. ALEXANDER STEWART HERSCHEL, F.R.S., honorary professor of physics at the Durham College of Science, died on June 18. Professor Herschel died at the Observatory House, Slough, Buckinghamshire, where his father and grandfather made their great discoveries.

DR. L. FISCHER, honorary professor of botany at Bern, has died at the age of seventy-nine years.

THERE will be a civil service examination on July 10 for the position of assistant in soil bacteriology in the Bureau of Plant Industry at a salary of $1,400, and for the position of laboratory aid in economic botany at a salary of $600.

THE Japanese government announces an appropriation of $5,000,000 for the international exposition to be held at Tokyo in 1912. The money is to be paid in installments from 1908 to 1914.

THE next Esperanto Congress will be held at Cambridge, where the delegates will be the guests of the university.

IT is reported from Ottawa, Canada, that the Georgian Bay Canal Commission has practically completed, at a cost of some $600,000, a thorough survey of the proposed 21-foot waterway from Georgian Bay to Montreal *via* the French River, Nipissing, and the Ottawa River. The engineers of the commission have not compiled a final estimate as to the whole cost of the canal, but from information now available it is stated that the total expenditure required for a continuous and easily navigable waterway, with a minimum depth of 21 feet from Georgian Bay to tide water, will be close to $105,000,000. It is said that the canal will shorten the distance from Fort William, on Georgian Bay, to Montreal by over 400 miles. It is estimated that with the completion of the canal there will be 500,000 horsepower available along its course—almost as much as is available at Niagara. With so much cheap power available and with its great resources of iron ore and timber the Ottawa Valley is expected to become one of the greatest manufacturing centers of the continent. Referring to the early construction of the canal, Sir Wilfrid Laurier recently said that if he had the money to do so he would begin work immediately.

UNIVERSITY AND EDUCATIONAL NEWS

WE noted last week that the legislature had increased the appropriation of the University of Michigan from one fourth to three eighths of a mill. It may be further added that this will give the university the sum of $650,000 a year. The additional increase, together with the readjustment of values, which was accomplished last year, added about $250,000 to the annual income of Michigan.

AT the annual alumni dinner of Vanderbilt University on June 17 Chancelor Kirkland announced a gift of $100,000 from Mr. W. K.

Vanderbilt, of New York, grandson of the founder of the university.

Notice has been received from the administrator of the estate of Eliza O. Ropes, late of Salem, Mass., that her will provides for a gift to Harvard University of about seven hundred and thirty shares of common stock of the Boston and Maine Railroad for the establishment of the Nathaniel Ropes professorship of political economy, any annual income not needed for the salary of the incumbent to be given to the Peabody Museum of American Archeology and Ethnology. The will also gives about ten thousand dollars for the establishment of the Nathaniel Ropes Jr. scholarship fund.

By the will of Miss Catherine L. R. Catlin, New York University has received $10,000 for the establishment of a scholarship.

The summer course in the forestry school of Yale University, which constitutes the first term of the junior year, will open its session of seven weeks on Friday, July 5, at Milford, Pike County, Pa. Through the generosity of James W. Pinchot, six buildings, including an experiment station with tracts of land amounting to 200 acres have been provided for the use of the students in this preliminary summer work.

The daily papers state that President Benjamin Ide Wheeler, of the University of California, formerly professor of Greek at Cornell University, has declined the offer of the executive committee to nominate him for the presidency of the Massachusetts Institute of Technology. It is said that President Wheeler's salary at the University of California is $10,000, and that he would have received $15,000 at the institute. It is further reported that Dr. Henry S. Pritchett, president of the Carnegie Foundation, who has retained the presidency of the institute until a president should be found, will retire on July 1, and that Dr. William A. Noyes, professor of chemistry in the institute, will be made acting-president.

After thirty-nine years of continuous service Professor William Willard Daniells, of the chemical department of the University of Wisconsin, has been made professor emeritus. Professor Louis Kahlenberg, hitherto professor of physical chemistry, has been made professor of chemistry and head of the chemical department. He will hereafter deliver the basal lectures in general chemistry and will also lecture on physical chemistry and personally supervise chemical researches, especially in physical chemistry.

At Western Reserve University, Dr. Olin Freeman Tower has been promoted to be professor of chemistry and head of the chemical laboratory in Adelbert College, and Dr. Harry W. Springsteen has been appointed instructor in physics. In the Woman's College, Dr. Hippolyte Gruener has been promoted to a professorship of chemistry.

Ralph S. Lillie, Ph.D., Johnston scholar in physiology in the Johns Hopkins University and instructor in general and comparative physiology in the Marine Biological Laboratory, has been appointed instructor in physiological zoology in the University of Pennsylvania.

At a recent meeting of the board of regents of the State University of Iowa, Assistant Professor Byron J. Lambert was appointed professor of structural engineering; new instructorships in civil engineering and in descriptive geometry and drawing were created; and an extension of the engineering building more than doubling its present capacity was ordered.

At the June meeting of the regents of the University of Nebraska Dr. Elda R. Walker was advanced from an instructorship, to the adjunct professorship of botany. At the same meeting Raymond J. Pool, A.B., was elected to an instructorship in botany.

J. C. McLennan, B.A., Ph.D., for some years director of the Physical Laboratory, has been elected to the professorship of physics in the University of Toronto, in succession to ex-President James Loudon, LL.D.

Mr. George A. Carse, M.A., B.Sc., has been appointed university lecturer and assistant in natural philosophy in Edinburgh University, in room of Dr. William Peddie.